The Ludwik Kowalski "Learn Cold Fusion" Website Archive

Dr. Ludwik Kowalski contributed to the cold fusion field in several ways. He created a website that is both a storehouse of information and a complete report on his work in the field. He conducted experiments and collaborated with other researchers. He understood and commented on the sociology of science issues that arose in the rejection of cold fusion.

Dr. Kowalski passed away in October 2021. More information can be found in the memorial obituary "Ludwik Kowalski, Major Contributor to the Cold Fusion," which appears in *Infinite Energy* Issue 159.

Dr. Kowalski's cold fusion contributions and accomplishments are recorded in his "Learn Cold Fusion" (LCF) website. His motivation in creating the website is described on an introductory webpage as follows:

> **About my "learn cold fusion" project**
>
> In the fall of 2002, to my surprise, I discovered that the field of cold fusion is still active. This happened at the International Conference on Emerging Nuclear Systems (ICENES2002 in Albuquerque, New Mexico). Several papers presented at this conference were devoted to cold fusion topics. Intrigued by the discovery I started reading about recent cold fusion findings and sharing what I learned with other physics teachers. I have been doing this over the Internet using Montclair State University website:
>
> https://msuweb.montclair.edu/~kowalskil/cf/
>
> What follows is a set of items posted, more or less regularly, on that website since October of 2002. The items reflect my own process of learning, mostly from articles published by cold fusion researchers. I am still not convinced that excess heat, discovered by Fleischmann and Pons, is real or that nuclear transmutations can occur at ordinary temperatures. But I do think that time is right for the second evaluation of the entire field. I do not believe that extraordinary findings of hundreds of researchers are products of their imagination or fraud. Our scientific establishment should treat cold fusion in the same way in which any other area is treated. Those who study cold fusion do not appear to be pseudo-scientists or con artists. The items on my list are arranged in the order in which they were posted on my website.

The version hosted at Montclair State University, where Dr. Kowalski was emeritus professor, is no longer up to date. The website archive has therefore been included on the Infinite Energy website for completeness and for reader convenience.

The broad coverage of the website demonstrates Dr. Kowalski's in-depth knowledge of the issues and activities of the entire field. It is impossible to characterize the LCF contents briefly, but typical topics are descriptions of experimental approaches, theoretical explanations, reviews of the work of others, discourse with other researchers, attendance at ICCFs, and cold fusion as science rather than pseudoscience.

In general, the articles stand as independent essays. They have references among them and often have subsequent inserts for annotation, such as more insight or further developments. The LCF website consists of a remarkable 416 webpages and comprises over 1800 pages.

Thomas Grimshaw
LENRGY, LLC
December 20, 2021

This website contains other cold fusion items.
Click to see the list of links

Links to "cold fusion" items.

Ludwik Kowalski

My motivation? Click to see a short introduction.

Click here to go to the bottom of this long list

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0) I am no loger saying "it is woodoo sciece." click
1) Introducing Cold Fusion to students. click
2) A typical "cold-fusion" setup. click
3) Three kinds of Cold fusion. click
4) Short biographies of three Cold Fusion Scientists. click
5) Aberration of the scientific methology. click
6) On dangers of "second hand" publishing. click
7) On Pathological Science (N-rays story). click
8) On Burden of Proof in Science. click
9) Scientific Method in Cold Fusion. click
10) A Russian connection. click
11) Bottom Line. click
12) What do physics teachers think about CF? click
13) More about the Russian Connection. click
14) What is pseudo-scientific in this? click
15) Or what is pseudo-scientific in this? click
16) Here is an example of real pseudo-science. click
17) An Italian connection. click
18) Nobel Prize for "cold fusion?" click
19) A French connection. click
20) Excommunication of heretics? click
21) If it were up to me I would do it. click
22) Another good article summarized. click
23) A Japanese connecion. click
24) Three short introductionary tutorials. click
25) A technical tutorial. click
26) Comments on the 1989 ERAB report. click
27) Conspiracy? For what purpose? click
28) Summary of a very impressive paper. click or
29) Another French connection. click
30) New APS ethics guidelines and the CF issue. click
31) Excess heat for a student lab? Yes, why not. click
32) Pathological science or important observations to share? click
33) How would Richard Feynman react to CF? click
34) My own proposal. click
35) On methodology and on difficulties. click
36) Ethical issues as seen by an active CF researcher. click
37) On coulomb barrier lowering. click
38) Producing radioactive tritium. click

39) Changing isotopic composition. click
40) My cold fusion lecture plan. click
41) Comments from a friend. click
42) More comments.; to publish or not to publish? click
43) One year after the announcment: click
44) Before going to Salt Lake City: click
45) After returning from Salt Lake City: click
46) Charlatans versus scientists: click
47) Catalytic fusion: click
48) Charge Clusters ? click
49) Not accepted by The Physics Teacher: click
50) From the last APS meeting: click
51) US Navy supported cold fusion: click
52) Alchemy in cold fusion: click
53) Another way; role of surface structure: click
54) Criticizing cold fusion: click
55) The smoking gun?: click
56) Technological Con Artistry?: click
57) And what about hydrinos?: click
58) From a debate on another list: click
59) A piece to publish in a newsletter: click
60) Nuclear Alchemy, 1996: click
61) What are the causes of this conflict?: click
62) Cold Fusion was compared with creationism: click
63) Jed's interesting general observations: click
64) Stalin's pseudo-science: click
65) Pseudo-science in Russia today: click
66) Cybernetics as pseudo-science: click
67) Observations made at Texas A&M University: click
68) Two meanings of "impossible:" click
69) Conspiracy to deceive? I do not think so:" click
70) Please help us click
71) A Nobel Laureate about voodoo science click
72) Anecdotal Evidence? click
73) A confirmation of a reproducible excess heat experiment click
74) E. Mallove describes reproducible excess heat experiments click
75) Do not mix science with fiction click
76) Secrecy in cold fusion research click
77) Another evidence of nuclear reactions in "cold fusion" click
78) An older fight for acceptance; the story of Arrhenius click
79) Early beta decay studies compared with cold fusion click
80) Secular theology? click
81) Where are theories of cold fusion? click
82) Speculations of a retired physicist (This unit is being revised by the author) click
83) Disassociate cold fusion from antigravity, hydrinos, etc. click
84) A cold fusion opinion statement of a physics teacher click
85) From a book of a cold fusion researcher in Japan. click
86) Pseudoscience in Russia. click
87) Fighting a straw man. click
88) Rejections of cold fusion papers by editors click
89) Hydrinos again click
90) My talk at the 10th International Cold Fusion Conference click
91) My poster at that conference click
92) Agenda for the preconference cold fusion workshop click

93) Back to stories from Kruglyakov's book click
94) Browsing the Internet click
95) Catalysts in cold fusion? click
96) No gamma rays were found in our experiment click
97) My published letter to the editor of The Physics Teacher click
98) Students demonstrating excess heat from cold fusion click
99) Speeding up radioactive decay? click
100) Documenting a rejection by Physics Today click
101) How excess heat was measured. click
102) A paper by the retired physicist from unit #82. (This unit is being revised by the author) click
103) Students trying to demonstrate excess heat. Is it nuclear? click
104) New alchemy? Yes, indeed. click
105) More about new alchemy experiments. click
106) Why is Norman Ramsey silent today? click
107) Biological alchemy ? click
108) Another experiment for your students ? click
109) A video cassette "Fire from Water" for your students click
110) They need a real leader click
111) Photos of Fleischmann and Jones, August 2003 click
112) The dilemma of a physics teacher. click
113) Unexplained neutrons and protons; recent papers of Steven Jones. click
114) Voices from teachers and students (?) click
115) They need your suppost click
116) A negative evaluation of cold fusion claims click
117) Exposing false claims click
118) New error analysis versus old? click
119) Errors in unison click
120) A Chinese connection click
121) Just Withering from Scientific Neglect click
122) Laser-like X-rays in "cold fusion?" click
123) An important Japanese connection (Iwamura) click
124) How can one doubt that charged particles are real (WAITNING FOR PERMISSION TO SHARE) click
125) An article I want to publish click
126) Reactions or contamination, that is the question click
127) "Water remembers?" This is pseudoscientific click
128) Screening in condensed matter or something else? click
129) Quixotic Fiasco? click
130) Sonofusion becomes acceptable click
131) Cold Fusion History described by Steven Jones click
132) Cold Fusion History described by Martin Fleischmann click
133) Cold Fusion name was dropped click
134) Second evaluation by the DOE decided. How certain is this? click
135) Seek not the golden egg, but the goose click
136) What is cold fusion? click
137) An inventor or a con artist? click
138) Recent Internet messages. click
139) If I were in charge. click
140) Kasagi's papers. click
141) A paper from Dubna, Russia. click
142) In memory of Eugene Mallove. click
143) Questions about science and society. click
144) Catalytic nuclear reactions. click
145) Role of the non-equilibrium. click
146) Scientific or not scientific? click

147) Extract from an old good summary (E. Storms, 2000). click
148) On difficulties communicating. click
149) A message from a young person. click
150) Answers to some of my questions formulated in unit #148. click
151) Richard's simulated debate about excess heat errors. click
152) My review article on current cold fusion claims. click
153) TOO LONG (History of rejections of my review article.) click
154) SHORTER (History of rejections of my review article.) click
155) Storms' tutorial on diffcult cases in calorimetry. click
156) Unexpected charged particls were observed again. click
157) Detecting cold fusion charge particles with CR-39: Comments and questions. click
158) An extract from an interesting MIT article. click
159) Categorization of cold fusion topics. click
160) Radon background or not? (WAITNING FOR PERMISSION TO SHARE) click
161) Josephson's lecture and other comments on cold fusion (mostly from teachers). click
162) An example of a cold fusion claim that makes no sense to me. click
163) Absence of 100% reproducibility: What does it mean? click
164) A case of mutual deception? click
165) A short comment on names and definitions. click
166) Non-scientists in cold fusion? click
167) An unnecessary "open letter?" I think so. click
168) Nucleosythesis in a lab? A Ukrainian connection. click
169) An interesting effect was discovered in Texas click
170) A Swedish connection that became something else. click
171) A lively and informative discussion? I hope so. click
172) Cold fusion being presented to students. click
173) Wikopedia: Philosophical points of view. click
174) What was the origin of excess power? An experiment worth replicating. click
175) According to Mizuno et al. excess power can not possibly be chemical. click
176) Swift nuclear particles from an electrolyte? Check it in a lab. click
177) List of eleven international cold fusion conferences. click
178) Sharing recent messages and comments click
179) A student project. Work in progress. NOT YET POSTED click
180) Please help to preserve cold fusion history. click
181) A new cold fusion book. click
182) Seeing a huge number of cold fusion tracks with my own eyes. click
183) Pictures and numbers. (continuation from the unit #182). click
184) Contamination or very long "life after death?" (continuation from the unit #183). click
185) CR-39 detectors of charged nuclear particles. click
186) Too good to be true? Turning radioactive isotopes into stable isotopes. click
187) Magnetic monopoles in cold fusion, and other claims. click
188) A chemically triggered nuclear process? What else can it be? click
189) About my four attempts to observe a nuclear "cold fusion" effect. click
190) A better generic name for "cold fusion?" click
191) Trying to describe my understanding of Fisher's polyneutrons. click
192) Trying to replicate Oriani's observations in my own cell. An electronic logbook. click
193) Links to another website. click
194) Comments about theories. click
195) A pdf file to share. Click to see my introduction. Then download, if you want. click
196) Open letter to the DOE scientists who investigated recent CANA claims. click
197) My second Oriani effects experiment (the first is described in the unit #192). click
198) Work in progress
199) Nonsense, fraud or very advanced science? click
200) Teachers discussing scientifc methods click

201) Cooperating with a high school student performing excess heat experiments. click
202) Fraudulent claims of a German anthropologist. click
203) On ending the controversy. click
204) An Israeli connection. click
205) A troubling episode. What can be done to prevent such things? click
206) A new Russian report on nuclear alchemy. click
207) Controversial cases in science (from New Scientist). click
208) Haiko's conversation with Martin Fleischmann click
209) An Australian connection. click
210) Making progress toward 100% reproducibility? click
211) Charles Beaudette writes about the DOE report. click
212) Answering four questions. click
213) About the company Energetics Technologies in Israel. click
214) The power of delusion or healthy optimism? click
215) Solar Electricity click
216) Too good to be true click
217) Ukrainian connection again click
218) To do or not to do it? click
219) A workshop at Stevens Institute of Technology. click
220) Upcoming CF workshops and conferences. click
221) Work in progress (Mitch) click
222) The majority of nature's treasures are still hidden. click
223) A spectacular excess heat report from Russia. click
224) A cold fusion colloquium at MIT. click
225) A student essay (WORK IN PROGRESS) click
226) Another attempt to commercialize? click
227) A new version of Fisher's polyneutron theory. click
228) Cars running on water? An old US patent. click
229) A Russian patent of Gnedenko et al. click
230) Translations of two Russian papers. click
231) Gold from carrots. click
232) Free energy and its impact. click
233) More on free energy. click
234) Comments on Ellis' article about laws of complexity. click
235) One year later. click
236) Promises promises. click
237) An MIT professor writes a report on an iESiUSA device shown to him. click
238) What is cold fusion? click
239) Identity theft? Cold fusion claims should be justified scientifically. click
240) Generation of helium in cold fusion. click
241) Questions concerning the protocol described in unit #240 click
242) Now I must deal with two slightly different protocols. click
243) Will sixty letters to the editor be published by Physics Today? click
244) Coulomb barrier depends on the range of nuclear forces. click
245) Avoiding a global disaster. click
246) Manipulating half-lives of radioactive nuclei ? click
247) Can magnetic forces (resulting from rotation) help deuterons to overcome coulomb barriers? click
248) A proposed set of better names for known nuclear anomalies. click
249) Trying to understand a theory explaing Condense Matter Nuclear Science (CMNS) data. click
250) Stanislaw Szpak et al. -- another case of nuclear alchemy. click
251) Fracto-fusion, crack-fusion, Casimir-fusion, van der Waals fusion, hammer-fusion. click
252) An invitation to perform a simple excess heat experiment. click
253) History of Mizuno-type experiments (such as that described in unit #252). click
254) Comments of a theoretical paper of Windom and Larsen. click

255) Progress report and comments. click
256) A possible source of error in some excess heat reports click
257) A difficult to accept statistical protocol of Bass and McKubre click
258) Can systematc errors result from sampling of irrecular waveforms? click
259) The excess heat can be apparent in our next week experiment. click
260) Is that kind of excess heat real or apparent? click
261) How much excess heat ? click
262) Common hydrogen (H2O) verus heavy hydrogen (D2O). click
263) Fraudulent schemes are probably as old as civilization. click
264) Measuring electric energy. click
265) Another Italian connection. click
266) Scared, reassured and scared again. click
267) Excess heat not confirmed in our Texas experiment. click
268) With an apology to Dr. Dean Sinclair click
269) Analytical methods used in CMNS (condense matter nuclear science) research. click
270) Colorado experiments also fail to confirm excess heat. click
271) Another Colorado experiment. click
272) No excess heat from Mizuno-type experiments. click
273) Mircobial Transmutations at ICCF12 click
274) Scientific Fraud ? An article in Washington Post and comments it generated. click
275) Kasagi and excess fusion cross sections at low energies. click
276) Low counts statistics (not finished?) click
277) An outburst of messages. click
278) New tabletop fusion devices: is it hot fusion or not? click
279) Fraudulant financial manipulations ? click
280) No courtesy of replying from Yale Scientific. click
281) All reliable results should be reported. Hiding negative results is not scientific. click
282) Velikovsky's speculations. click
283) Trying to be a moderator at the ISCMNS meeting. click
284) Hydrinos versus CMNS click
285) Our private correspondence before the Colorado-2 experiment. click
286) An exciting Colorado2 experiment and comments over the Internet. click
287) Social aspects of our controvery that started 17 years ago. Work in progress click
288) Voices from a restricted list for CMNS researchers. click
289) Another Russian connection? click
290) Unexpected comments from some subscribers of the restricted CMNS discussion list. click
291) Yes, these experiments are dangerous, but . . . click
292) Why is this kind of discrimination legal? click
293) Pathological science? click
294) A historical overview of cold fusion. click
295) Chiropractic also had to fight for recognition. click
296) About the origin of Mizuno-type excess heat. click
297) Too much sociology? click
298) Nuclear alchemy in CMNS. click
299) Randy Mills and his new chemistry. click
300) Preliminary Colorado2 results. click
301) Colorado2 results are now much less certain. click
302) Alarming numbers and comments. click
303) Well known reactions or something else? click
304) Researchers discussing excess energy. click
305) Science versus protoscience. click
306) How to restrict a Google search to one server? click
307) Archive of private correspondence about Mizuno-type experiments click
308) Steven Jones plus an expecting new book about CMNS click

309) Researchers speculate about NAE (nuclear active environment) click
310) Alchemy versus CMNS; waiting for the proverbial "proof in the pudding." click
311) Reifenschweiler Effect (introducing an expected essay) click
312) My old speculation about another kind of beta decay click
313) Are oil companies responsible for conspiring against CMNS? click
314) Will this be the first simple and truly reprodicible-on-demand demo? click
315) A new phenomenon or a wrong interpretation of experimental data? click
316) A new paradigm at the next stage! Why not? click
317) About CR39 and other things click
318) Theories, metatheories and philosophy click
319) Our Phase 1 of The Galileo Project experiment click
320) Our first steps in Phase 2 of The Galileo Project click
321) My rejected publication + references click
322) Rutherford-Bohr model being questioned. click
323) This publication was not rejected; it was withdrawn. click
324) Additional validation of our claim (made in unit #319). click
325) More about SPAWAR results. click
326) Online logbook of an experiment (continuation of unit #320) click
327) Online logbook of the next PACA experiment (continuation of unit #326) click
328) Srategy and scientific methodology: Recent comments and observations. click
329) Continuation of after item 327; the online logbook. Experiment #5. click
330) Trusting authotities in science click
331) An illustration of propagation of errors via calibration. click
332) Sonofusion is also struggling for recognition. click
333) Oriani's paper that was rejected by Phys Rev C without sending it referees. click
334) For an item devoted to an ongoing Canada project (to be shown to me). still waiting
335) A draft of my Catania 2007 workshop paper. click
336) Catania 2007 paper as submitted, after the workshop. click
337) Catania 2007 paper on nuclear radiation inside a glow discharge cell. click
338) Voices from an interesting discussion about theories. click
339) Three body orbiting: macroscopic and submicroscopic. click
340) Speeding up radioactive decay: why is it not used to destroy radioactive waste? click
341) Bazhutov's search for erzions and enions click
342) Two speculative messages from theoretically-oriented people click
343) Work in progress click
344) A new book about cold fusion (plus 4 recent messages from the CMNS list). click
345) My own comments on the new book about cold fusion. click
346) Calibration of CR-39 and other useful data. click
347) About a new cold fusion paper published in a European mainstream physics journal. click
348) Replying to a student interested in cold fusion click
349) Modeling CR-39 tracks click
350) What is it, unexplained alpha particles or something else? click
351) High voltage electrolysis experiments (updates) click
352) Ludwik's paper for the next Cold Fusion conference (Washington DC, August, 2008) click
353) After the Cold Fusion conference (notes and reflections) click
354) Excess-heat cell of John Dash. click
355) Neutrons ? click
356) 20th anniversary is approaching click
357) Discussing SPAWAR interpretation in a mainstream refereed journal. click
358) Summary of Ludwik's CMNS projects click
359) SPAWAR high energy neutrons (plus other things) click
360) CBS broadcased a unit about cold fusion click
361) SPAWAR triple tracks click
362) Curie Project click

363) About Alchemy and CMNS click
364) Do polyneutrons explain CMNS? click
365) My shot-in-a-dark experiment click
366) Cold Nuclear Fusion: Does it exist? A recent review by a Russian scientist click
367) Discussing theories click
368) The Curie Project (a difficult start) click
369) History of my CR-39 cooperation with Oriani click
370) Spawar new results and new interpretation click
371) Physics Teachers discuss our energy options (not a cold fusion item) click
372) Technical information about CR-39, mylar, etc. click
373) The Curie Project (update) click
374) New Scientist thread (NOT READY discussing cold fusion) click
375) Results from The Curie Project (NOT READY. To be shown after results are published) click
376) Arata-type experiments click
377) Scientific method click
378) Destruction of radioactivity by cavitation or a false alarm? click
379) My paper (comments about SPAWAR results) was rejected by a mainstream journal. click
380) Destruction of radioactivity by cavitation or a false alarm? click
381) Free proceeding from the 4th cold fusion conference (ICCF4) click
382) Four most important cognitive terms to discuss scientific validations. click
383) Other sets of CR-39 results click
384) Loose ends: The debate is going on. click
385) More speculations. click
386) Integrity or hypocrisy (on the Physics Today web site)? click
387) Voices from the private discussion list for researchers.? click
388) A patent for a spectacular energy amplifier click
389) This article might be a joke. click
390) Another set of spectacular claims. But the two papers are poorly written. click
391) Topic to be assigned click
392) A potentiall damaging episode click
393) Rejections of CF manuscripts click
394) Draft of the Montreal article click
395) A new SPAWAR paper (emission of high enegy neutrons). click
396) What is new in March 2012 ? click
397) Ludwik's first Progress in Physics article (about Rossi) to download. click
398) Ludwik's second Progress in Physics article (Social aspects of CF) to download. click
399) Spectacular claims of Andrea Rossi click
400) Why no follow-up investigations? click
401) Curie Project and SPAWAR project (July 2011). click
402) Bacterial transmutations (to download). click
403) Ludwik's 10 Years With Cold Fusion: A Memoir. click
404) Cold fusion is not the same as hot fusion. click
405) AmoTerra Process Destroying Radioactive Waste Again (see Unit 186). click
406) History of the biological alchemy controversy. click
407) Our Curie Project did not confirm this CF claim. click
408) Rossi's claims conflict with traditional nuclear physics. click
409) Social aspects of the cold fusion controversy. click
410) Cold Fusion Energy Levels. click
411) Interesting Fall 2012 messages (production of He4). click
412) NAE again; Storm's summary click
413) Philosophical and Social Aspects click
414) Sample of interesting posts click
415) Another Cold Fusion conference is approaching click
416) Discussing reproducibility click

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This website contains other cold fusion items.
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========== Comments will be appreciated
kowalskil@mail.montclair.edu

About my "learn cold fusion" project

Ludwik Kowalski, <kowalskiL@mail.montclair.edu>
Montclair State University, Upper Montclair, N.J. 07043

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In the fall of 2002, to my surprise, I discovered that the field of cold fusion is still active. This happened at the International Conference on Emerging Nuclear Systems (ICENES2002 in Albuquerque, New Mexico). Several papers presented at this conference were devoted to cold fusion topics. Intrigued by the discovery I started reading about recent cold fusion findings and sharing what I learned with other physics teachers. I have been doing this over the Internet using Montclair State University web site

http://csam.montclair.edu/~kowalski/cf/

What follows is a set of items posted, more or less regularly, on that web site since October of 2002. The items reflect my own process of learning, mostly from articles published by cold fusion researchers. I am still not convinced that excess heat, discovered by Fleischmann and Pons, is real or that nuclear transmutations can occur at ordinary temperatures. But I do think that time is right for the second evaluation of the entire field. I do not believe that extraordinary findings of hundreds of researchers are products of their imagination or fraud. Our scientific establishment should treat cold fusion in the same way in which any other area is treated. Those who study cold fusion do not appear to be pseudo-scientists or con artists. The items on my list are arranged in the order in which they were posted on my web site.

P.S.

What follows is an email message I received recently:

Dear Mr. Kowalski,
Help! My name is XXX XXXXX and I am a sophomore at XXXXX High School. In my chemistry class, I am doing a project on Cold Fusion. I was looking on the Internet for websites on Cold Fusion, and I came across your links to your Cold Fusion items. I was wondering if you could give me some advice or information? I would like to know what Cold Fusion is, [and] how Cold Fusion was started. . . . .

I am no longer comfortable saying that “cold fusion is voodoo-science.” I am a physics teacher; how should I answer questions about cold fusion?

Can a nuclear process be triggered by a chemical process? The answer, based on what we know about nuclear phenomena, is negative. On the other hand many experiments seem to indicate the opposite. These experiments were performed many years after the first evaluation of “cold fusion” was made by our Department of Energy. As a teacher I would very much appreciate a

second evaluation of the field by a panel of competent investigators. What can one do to make this happen?

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1) Introducing Cold Fusion to Students

Ludwik Kowalski, <kowalskiL@mail.montclair.edu>
Montclair State University, Upper Montclair, N.J.

I think that cold fusion is worth discussing in the context of introductory physics and chemistry courses, even in high school. What follows is an outline on how I plan to introduce cold fusion next time I teach electricity.

1) Let me summarize what we learned last week. We demonstrated that the amount of heat (Q) generated in an electric circuit is equal to U*I*t., where U is the applied voltage, I is the constant current and t is the duration. To verify this we used a calorimeter, a voltmeter, an ammeter and a timer.

2) Now imagine a situation in which someone claims to discover an electric circuit in which Q is larger than U*I*t. How would you interpret this? How can the amount of thermal energy released exceed the amount of electric energy supplied? The only reasonable answer is that some kind of heat-producing process is going on inside the calorimeter. What is the nature of this process? To answer this question one would have to design additional experiments. In 1989 two electro-chemists, Fleischmann and Pons, made a similar claim (1). Additional experiments, however, were far from being credible.

3) Their electric circuit was a little more complicated than a single wire loop in our calorimeter. It was a glass container filled with heavy water in which a small amount of salt was dissolved. Two electrodes were inserted into the container, one made from platinum and another made from palladium. Platinum was connected to the positive terminal of a car battery while palladium was connected to the negative terminal. The current flowing through the electrolyte and the voltage between the electrodes were measured, as we did in our experiment. The entire container, called the electro-chemical cell, was inserted into a calorimeter and Q was measured. The scientists claimed that the Q significantly exceeded the U*I*t. The difference (megajoules), they announced, was much larger than what could possibly be due to any conceivable chemical process.

4) The electrochemical cell connected to a battery can be viewed as a resistor. For example, if I is 0.1A and U=12 V, then R is 120 ohms. Under such conditions the cell would be receiving, in each minute, the energy of U*I*t=72 joules. Part of this energy would be used to produce chemical changes, mostly breaking molecules of water (bubbling hydrogen and oxygen), while the rest would become heat. In other words, the rate of heating should be smaller than 72 joules per minute and not larger. That is why the experimentally observed excess heat was paradoxical.

5) In my opinion the discovery of excess heat, after being verified several times, should have been announced, more or less, in this way: "we know that the excess heat, sometimes several megajoules, is really produced but we have no idea what process is responsible for it." But that is not how the discovery was presented. The two scientists declared, without having any evidence for it, that the process responsible for the excess heat was nuclear fusion of heavy hydrogen. This was an enormous mistake. Nuclear fusion has been studied for decades and it was well known that a neutron or a proton is nearly always emitted from each fusion event. The number of neutrons emitted to generate megajoules of heat would be more than sufficient to kill researchers observing the cell. Numerous attempts to detect the expected large flux of neutrons (emerging from a similar cell) were not successful. It soon became clear that the heat-generating process, if real, could not possibly be caused by what is commonly called nuclear fusion.

6) Some think that discovering an experimental fact without giving some kind of explanation is not a scientific event. I cannot agree with this. Many important discoveries were made long before they were theoretically explained. Finding something that seems to contradict our current knowledge, and being able to convince others that the effect is real, is

always an important scientific event. It stimulates further investigations and, sooner or later, leads to better understanding of nature.

7) Here is how the first thermal manifestation of nuclear energy (2) was described (in 1903) by Pierre Curie and Albert Laborde: "We have discovered that the salts of radium constantly release heat. ..... One gram of radium develops a quantity of heat of the order of 100 small calories per hour. .... The continuing development of such a quantity of heat cannot be explained by an ordinary chemical transformation." After presenting these facts, and after describing the experimental setup, the authors suggested the possibility that atoms of radium either change slowly or have an ability to use "the external energy of unknown nature." They had no way of anticipating ejection of alpha particles (carriers of energy) from atomic nuclei; the existence of atomic nuclei was discovered much later (E. Rutherford, 1911).

8) I suspect that the discovery of excess heat would have been much less controversial if its announcement had not been accompanied by unjustified speculations about fusion and about practical applications of the phenomenon. These speculations, and other factors (described in 3,4,5,6,7,8,9), created a situation in which the discovery itself was prematurely rejected as invalid. Fortunately, a small group of dedicated scientists did not accept the early condemnation of so-called "cold fusion" and continued to study the phenomenon. They finally identified conditions under which any qualified scientist could observe excess heat (10). The situation was very different 13 years ago; at that time success or failure was a matter of luck; sometimes excess heat was observable and sometimes it was not. Fortunately, the experimental situation has improved significantly. But a process, or processes, through which the unaccounted heat is generated, has not yet been identified. Here is an area of science to which some of you may contribute in the future

.

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7) G. Taubes, "Bad Acience: the Short Life and Weird Times of Cold Fusion," Random House, New York, 1993.

8) T. Mizuno, "Nuclear Transmutations: The Reality of Cold Fusion," Oak Grow Press, Concord, NH, 1998.

9) C. Beaudette, "Excess Heat. Why Cold Fusion Research Prevailed." Concord, NH, 2000.

10) Many recent findings are described in documents downloadable from the Internet site: http://lenr-canr.org/Features.htm
I strongly recommend the 1996 article of M.H. Miles et al., "Anomalous effects in deuterated systems," and the 2001 article of E. Storms, "Cold fusion: an objective assessment,"

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2) A Typical Experiment

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The first publication on excess heat (in the so-called "cold fusion" phenomenon) appeared in 1989 (1). The discovery, and its aftermath, were described in many books (2-8). A typical setup was a glass container filled with heavy water in which a small amount of the LiOD salt was dissolved to make an electrolytic cell. Two electrodes were inserted into the container, one made from platinum and another made from palladium. The platinum was connected to the positive terminal of a power supply while the palladium was connected to the negative terminal. Deuterium ions entered into the palladium electrode (cathode) and diffused to occupy the interstitial positions of the crystal lattice. This preparation process, called loading, is an exothermal reaction (formation of PdD) whose thermal output is about 9,000 calories/mole. The rate at which heat was produced during the loading was small in comparison with the rate of ohmic heating of the cell.

Nothing unusual was taking place at this stage; the current was small and the cell temperature was rising to reach 31 degrees. At that temperature the cell was in thermal equilibrium with its surrounding. After 19 days the atomic ratio of D/Pd was sufficiently high (close to unity) to start the second stage, generation of excess heat. This was probably accomplished by doubling or tripling the voltage between the electrodes. The cell temperature increased to 48 degrees (new thermal equilibrium) and remained high for a long time. During that time the rate at which heat was flowing out of the cell was measured. It turned out to be about two times higher than the rate at which electrical energy was supplied to the cell.

How can it be that the amount of thermal energy released, at any specified time interval, exceeds the amount of electric energy supplied? The only reasonable answer was that some kind of a heat-producing process was going on inside the cell. This unknown process was named "cold fusion." The ability to generate excess heat eventually ended, after about one month. During that time approximately 17 MJ (4.1 million calories) of heat was generated per cubic centimeter of palladium. The authors claimed that this could not possibly be attributed to a chemical process. Attributing it to a nuclear process was certainly premature in 1989.

Ideally one would like to have a process in which excess heat is generated for as long as at least some PdD is left in the cathode. In reality "the energy-out" over "the energy-in" ratio was decreasing progressively till it became less than unity, one month later. This has probably been correlated with the progressive deterioration of the cathode. Note that the thermal output of 17 MJ in one month amounts to only 6.5 watts. Practical applications of the process must wait for the development of long lasting electrodes able to generate heat at significantly higher rates. The mysterious "cold fusion" process must be studied in order to promote possible future applications. Unfortunately, the federal financial support for research in this area has been cut in 1990, mostly due to mistakes made by the early promoters. This, however, did not prevent a small group of researchers (at least one hundred) from exploring the uncharted territory and publishing the results of their studies, mostly over the Internet (9).

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9) Many recent findings are described in documents downloadable from the Internet site: http://lenr-canr.org/Features.htm
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Two appendicies for my CF essay

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I am writing a long essay on cold fusion; hopefully it will also be posted on my web site. What follows are two appendces to the essay.

Appendix 1: Three kinds of Cold Fusion</B>

As previously indicated, three kinds of similar phenomena were known in 1989: cold fusion (CF), Muon-Catalyzed Fusion (MCF) and Cluster Impact Fusion (CIF). In my mind the first is associated with a chemist Fleischmann, the second with a physicist S.E. Jones and the third with a nuclear scientist G. Friedlander. It is well known that palladium reacts to hydrogen as a "sponge." One cubic centimeter of that metal can be loaded with up to 900 cubic centimeters (at NTP) of hydrogen gas. In 1960's Fleischmann used palladium to separate isotopes of hydrogen. The cold fusion idea, and the discovery of excess heat, together with Pons, are probably connected with this work. My purpose here is to briefly describe MCF and CIF. A comment about CF is added at the end.

Jones observed muon-induced fusion long before the discovery of excess heat was announced; his work was rooted in ideas expressed in 1948 by F.C. Frank, from England and further developed by A. Sakharov, from the Soviet Union. According to (2) these two scientists were the first to speculate about spontaneous fusion of protons inside muonic molecules. What is a muon? It is a particle whose charge is the same as that of an electron but whose mass is 207 times larger. Discovered though cosmic ray studies, muons disintegrate rapidly after their formation; their half-life is only 2.2 microseconds.

A muon orbiting around a proton constitutes a muonic hydrogen atom. According to Bohr's model, muonic atoms are much smaller than ordinary hydrogen atoms. And molecules made from muonic atoms, either HD or DD, are much smaller than ordinary HH molecules. That is why the probability of fusion in muonic molecules should be much higher than in ordinarily molecules. Eight years later fusion inside muonic molecules was actually discovered by L.W. Alvarez, an American scientist. Sending a man-made beam of muons into containers filled with liquid hydrogen and liquid deuterium he and his collaborators discovered a pattern of tracks which was later identified as long sequences of fusion events in muonic molecules. The researchers were not guided by the speculations of Frank and Sakharov; their data, however, validated the speculations. It turned out that a single muon, during its short life-time, could become a component of many muonic molecules and thus is able to generate many fusion events. The process was named muon-catalyzed fusion, MCF.

Jones found that this process can not be used for practical purposes because the number of fusion events a single muon can generate, in its short life, is too small. The energy cost of producing one muon in an accelerator is much higher than

what could possibly be catalyzed by it. One of Jones' coworkers was promoting the idea that MCF can possibly play a significant role in geological processes. Influenced by that idea Jones wanted to investigate a possibility of what he called piezofusion; nuclear fusion under very high pressures. Can hydrogen nuclei be forced to fusion inside solid materials? This was the same question that Fleischmann and Pons, only fifty miles away, wanted to answer after observing excess heat. Jones' team was able to identify fusion events with a neutron detector (7). But the rate of neutron emission was about ten billion times smaller than necessary to justify the presumably-observed excess heat.

The third kind of cold fusion was studied at Brookhaven National Laboratory (8). Intrigued by the CF controversy, Friedlander and his co-workers accelerated microscopic droplets of heavy water (containing about 1300 D_2O molecules each) to a modest kinetic energy, about 220 eV per molecule, and observed what happens when droplets collide with a solid target. The idea was to test whether or not fusion occurs in a suddenly compressed droplet. The name of the phenomenon, cluster impact fusion (CIF) was given to the process after hot-fusion-like events were identified on the basis of protons and tritons with appropriate energies. Neutrons were also most likely present but the experiment was not set up to detect them.

The only unusual thing about the CIF was the number of fusion events. There were 10^{10} times more such events than one would expect by using the accepted hot fusion theory. The temperature that a tiny droplet could possibly reach, after being stopped at the target, was certainly below 10^5 K. This number is 10,000 times smaller than the 10^9 K needed inside a hot fusion reactor setuo. In other words, CIF fusion rates are also much too high to be consistent with the existing theory of nuclear fusion.

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8) R.J. Beuhler et al., "Cluster Impact Fusion." Physical Review Letters, vol. 63, no 12 (18 September 1989): 1292-1295
9) Edmund Storms, 2001, "Cold fusion: an objective assessment," downloaded from the Internet site http://lenr-canr.org/Features.htm (References used by the author have been removed to prevent confusion). Many interesting Internet links can be found at that web site.
10) Charles Beaudette, "Excess Heat: Why Cold Fusion Research Prevailed," Oak Grow Press, LLC, South Bristol, USA, 2000.

Appendix 2: Why is it so difficult?

The issue of irreproducibility is very serious. It refers to situations in which a phenomenon is observed by some but not by others. Here is how the issue was addressed in (17): "Most scientists hold the view that anomalous effects in deutrated metals can be explained by experimental errors. Some scientists go so far as attributing positive results to self-deception and even fraud and consign this phenomenon to the realms of Langmuir's 'Pathological Science.' Do to the lack of experimental reproducibility, this field remains practically defenseless against such attacks.

To our knowledge, no laboratory can provide detailed experimental instructions to another laboratory and guarantee the reproducibility of the excess heat effect. Nevertheless, considerable knowledge has been gained concerning experimental conditions that favor the excess heat effect. . . Our experiments indicate that the lack of reproducibility is due largely to unknown and uncontrolled variables contained within the palladium stock. ....[Our results] have been used to support both sides of the scientific controversy regarding anomalous effect in deuterated metals. Our first set of experiments conducted over a 6-month period (25 March - 7 September 1989) produced no significant evidence for any excess enthalpy produced. ....[Other groups] also reported no evidence for excess heat, thus greatly impacting the general scientific opinion regarding this field. All three [other] groups discontinued their experiments after only a few months of investigation.

We continued to investigate other palladium samples and eventually observed significant evidence for excess enthalpy from the use of Johnson-Matthey palladium rods. In retrospect, it would be impossible for any research group to adequately investigate the multitude of variables involved with this field in only a few months. These variables range from the palladium metallurgy to the D_2O purity, the type of electrolyte and concentrations, the electrochemical cell, the electrode arrangement, the type of calorimeter, proper scaling of the experiments, the handling of metals, the current densities used, the duration of the experiments, the loading of deuterium into the palladium, the use of additives, and so on." Ironically, this was written in 1996, seven years after the field of cold fusion was declared to be unscientific (1).

A simplified analogy would be a situation in which stars on the dark sky are seen on some nights only. I am thinking about a planet, somewhere in the universe, whose one side is always in the darkness and whose other side is not accessible. People live in darkness and are not aware of the existence of clouds. But they do see stars occasionally and argue about their reality. Does it mean that stars are not real?

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1) J.R. Huizenga, "Cold Fusion: The Scientific Fiasco of the Century," Oxford University Press, 2nd edittion, Oxford, 1993. (The November 1989 ERAB report to the DOE, called "Cold Fusion Research. A Report of the Energy Research Advisory Board to the United States Department of Energy," is available at http://www.ncas.org/erab)
17) M.H. Miles et al., 1996, "Anomalous effects in deuterated systems," downloaded from the Internet site http://lenr-canr.org/Features.htm
Another paper worth downloading from this good source is "Cold fusion: an objective assessment," by Edmund Storms, 2001.

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4) Three Biographies

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These short biographies of Fleischmann, Pons (chemists) and Jones (physicist) were copied from pages 46-49 of E.F. Mallove's book : "Fire from Ice; Searching for Truth Behind the Cold Fusion Furror," John Wiley & Sons, New York, 1991. I strongly recommend this book, and the book of J. Huizenga "Cold Fusion; the Scientific Fiasco of the Century," Oxford University Press, Oxford, 1993, to all those who are interested.

Martin Fleischmann, now a naturalized British subject, was born March 29, 1927, in Karlsbad, Czechoslovakia, to Jewish parents. The family came to England to avoid inevitable persecution by the Nazis. Martin went to high school in Sussex, England, during the war, attended Imperial College in London after the war (1947-1950), and later distinguished himself by achieving at age forty the professorial Chair in Electrochemistry at the University of Southampton. Fleischmann has been called a genuine Renaissance man with a reputation for brilliant and creative ideas -- not all of which pan out, but such is the nature of creativity. Surely, when one listens to or is in the presence of Martin Fleischmann, one feels that the image of an exceptional polymath fits him like a glove.

Since 1986, Fleischmann has been a Fellow of the Royal Society, an honor given only to the most distinguished of scientists. The author of over 200 scientific papers -- a number of them with Pons as collaborator -- and a number of portions of textbooks, Fleischmann won the Royal Society of Chemistry's medal for Electrochemistry and Thermodynamics in 1979. He was president of the International Society of Electrochemistry (1970-1972). In 1985 he was awarded the Palladium Medal (how appropriate!) by the U.S. Electrochemical Society. Fleischmann, married since 1950, is a father of three (a son and two daughters), and a grandfather of four. His leisure interests run the gamut from skiing, walking, and music to an appropriate avocation for a chemist -- cooking. (Those few readers who still may think of Martin Fleischmann as a quack will be happy to know that he lives on Duck Street in a nice English town.)

B. (Bobby) Stanley Pons is about young enough to be a son of Martin Fleischmann. It was mildly ironic that Pons was born in 1943 in the small town of Valdese in the North Carolina foothills, because on the day of the cold fusion announcement, the huge oil tanker Exxon Valdez (same pronunciation as Valdese) was coming to grief on the rocky Alaskan coast. There soon appeared a MacNelly (Chicago Tribune) cartoon connecting cold fusion with the oil spill. An oil-soaked bird adrift on a buoy was remarking to a similarly blackened seal or sea lion, "Any more word on how those fusion experiments are going?"

Pons's Italian Protestant ancestors had fled religious persecution in the old world. Now, less lethally but in some fashion, Stan Pons was about to be assaulted by many members of the scientific community. He would need a lot of stamina to fight back. In his youth, as at present, Pons was very athletically oriented, engaging in track and football. The cold fusion brouhaha immediately took away from his love for skiing, which in calmer times he had pursued in the Wasatch Mountains, sometimes with Fleischmann. Pons was also drawn to the world of chemistry as a child, as many youngsters had also been, encouraged by parent-bestowed chemistry sets and the like.

Pons attended Wake Forest University in Winston-Salem, North Carolina, graduating in 1965, and began advanced studies at the University of Michigan at Ann Arbor. But with his doctorate almost in hand in 1967, he, the eldest of three brothers, left school to work in his father's prosperous textile mills and to manage a family restaurant in North Palm Beach, Florida. Eventually, his love for chemistry drew him back to active science. With the encouragement of faculty at University of Southampton in England, he entered its graduate program in chemistry and received his Ph.D. there in 1978. Martin Fleischmann was one of his professors. After being on the faculty at Oakland University in Rochester, Michigan, and the University of Alberta in Edmonton, Pons came to the University of Utah in 1983 as an associate

professor, becoming a full professor in 1986, and Chairman of the Department in 1988. He has authored or coauthored over 150 scientific publications.

Unlike Fleischmann and Pons, however, **Steven Jones** was well known to physicists and the hot fusion community, which gave him a credibility that Fleischmann and Pons could not match. That Jones came out with a dissimilar but closely related item of cold fusion news at about the same time, ironically, may have boosted the credibility of Fleischmann and Pons in their claims. But there was initial confusion about what Jones was asserting, because of his well-known earlier work on cold fusion of a different sort -- the concept called muon-catalyzed fusion (Chapter 6).

Much of the difficulty that ensued between Fleischmann and Pons on one side and Jones on the othe -- a friction that has now lessened considerably -- can be understood in part from a chasm of personality differences. Jones is the youngest of the threesome, having been born in 1949 and raised a Mormon, with all that his religion's outlook and demanding codes of conduct implies. Jones was a missionary in Europe for the Church of Latter-Day Saints and abides by the faith in not drinking alcoholic beverages, coffee, or tea. He is the father of seven children. His frameless glasses give him an upstanding, almost Boy Scoutish bearing; he speaks in a soft voice and with hesitation at times grinning and laughing frequently. Jones pursues his science with religious fervor, almost literally. His University stationery bears witness, inscribed as it is with the Brigham Young University motto, "The Glory of God Is Intelligence." For about a decade, Steven Jones and his colleagues had been pursuing muon-catalyzed fusion, a technique that they already had shown, experimentally to produce low-intensity fusion reactions at room temperature within a sample of deuterium -- certainly a kind of cold fusion in its own right."

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5) The Scientific Process

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What follows was copied from J. Huizenga's book "Cold Fusion; The Scientific Fiasco of the Century," Oxford University Press, Oxford, 1993. It is a section (pages 234-236) about violations of the normal rules and procedures of the scientific process in the cold fusion controversy.

"The whole cold fusion fiasco serves to illustrate how the scientific process works. However, seldom do far-out claims receive the amount of national and international attention given to cold fusion. Scientists are real people and errors and mistakes do occur in science. These are usually detected either in early discussions of ones research with colleagues or in the peer review process. If mistakes escape notice prior to publication, the published work will come under close scrutiny by other scientists, especially if it disagrees with an established body of data. The greater the implication of a result, the sooner it will be reexamined. Scientific results, if valid, must be reproducible. When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press.

The scientific process is self-corrective. This unique attribute sets science apart from most other activities. The scientific process may on some occasions move slowly, sometimes even along a circuitous path. The significant characteristic of the scientific method, however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error. This has been so firmly demonstrated again in the present case by showing that there is no evidence to support the claim of measurable amounts of heat energy coming from room temperature nuclear fusion. True progress must withstand the test of time.

When the news of cold fusion broke, the scientific establishment moved quickly to investigate the validity of the far-out claims which many thought were nonsense. The scientific establishment was not too arrogant to examine room-temperature fusion when it came along, even though it contradicted well-established experimental and theoretical results in nuclear physics. There are occasionally surprises in science and one must be prepared for them. Within the first few weeks after the University of Utah press conference, several multidisciplinary research teams could not replicate any of Fleischmann and Pons' reported claims. Such teams were necessary to investigate quickly the cold fusion claims. Had Fleischmann and Pons formed an interdisciplinary team in their early experiments, cold fusion might have had a very short lifetime.

On the first week of May, cold fusion was the cover story of Time, Newsweek and Business Week, an unprecedented occurrence for a science story. Why did cold fusion generate so much national and intemational excitement? Following the many media stories about the erosion of our environment by the greenhouse effect, acid rain, chemical and radioactive wastes, and the Valdez and Chemobyl disasters, the announcement on March 23, 1989, of the sudden possibility of an abundant, cheap and pollution-free energy source captured everyone's imagination. It was a dream come true, giving us new confidence of our technological competitiveness. Few were concerned or even aware that the University of Utah's publication-by-press-conference released the cold fusion story directly to the public, bypassing all the normal checks and controls of the scientific process. There was no manuscript available for evaluation, there had been no peer review of the science; in fact, the University administrators had not even consulted the nuclear physicists on their own campus. There was only an announcement that watts of excess energy had been produced by a nuclear fusion reaction at room temperature in a small electrolytic cell. The press conference did mention the observation of fusion products to reinforce the claim of nuclear fusion. There was no mention, however, that the fusion product yield was more than eight orders of magnitude less than the claimed excess heat. Later it was shown by others that Fleischmann and Pons made their claim of nuclear fusion before having any solid evidence of having observed any fusion products.

The University of Utah's handling of cold fusion is a striking illustration of what happens when scientists circumvent the normal peer-review process, when scientists use the press as a conduit to disseminate information about a claimed discovery in an unrealistic and overly optimistic tone, when scientists require too many miracles to account for their results, when research is done in isolation by scientists who are outside their field of expertise, when data are published by private communication rather than by those responsible, when administrators use potential royalties to force premature publication and when university administrators lobby for large federal funds before the science is confirmed. Cold fusion is an example of bad science where the normal rules and procedures of the scientific process were violated. One can only be amazed by the number of scientists who reported confirmation of cold fusion by press conference, only to follow later with a retraction or at least a confession of irreproducibility. Reproducibility is the essence of science. It has taken upwards of some fifty to one hundred million dollars of research time and resources to show that there is no convincing evidence for room-temperature fusion. Much of this effort would not have been necessary had normal scientific procedures been followed. The idea of producing energy from room-temperature fusion is destined to join N rays and polywater as another example of a scientific aberration.

The purpose for exposing the cold fusion episode is to show that serious mistakes do occur in science. It is important that we learn from these mistakes. I hope examples discussed in this book will give others new insights into the way science should be done. The general scientific enterprise is vibrant and healthy and has weathered the cold fusion flurry with only minor bruises and scratches. The cold fusion fiasco illustrates once again, as N rays and polywater did earlier, that the scientific process works by exposing and correcting its own errors."

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6) Publication of Primary Data

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What follows is a section copied from pages 221-222 of J. Huizenga book: "Cold Fusion; the Scientific Fiasco of the Century," Oxford University Press, Oxford, 1993. It describes an example of publishing of incorrect data about Cold Fusion.

"Researchers have the responsibility to publish their own experimental data. They should be in a position to explain and defend their results to other qualified experts. This is especially true when the reported data are controversial and directly contradict well-established scientific results in the literature. The description of experiments and results should be published in sufficient detail to give the expert reader the possibility of evaluating the significance of the claimed result. This standard scientific procedure was not followed by University of Utah scientists when reporting large amounts of He in the gases evolving from Fleischmann and Pons' electrolytic cells, supposedly from D+D fusion.

On April 14, 1989, three weeks after the well-known University of Utah press conference, Walling and Simons, colleagues of Fleischmann and Pons, submitted a 'theoretical' paper to the Joumal of Physical Chernistry. In this paper they reported, by way of a private communication from Pons and Hawkins, that the He production in cold fusion cells was even larger than that required to account for all of the claimed excess heat. With this very preliminary and unsubstantiated evidence, Walling and Simons proceeded to construct their three-miracle 'non-theory' described in Chapter 3. At this early stage in the history of cold fusion, He was being promoted as the critical evidence for cold fusion. In the Editorial Comment section (a most unusual addition to a scientific paper) at the end of the Walling and Simons paper, it is recorded that one reviewer stated "it is of utmost importance to get the data presented in this paper into the public domain as quickly as possible." Although I support fast (and accurate) publication, the above advice to publish by private communication data that directly contradicted well-established results in nuclear physics was not in the best long-term interest of science. Authors themselves have the responsibility to publish such findings along with supporting experimental evidence. It is completely unsatisfactory to introduce controversial and potentially important experimental results into the literature through someone else's publication by way of a private communication.

On May 8, 1989, at the Los Angeles Electrochemical Society meeting, three weeks after the submission of the Walling and Simons paper, Fleischmann and Pons announced that their He measurements were flawed. Nowhere in the Walling and Simons published paper (published on June 15, 1989) is this very serious mistake mentioned! The He data, if true, would have been revolutionary. In actual fact, however, these data were completely erroneous, and no retraction has appeared. Presumably, since Pons and Hawkins did not publish their He results, they felt no responsibility to retract the incorrect information put into print by Walling and Simons. On the other hand, Walling and Simons apparently felt that it was Pons' responsibility to set the record straight since they didn't add a note in proof warning the reader that the He data were erroneous. This, of course, would have undermined the entire justification for publishing the Walling and Simons paper. The helium episode is an example of a worst case scenario of what can happen when data enter the scientific literature by way of private communication. When errors are discovered, they should be acknowledged immediately, preferably in the same journal."

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7) Pathological Science

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". . . On a bus ride from our downtown hotel to the Hahn-Meitner-Institute on the outskirts of Berlin, Wheeler expressed his views to me about cold nuclear fusion by comparing the University of Utah episode with Rene Blondlot's discovery of N rays. As will be described below, the N-ray affair is one of the most remarkable known cases of self-deception in science which affected many French scientists . . . In 1903 Blondlot, a leading French physicist and member of the French Academy of Sciences, announced he had discovered a new kind of rays, which he named N rays, after the University of Nancy, where he did his research. Following the fundamental discovery of x rays by Roentgen in 1895, Blondlot in experimenting with x-ray sources claimed he had found a new kind of emanation. These strange new rays could penetrate inches of aluminum but were stopped by thin foils of iron.

The general properties of N rays were elusive at best. When N rays impinged on an object it was claimed that there was a slight increase in brightness. Blondlot admitted, however, that a great deal of skill was needed to see the effect of these rays. This did not keep a number of other physicists from reporting and extending Blondlot's findings. A large number of papers were published, many arguing that N rays ought to be important because x rays were considered to be one of the most important types of radiation known. N rays do not exist. The many scientists who reported seeing them were the victims of self-deception.

One of the interesting aspects of this episode is the large number of working scientists who were taken in. When the renowned American physicist R.W. Wood heard about these claims, he went to France to visit Blondlot's laboratory and to observe his experiments first hand. Blondlot at that time was using a prism to separate the different components of N rays. In a dark room Blondlot was demonstrating to Wood that he could measure three or four different refractive indices, each to two or three significant figures. The amazing feature of these experiments, as asserted by Blondlot, was that they were accurately repeatable. Wood listened and observed for a period of time, noticing that Blondlot was measuring the position of the beam of N rays to within a tenth of a millimeter. On asking how it was possible to detect the N rays with such great precision, Blondlot replied (as recorded in Physics Today, October, 1989) 'That's one of the fascinating things about N rays. They don't follow the ordinary laws of science that you ordinarily think of. You have to consider these things all by themselves. They are very interesting but you have to discover the laws that govern them.'

By now Wood had correctly surmised that something peculiar was happening. In the darkened room Wood surreptitiously removed the prism and placed it in his pocket. He then politely asked Blondlot to repeat some of the previous measurements, which he was happy to do. With the center piece of the experiment missing, Blondlot obtained exactly the same results. Wood wrote a devastating account of his observations, which was published in Nature, showing Blondlot's N rays resulted from self-deception Wood's expose finished N rays outside France, but French scientists continued to support Blondlot for some years. "

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8) Burden of Proof

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What follows was copied from J. Huizenga's book "Cold Fusion; The Scientific Fiasco of the Century," Oxford University Press, Oxford, 1993. It is the beginningof the last section (pages 285-287) of the Epilogue. The author is a well knownnuclear chemist; he was the Chairman of the DOE/ERAB panel (Department ofEmergy/Energy Research Advisary Board) which produced a highly critical 1989 report on cold fusion.

“The term ‘cold fusion’ as presently used encompasses a ‘melange’ of claims as discussed in the previous sections of this chapter. The more avid proponents of cold fusion continue to argue that the excess heat in many experiments is so large that the source of the energy must be nuclear fusion or some other unknown nuclear reaction. A fraction of these proponents takes the more conventional point of view and admits that if the process is truly nuclear, there should be a commensurate amount of nuclear ash. The task for these advocates is clear-cut: find the nuclear products. If the reported intensity of nuclear products is orders of magnitude less than the claimed excess heat, then the excess heat is not due to a nuclear reaction process. Furthermore, if the claimed excess heat exceeds that possible by other conventional processes (chemical, mechanical, etc.), one must conclude that an error has been made in measuring the excess heat.

There are, however, many proponents who ignore or deny the fundamental equality between the magnitudes of the excess heat and nuclear reaction products. They cite the unsubstantiated claims of insignificant amounts of neutrons, tritium, helium, etc. to bolster their belief that the claimed excess heat must be nuclear. The latter school of advocates wants to put the burden of proof on the skeptics! They challenge the skeptics to prove that the excess heat is experimental error. What is required, they say, is a modem-day R.W. Wood to rise up and find the critical mistake being made in calorimetry. This, of course, is impossible because the level of absurdity of cold fusion claims has grossly multiplied with time. First, heat was reported to be produced with heavy water and palladium, where light water served as the control. Then it became fashionable to claim even greater success and larger power gains with light water and nickel.

For good measure, element transmutation has also been reported to occur in different media including biological solutions. Even the ultimate dream of the alchemists is claimed, the production of gold from mercury. All of this wishful thinking about nearly-for-free energy has attracted a fringe element of charlatans attempting to promote their own fame and fortune. It is not the responsibility of skeptics to disentangle this web of cold fusion claims ranging from the marginal to the ridiculous. All of these unsubstantiated claims seem to merit the same value to proponents who lump them together as equally-weighted contributions. It is this assorted package of claims that serves as the basis for the oft-heard myth that cold fusion has been verified hundreds of times. No claim is too preposterous to be denounced by the advocates. Any and every result is acceptable under the umbrella of cold fusion. Denial of the equivalence between the magnitude of excess energy and the number of products associated with a nuclear reaction is a rejection of the law of conservation of mass and energy, and qualifies as blatant pseudoscience.

In the final analysis, the burden of proof falls squarely on the shoulders of the proponents of cold fusion. That is the way science works. Advocates must describe their experiments in sufficient detail in publishable papers so that their results can be replicated by knowledgeable scientists. Ten (or some other incommensurate number of) nuclear products per second does not serve as a replication of one watt of excess power. Verification will occur when enough separate and independent replications have been performed so that experienced scientists agree on the outcome. After nearly four years and the expenditure of many tens of millions of dollars worldwide, we still only have unsubstantiated and fragmentary claims of watts of excess heat generated from light and heavy water (and H_2 and D_2 gases) by some

unknown mechanism reported to have a nuclear origin. To this date, not a single, well-controlled and reproducible experiment has been reported where corresponding amounts of excess heat and nuclear reaction products are reliably measured.

This is the current [1993] status of cold fusion. Hence, I conclude now, as I did in July 1991, that the claim that cold fusion is a nuclear process producing watts of excess power, without commensurate nuclear reaction products, is a chimera and qualifies as pathological science (see Chapter 12). At best, the cold fusion fiasco may lead to new information on materials and energy storage, but even this has not been established at this time. Furthermore, there is still no persuasive evidence that any nuclear reaction products have been positively identified in cold fusion experiments in very low level background environments. Regrettably, the idea of producing useful energy from room-temperature nuclear reactions is an aberration. ....."

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9) Storms on Scientific Method in CF

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The piece below was extracted from an article of E. Storms, as downloaded from < http://home.netcom.com/~storms2/park.html >

The article is a review of a book "Voodoo Science" published by R. Park. Addressing the issue "observations versus theories" Storms wrote: "Science prides itself on resolving conflicts using logic and facts to provide a better understanding of nature. . . [But to Park] the explanation becomes more important than the observation. Because this particular explanation can not be believed, the observation must also be rejected. Thus, a major flaw in modern science is revealed - a Theory is more important than an Observation. The behavior of nature is not real unless it can be explained, especially using conventional concepts. This flaw in logic is at the heart of the book and provides an explanation for rejection of these and other subjects by many scientists.

New discoveries always conflict with some dearly held belief. This conflict when used to reject the claims, prevents new discoveries from being explored and properly explained. This is not to say that all ‘strange’ ideas are correct or that all have a new and worthwhile explanation. Clearly, some should be rejected as being caused by obvious error, fraud, or simple insanity. The problem comes in deciding how much time and resource should be devoted to a search for an explanation and how the resulting facts should be evaluated. ..... If science is to clean up its act, this defect in the approach scientists use needs to be addressed. A clear and extensive discussion of this general problem can be found in the book "Revolution in Science" by J. Bernard Cohen (1985) or "Forbidden Science" by Richard Milton (1994)."

The next piece was extracted for a relatively recent articles of E. Storms, "A Critical Evaluation of the Pons-Fleischmann Effect" Infinite Energy, 2000, #31, p 10. I downloaded this article as the StrmsEacritical.html file from the following web site: < http://lenr-canr.org/Features.htm >

"Many new studies are available to make an objective evaluation of the Pons-Fleischmann effect possible. ... A wide range of observations involving anomalous production of energy as well as nuclear products have been published. While many of the claims are still open to interpretation, the general conclusion is that an important, novel phenomenon has been discovered which deserves renewed interest. Since the claims of Profs. Pons and Fleischmann were announced ten years ago, studies have been undertaken by hundreds of scientists in laboratories of at least nine countries in an attempt to verify what they then called "cold fusion." Some of the results support the idea that the anomalous nuclear reactions can be made to occur in special solid materials. Many more investigations failed to show the claimed effects. Although this body of work is largely unknown to the general scientific community, it nevertheless is believed by many to show the claims to be false.

Ten years of work worldwide have produced over 2500 published papers, many peer reviewed, which have

answered most of the objections leveled by the critics. It is now possible to make a more objective evaluation of the phenomenon than was previously possible. Unfortunately, during this time the claims have been the subject of considerable distortion. The reader is asked to lay aside the emotional reaction 'cold fusion' can generate and read the following arguments with an open mind. ..... Of course, many studies have been poorly done and cannot be used to evaluate the claims either way. Such deficiencies are common in all aspects of science. [In this article] an attempt will be made to determine how well the claims have been replicated and whether consistent patterns of behavior have been revealed. For good reason, these are the techniques science demands be followed when any new ideas are evaluated.

In contrast to these conventional criteria, numerous critics have observed that 'extraordinary claims require extraordinary proof.' This is a very high standard which has prematurely doomed many new ideas to the trash bin, some deserving and some not. We need to realize that potential errors can be found by clever critics in any study, no matter how well done. Hence, a perfect proof is almost impossible to obtain until considerable information has accumulated. Such an accumulation is very slow and difficult if an idea is completely rejected, as has been done in this case. Consequently, at the very least, I would hope that the skeptical reader would entertain a possibility that some parts of the claim deserve further study, even though all important questions have not, as yet, been answered."

That was an introduction to a very long and worth reading article. Let me mention that Edmund Storms is a retired material scientist with a Ph.D. in radiochemistry. Prior to his retirement he worked for 34 years at Los Alamos National Laboratory. His work involved basic research in the field of high temperature chemistry as applied to materials used in nuclear power and propulsion reactors, including studies of the "cold fusion" effect. He was among the first scientists to discover radioactive tritium in electrochemical cells after they were used to produce excess heat.

Over seventy reviewed publications and monographs resulted from this work as well as several books, all describing an assortment of material properties. He presently lives in Santa Fe where he is investigating the "cold fusion" effects in his own laboratory. These studies have resulted in sixteen presentations to various conferences including the ACS and APS. In addition, twenty-one papers have been published including three complete scientific reviews of the field, one published in 1991, another in 1996 and the latest one in 1998. A critical evaluation of the Pons-Fleichmann Effect was published in 2000. In May 1993, he was invited to testify before a congressional committee about the "cold fusion" effect. These biographical details were extracted from < http://home.netcom.com/~storms2/index.html >. Let me quote a letter that Dr. E. Storms sent to the editors of Scientific American.

* * * * * * * * *

November 30, 2002
Editors Scientific American
415 Madison Ave. New York, NY 10017

Dear Sir:
Your analysis in the December issue about why science is neither respected nor understood by the general public I found to be very much to the point. Those occasions when science accepts claims that are later found to be false clearly give science a black mark. Unfortunately, in an effort to avoid such embarrassment, science also rejects claims that are later found to be true. I ask you, which is the greater threat to science and mankind, accepting a claim that can have no possible benefit or rejecting a claim that

can have great benefit?

I could offer many examples of how good ideas have been rejected in the past, but I would like you to consider one very important claim that now has almost universal rejection, yet is supported by a growing body of data. As a scientist, I was trained to judge the reality of nature from good data based on replicated experiments. Yet, I find that the scientific community increasingly bases what is real on the opinions of a few respected journals and academics using theoretical arguments, regardless what is being discovered by other scientists operating in the real world. How is the general public expected to respect science when it does not follow its own stated rules of evidence?

The discovery I would like to use as an example of this double standard is what is called LENR or low energy nuclear reactions. This has also been given the very inaccurate name of cold fusion, a name that now causes rejection and ridicule. This ridicule comes from people who have no understanding about what is now known, yet their opinions are accepted as fact. Is this the way science is supposed to operate?

If you wish to be true to your stated wish to make science more respected, I suggest you educate yourself about this important phenomenon by reading information available at www.lenr-canr.org. There you will find over a thousand publications that support the reality of such anomalous nuclear reactions, as well as several reviews in full text that answer important questions raised by skeptics in the past. Serious scientists rejected ‘cold fusion’ in the past for good reason. These reasons no longer apply. If science cannot correct a past rejection, then what good is the scientific method? Can anyone respect a scientist who cannot change his/her mind after being presented with better data?

Respectfully, Edmund Storms, Ph.D.

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10) A Russian Connection

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In October 2002, I attended the International Conference on Emerging Nuclear Energy Systems in Albuquerque. There I met a Russian scientist, Alexander Karabut. He was one of the authors who published an early report (1) on nuclear processes taking place in so-called "cold fusion." Here is what was written about this work by one of the opponents (2) of CF. "Some of the most incredible results were reported by groups from Russia..... [For example, (1). That group reported observing] charged particles with thirteen different energies ranging up to the maximum of 18 MeV, neutrons with energies up to 17 MeV, gamma rays, x-rays of several energies, many radioactive isotopes, palladium fission fragments and an increase in helium concentration in cathodes by mass spectroscopy. Some of the radiation effects continue for times of minutes to hours after the [current] is turned off. … The above Russian results could not be verified in other major Russian laboratories, such as Dubna."

These critical words were written ten years ago. Since then the byproducts of nuclear processes were reported by many groups in several countries. Karabut spent ten years to study nuclear process associated with generation of excess heat. His talk at Albuquerque was the summary of findings. Knowing Russian I helped the author to improve the translation and had a chance to ask for clarifications. Karabut thinks that the scientific establishment in Russia considers cold fusion to be voodoo science. In fact, after returning to Moscow he sent me an interesting book. The author (3) heads the "Commission to Oppose Pseudo-Science and Falsifications in Scientific Research." I had no idea that such a commission has been created by Russian Academy of Sciences. The book ranks cold fusion at the same level as N rays, astrology, extrasensory perception and magic.

Karabut hinted that the antagonism against cold fusion in Russia has more to do with the competition for very limited financial support than with objectivity. I am not in a position to determine the validity of this hint. But my impression of Dr. Karabut was very positive. He graduated from the Soviet equivalent of MIT and worked as a weapons scientist in top secret laboratories (until they were closed after 1990). His main instrument was a glow discharge chamber in which cathodes, such as Pd, could be loaded with deuterium. The maximum voltage used was 1500 V, the maximum current was 100 mA. The chamber was equipped with a very sophisticated flow calorimeter and various detectors. My translation of Karabut's presentation will probably be posted on this web site. For the time being let me summarize their major findings.

1. Alpha particles of 14 MeV and protons of 3 MeV were identified when the cathodes were Pd, Ti and Ni, but not Ta. I like the idea of using the $1/r^2$ law to demonstrate that particles were emitted by the cathode. No charged particles (above the usual background) were detected when an identical experiment was performed with H_2 instead of D_2

2. Gamma rays were studied with a Ge-Li detector. A bombardment of a Pd cathode for about ten hours produced enough gamma activity to be observable (above the background) for up to eight days after

turning the current off. Numerous nuclides were identified on the basis of their characteristic peaks and decay times. In particular all nuclides of the A=101 beta radioactive chain (from Rb to Ru) were present. The first nuclide of this chain has a 31% chance of decaying by emitting a neutron.

3. A large variety of non-radioactive nuclides also appeared in the cathode after it was exposed to D_2 in the glow discharge chamber. Several analytical techniques were used to identify stable elements, including mass spectroscopy. The isotopic composition of iron, produced in the cathode, was found to be very significantly different from what is common in nature (50% of ^{57}Fe near the surface and 30% at the depth of 400 nm, instead of usual 2.2%).

4. The rates at which some of the recognized nuclides are produced are roughly 10% of what be required to match the rate at which the excess heat is generated. In other words, the Russian team tentatively identified some of the non-radioactive "ashes" of the mysterious nuclear combustion process. This is an important finding; absence of “ashes” was often presented as an argument against “slow nuclear burning.”

5. Coherent x-ray radiation, associated with atomic L and M levels of solids, was observed in some experiments.

Such observations, if confirmed, would be extremely significant and I am surprised that they are not used widely as arguments in cold fusion debates. Some of them could be verified using standard equipment available in most nuclear laboratories. A piece of Pd was used as a cathode of a D_2 discharge tube and this resulted in large changes of isotopic ratios of several elements. How could this happen without a nuclear process of some kind? Why doesn’t this happen when D_2 is replaced by H_2? These extremely important questions would have been answered more quickly, I assume, if the area of cold fusion research were not isolated from mainstream science. My own impression is that cold fusion is still blacklisted by the scientific establishment. Why are mistakes made in 1989 still used against honest cold fusion researchers?

References:

1) A. B. Karabut et al., Phys.Rev, Lett. A 170 265 1992
2) J.R. Huizenga, "Cold Fusion: The Scientific Fiasco of the Century," Oxford University Press, 2nd edittion, Oxford, 1993. (The November 1989 ERAB report to the DOE, called "Cold Fusion Research. A Report of the Energy Research Advisory Board to the United States Department of Energy," is available at http://www.ncas.org/erab)
3) E.P. Krugliakov, "The Highwaymen of Science," Nauka, Moscaw, 2001 (ISBN 5-02-013116-4).

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11) Bottom Line

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What is the main difference between hot fusion and cold fusion communities? In both cases the goal was to build a device whose energy output exceeds the energy input, without consuming chemical fuel. The hot fusion community has been trying to achieve the "break-even" point for five decades and it knows exactly why reaching it is so difficult. The cold fusion community, on the other hand, started by building break-even devices without understanding what was going on and why.

This calls for an elaboration. The hot fusion pursuit started in the 1930's when cloud chambers were used to visualize tracks resulting from d+d collisions. The QM tunneling theory was used to anticipate what should happen at extremely high temperatures; hydrogen bombs confirmed these predictions. Practical attempts to use nuclear fusion for peaceful purposes have been guided by sophisticated theoretical investigations since the 1950's.

The so-called 'cold fusion' pursuit is much younger. It started in 1989 when Fleischmann and Pons discovered that unexplained thermal energy is generated in palladium loaded with deuterium. The first method of loading was electrolytic but excess heat was also observed with other loading methods (for example, by placing palladium chips in deuterium gas under high pressure and in glow discharge tubes). Those who discovered the effect believed in its nuclear origin but they had no evidence. That antagonized many scientists and the entire field was declared unscientific. This opinion prevailed and US funding agencies cut their support for 'cold fusion' research.

Fortunately, many researchers were not discouraged and continued to investigate the phenomenon with very limited means. They soon realized that nuclear processes, if any, responsible for the excess heat in solids, are totally different from those occurring in very hot ionized gases. The field, however, is still treated as pseudo-science by founding agencies. Several theoretical attempts to explain the mechanism of generation of anomalous heat have been undertaken but, so far, none of them has been generally accepted. The entire field is still at the fact-gathering stage. The absence of a theory is not a good reason to disqualify an emerging field of research.

What are the main issues in the CF area? In my opinion they are as follows:

1) The name "cold fusion" is highly inappropriate; it perpetuates an early misinterpretation of the phenomenon. The name anomalous energy (AE), used by many authors, is much more suitable. What is wrong with saying "the AE effect?"

2) All factors influencing generation of anomalous energy should be clearly identified. Great progress has been made in this area, and the reproducibility of experimental results, by qualified scientists, is much better today than it was thirteen years ago.
<P>
3) Experimental evidence for nuclear processes taking place in solids loaded with deuterium is badly needed. At least one such process should be identified and confirmed by several investigators. This is more important, at this early stage, than showing that the rate of the process is commensurable with the rate at which the AE is generated inside a solid. Confirmation of a nuclear process, or of enhanced radioactivity, caused by a chemical structure would already be a great step forward.

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12) Physics Teachers Talking about CF

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Physics teachers on the Internet list Phys-L, often discus controversial topics and Cold Fusion was not an exception. Prompted by a reply to my recent message (12/10/02) I went to the list archive at:

http://lists.nau.edu/archives/phys-l.html

and started searching for "cold fusion." This produced more than one hundred hits. Unable to read so many old messages I randomly selected some. Let me show them here; they are quite interesting. The names of authors are not shown; authors are identified as teachers. The first message below was a reply to my call for comments about cold fusion. It is followed is my response and then by messages from the archive.

Teacher 1 (12/10/02)
The topic of cold fusion is most interesting but I'm surprised that it is still a topic of current interest on this list-server. I recall that someone reported that cold fusion had been achieved a few years ago but no one has been able to reproduce the effect to this date. However, I thought that most scientists now agree that cold fusion can never be achieved. Is there any new evidence to the contrary .....or is it like the alchemists who spent many fruitless decades trying to turn lead into gold?

Teacher 2 (12/10/02)
I think there exists new evidence for three things:
1) Excess heat is now reproducible, and not only via electrolytic loading. In using electrolytic loading one must be aware that success depends on many factors not known ten years ago.

2) Unusual nuclear processes (emission of nuclear particles and radiation) have been observed in metallic crystals, such as palladium, heavily loaded with D_2.

3) These puzzling processes do not resemble nuclear fusion taking place in high temperature plasma. The ratio of tritons over neutrons, for example, is highly skewed (by many orders of magnitude) in favor of charged particles. Another dramatic difference is that the reported production of ^4He is not associated with the emission of 23.8 MeV photons. For more details see my short essays posted at:

http://blake.montclair.edu/~kowalskil/cf/

and, above all, references which are quoted in my essays. I am only an outsider who tries to learn from experts. It would be desirable if a new panel of experts (physicists, chemists and material scientists) were created by our scientific establishment to evaluate the validity of recent findings and claims. I do not want to be like those who refused to look at what Galileo was showing because, according to Aristotle, such things were not possible. Yes, cold fusion announced in March of 1989 may turn out to be nothing but self-deception or even, in some cases, fraud. But I would like to hear a statement of that nature from honest experts. The first "official" pronouncement (November 1989) should be revised to account for research done since 1990. That is what I think.

Teacher 3 (8/2/02)
When challenging the "laws" of physics there's a right way and a wrong way to go about it. (The same applies to any other activity.)

A1) The rules need changing, and the scientific community handles it well.
A2) The rules don't need changing, and it is handled well. Example: The null results of Eötvös.
B1) The rules need changing, and it is handled poorly.
B2) The rules don't need changing, and it is handled poorly. Examples: N-rays, cold fusion.

And there is a fifth class, where the scientific community responds scientifically but fails to bring the broader society along. Examples: copper bracelet therapy, magnetic bracelet therapy, homeopathic medicines. The task of challenging established ideas is not assigned only to giants like Michelson and Rumford and Rutherford, but also to every worker-bee in the scientific community. To summarize:

-- Primarily we should discuss the right way to challenge the established rules. And the necessity for doing so.
-- Secondarily we should discuss N-rays, cold fusion, homeopathy, etc. as counterexamples, as perversions. We shouldn't call them "positive".

Teacher 2 (12/16/02)

> I am (in general) a rabid anti-conspiracy nut. This promise for
> Cold Fusion is SUCH an attractive possibility that MANY labs
> will immediately jump into the arena as soon as it is established.
> A FEW brave souls will (as we speak) be doing their damnedest
> to get positive results and be among first on the bandwagon. ...

But cold fusion is far from being able to convince others that it is "an attractive possibility." At an early stage a field of science is vulnerable to many things. It has been bastardized prematurely, I think. Whose fault was it? Mostly of those who announced the discovery, and of those who tried to control the discovery by political means. Was it a conspiracy? I do not think so. It just happened this way, unfortunately. I small initial perturbation created a lot of unexpected consequences.

Teacher 4 (7/1/1998)
Teacher 2 makes an important point. The skepticism about "cold fusion" arises not because of any "conspiracy" on the part of "orthodox" science, but rather because literally hundreds of competent scientists have attempted to reproduce the effect without success. The nuclear reactions associated with fusion are well understood, and have well known signatures (reaction products such as neutrons and gamma rays). These have been looked for with the most sensitive of detectors, and have not been found. Since fusion is a nuclear process, it is these nuclear reaction products that carry away the excess energy. If you don't have any nuclear reaction products, then it can't be a fusion reaction that is taking place in these "cold fusion" cells. Rather than taking someone's word for it (even if that someone happens to be Clarke or Schwinger), I prefer to base my own judgments about "cold fusion" on the evidence. And right now the evidence for "cold fusion" is pretty meager, while the evidence against it is pretty strong, at least IMHO.

Teacher 5 (7/1/1998)
I believe that it is nigh-impossible to change people's opinions regarding "Cold Fusion," so I usually am not tempted to dive in and argue about it. When any reversal of opinion requires the losing of face, then reversals of opinion cannot occur in public. Therefore why even try? If "CF" is eventually shown to be valid, then everyone will leap on the bandwagon, but there will be no detailed investigations of the ones who spent years ridiculing the topic. "Who, me? I was always a supporter!" It sure is easy to be on the side that's winning? I hope that I myself, years from now when CF is shown to be entirely bogus, will still have the stomach to read all these old archive files and see what led me into my shameful pro-CF beliefs. I probably won't though. I'm just as human as anyone.

> Isn't this the standard scenario of pathological science? Wild enthusiasm,
> followed by belated skepticism when the confirming experiments don't work,
> followed by increasing paranoia on the part of the original discoverers,

> whereupon the whole thing goes underground and survives on rumors and
> conspiracy theories.

Yes, it suggests that pathological science is a distinct possibility. However, we cannot argue that, since the above symptoms exist, THEREFORE it must be pathological science. There are other alternatives, so the conclusion does not follow. But since pathological science is a possibility, we should be wary and not immediately adopt solid beliefs regarding the reality of LENR (low energy nuclear reaction) phenomena.

> The excuse that they won't make it publish for fear of ostracism just
> doesn't wash. If it really works, and they can demonstrate it conclusively
> (the real thorn in the ointment is that word--conclusively)

Not necessarily. If it really works, yet is a feeble and hard-to-initiate effect, and if it goes against solid theory, then it becomes incredibly difficult to convince anyone that it exists. Rather, the effect itself will be judged to be impossible, and this fact will be used to judge the reliability of researchers: anyone who sees evidence in support of CF is in incompetent researcher (and knowing the reception they will receive if they noise it around that they've seen such things, the they are probably stupid.) If CF papers are constantly rejected by journal editors, and finally some separate CF-only peer review journals come into being, then researchers simply publish their works there. Those who wish, can subscribe. Those who know that CF is a waste of time need never see a paper about it.

> they, and Pons & Fleishmann, will be wealthy beyond their wildest dreams,
> and then couldn't care less about what the scientific community thinks.

Hardly. Only if CF was easily converted from a laboratory curiosity into a major new energy source would wealth and fame be automatic. The researchers who successfully replicate the CF phenomena have had no luck in making the effects reliable, or increasing their scale. (The people at CETI were claiming various successes, but they are a business, and they keep their developments behind a cloak
of secrecy.) The CF effects seem to be critically dependent on microscopic surface processes which nobody understands well. If it was easy, we wouldn't be having this discussion, and "CF" would have been immediately industrialized. That it was not, is evidence either that is doesn't exist, or that it always was a feeble, flaky, poorly-understood effect. In addition, if it is an effect which attracts the wrath of those who dislike the idea that theory-violating anomalies exist, then it is little wonder that the CF field is just where it is today. "Extraordinary phenomena require extraordinary evidence". In other words, we raise the bar for results which go against theoretical expectations, and lower the bar for results which validate known theory.

> In fact, if they did demonstrate it conclusively, and could
> give a rational scientific explanation of what they did, the
> scientific community would embrace them with open arms.

They have no rational explanation. There are many competing theories, but none mesh easily with known physics. I expect that we will see CF-based commercial products long before anyone figures out how the process works. If Pons and Fleischmann are ever embraced by science, it will be after they are safely dead and are unable to remind everyone of the suppression and ridicule which took place.

> Not only would they celebrate one of their own, but these people
> would be the saviors of scientific research, because everybody would
> be eager to fund all kinds of research, hoping to find more of these
> breakthroughs. If you believe all this is going to happen, or that it has
> already happened but is being kept secret for fear of ridicule, they I
> have a bridge up in Brooklyn, that I would like to interest you in
> next time I'm in your neighborhood.

Not kept secret for fear of ridicule exactly. Papers are rejected after peer review, and if most peers have an attitude of "I won't believe it unless you show me incontrovertible proof", then no papers will be published. Do most research

findings require incontrovertible proof? What about the common wry assertion that 90% of the papers found in journals will be shown to be wrong? There is a bias against CF, the barriers have been artificially raised against it.

Now, popular articles with a pro-CF slant, that is another story. Of course editors are fearful to publish such things. Look at the reaction towards them just in this group! Who has the bravery to stand up to that sort of critical response? Few people, if their jobs depend on it. I'm safe, people can attack me all they want and it only damages my ego, not my career or family's income. Actually, it is more than likely that the editor of a magazine already is hostile to CF, and so the fear is on the part of the writers. As a professional science writer, do I dare to damage my reputation by reviewing the latest ICCF conference, or by even mentioning that such conferences are still taking place? I certainly would think twice. Probably much more than twice.

> I believe that Pons & Fleischmann most recently took their operation to the
> French Riviera, where they were working in a lab funded primarily by Sony
> (actually, not a bad investment for them--they put up what is for them
> petty cash for a few years and if it does pan out they are huge winners. If
> it doesn't, they have lost nothing of consequence to them). But I recently
> heard that Sony had cut off their support. Can anyone confirm or clarify
> this information?

I think the lab is no more. I know that Dr. Fleischmann had retired awhile back. They were unable to take their results much beyond the hard-to-replicate "laboratory curiosity" stage they initially started with.

Teacher 6 (9/20/1999):
I enter this discussion with great fear, but I can't help getting this out of my system. (1) Can we agree that there can be a small amount of muon induced fusion in a tabletop experiment, but not enough to make significant temperature changes? (2) Can we agree that the other types of cold fusion that are being discussed require us to violate Coulomb's Law and therefore are unlikely to occur?

Teacher 7 (9/21/1999):
First of all, just because someone says 'a new result is only accepted if there is at least a plausibility argument advanced to support it' doesn't make it so. You don't have to look any further back than the announcement of high-Tc superconductors to see that such is not the case. When the announcement was made, no one had any theoretical explanation for how it could happen. The BCS theory only applies in metals with free electrons that can be paired up, not in ceramic insulators. But, the claim was made that these ceramics could be made superconducting at much higher temperature than the highest known metallic superconductor. What was the reaction of the physics community? Not to say, 'Oh that doesn't fit our neat BCS theory, so it must be wrong.' No, it wasn't that at all. Large numbers of groups rushed to their labs to try it out to see for themselves if it worked. And, lo and behold, it did work! To this day, as far as I know, there is no satisfactory explanation of the effect. Even John Bardeen himself was unable to come up with an explanation. But it works, and the alloy combination has been modified to tweak the temperature up to nearly 100 K. I expect that some day someone will hit on a way to explain the effect, but the lack of a theory has certainly not been a restraint on people utilizing the effect. There are commercially available kits to demonstrate high-Tc superconductivity in probably every physics department in the country.

On the other hand, when CF was announced, hundreds, literally, of groups went to their labs to try and replicate the effect. Unfortunately, not enough information was given in the press conference to know all the conditions that were used. One group, at Ohio State, wore out a video tape they had obtained of the press conference, looking at the apparatus, comparing the size of the hands in the pictures, etc. Many meetings and conferences were held, the hundreds of groups reported mostly negative results, while a few reported sporadic positive results. These were hailed as having done it right, the others as having done it wrong. The bottom line, for me at least, is that no prototype energy producing device has been forthcoming. In other words, there still is no water heater.

There is a lesson in the CF business. One should not let dollar signs cloud your ability to be objective about your work. And be sure you have your ducks all in a row before you start shooting. P&F gave their first scientific paper on CF at a chemical society meeting in Dallas, (not even to a bunch of physicists) and were asked the two most obvious questions:

1) have you integrated the power input to the cells over the duration of the experiment to be sure you have net excess energy (they had not), and 2) what happens when you do the experiment with ordinary hydrogen (that experiment was only in process, and seemed to give the same effect, so they weren't going to talk about it). Eventually they blamed physicists for being severe critics, but it started with chemists. Carl Sagan said something that I think has been quoted on the list before: "I believe that the extraordinary should be pursued. But extraordinary claims require extraordinary evidence." I hope cold fusion does pan out some day. But you really can't blame people who have tried it and found nothing for being skeptical. And it is not just a matter of dismissing it out of hand because there is no theory to back it up.

Teacher 8: (9/19/99):
> This is a valid problem: Conflict of Interest in those who render
>judgment upon a new discovery. "Don't put the foxes in charge of the
>chicken coop." Who should judge whether CF is valid? Most hot-fusion
>professionals are in danger of emotional bias. Scientists are human, so
>they'd better not go around declaring that they can control their biases.
>If the CF phenomenon is genuine, then it means that the staggering amounts
>of money put into Tokamak-style fusion might have been wasted. It means
>that hundreds of people devoted their careers to a technology which might
>prove of little worth should electrochemical-fusion result in efficient
>reactors. Obviously the pressures on such people would be tremendous.
>They would have to be literal *saints* to not be affected by it. If they
>are normal, non-saintly humans, then they would be in danger of succumbing
>to tricks of their subconscious, such as conveniently finding strange
>justification and weak excuses to dismiss CF as unreal, and they would not
>even know that they were doing this. It would seem perfectly sensible at
>the time, yet future historians would see something entirely different.

And likewise for the CF proponents. What they lack in time and money expended they make up for with pride and the desire to retain professional respect in a field marred almost from the get-go. The earliest proponents of CF were the worst of all possible foxes in charge of the chicken coop. Everyone since has labored under this cloud and they are mostly in the unenviable position of "put up or shut up," a virtual death sentence of many things science in today's research atmosphere.

>In all my reading of CF literature, I've not encountered any "conspiracy
>theory" stuff. In the "perpetual motion" and "antigravity" crackpot
>fields the situation is far different. There it's rare to find a
>researcher who DOESN'T accuse government or industry (or Space Aliens!) of
>suppressing the research. When the crackpots start discussing
>antigravity, it's an effective (though dishonest) tactic to bring up
>conspiracies. The crackpots will launch into paranoid tirades and destroy
>their own credibility.
>
>But try the same with Cold Fusion people, and it is not *their*
>credibility which comes into question. Cold Fusion requires serious
>brainpower and facilities before any research can be done. Cold Fusion
>supporters are professional physicists and engineers, not weak-minded
>basement inventors who, once disparaged, will STAY disparaged.

With the possible exception of the two that put it on the map: While Pons' "partner in crime" has perhaps gotten some unfair coverage, Pons himself was the original apparently-respected researcher who could not sort out conflict of interest, who could not stand the thought of being corrected (much less being wrong) and who could not stand up to the political machine that took over. Had CF followed the path set out by Jones, it would not be where it is today. This statement is one of those double-meaning statements recently discussed in the "recommendations" thread. If Jones had his way, there would not be much promise of virtually unlimited "table-top" energy. It is clear that Jones did not have

his way, and yet there is still not much promise of virtually unlimited "table-top" energy. Given this outcome, it seems that an original approach of slow methodical unheralded peer-reviewed research would have been better for the entire CF field.

>...perhaps CF truly is bogus, and the outcast status of the "CF research
>community" is entirely proper. I myself look at the evidence and decide
>that CF has about a 95% chance of being real. Does that make me a
>"believer"? Of course not. A "believer" believes, and evidence be
>damned. Just as disbelievers disbelieve, and evidence be damned.

To paraphrase, I myself look at the evidence and decide that CF has about a 5% chance (or less) of being real. Does that make me a "disbeliever"? Of course not. A "disbeliever" disbelieves, and evidence be damned. Just as believers believe, and evidence be damned. Although I previously stated that I am a "disbeliever," this was merely to draw a comparison.

Rather, I am extremely skeptical. I was a graduate student in solid state physics at the University of Utah when the story broke, and I was acquainted with several behind-the-scenes people involved (though not PF) in the early days, including Salamon and others. I also had the luxury of just sitting back and observing what was happening. Not long after, I was fortunate enough to be able to witness seminars given by Jones at the University of Utah, only a short drive from his lair. And unfortunately, I and my peers were likely the first grad students on this planet subjected to quantum mechanics homework problems involving CF, from a professor who had been sitting in on telephone conferences and chalkboard discussions literally in the first hours of the story. No more going to look the answers up! :-(

As it did then, the community will ultimately decide the validity, interim and final. Truly. And those who have purposefully impeded the advance of science on both sides of the question will ultimately have their comeuppance. PF have already *rightly* had their comeuppance. Others have followed. Others will follow. And the rest will survive with reputations validated or revived.

Teacher 8 (9/22/99):
>First of all, just because someone like Teacher 8 says 'a new
>result is only accepted if there is at least a plausibility argument
>advanced to support it' doesn't make it so. You don't have to look
>any further back than the announcement of high-Tc superconductors to
>see that such is not the case.

It was not my intention to imply that nothing can proceed without at least a plausibility argument (if not more). My point, admittedly not well presented, was that experimental results, in order to be peer-reviewed published, generally require more than a vacuum of discussion. If nothing else, such results, to be published, at least require a discussion of how or why current theory might be inadequate to explain the results, if that appears to be the case. Rondo further pointed out a crucial requirement in the particular case of anomalous result: that any published work of this nature, peer-reviewed or not, provide great and specific detail regarding the duplication of the measurement, so that everyone can literally rush to their labs to try it, with more to go on than the proverbial video tape. At times there exists an uneasy truce between theory and experiment. A comparison of the examples of high-Tc superconductors and cold fusion will probably make for interesting reading some day, in the annals of social studies.

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13) A Brief Introduction

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What follows is a paper on what used to be called, quite inappropriately, Cold Fusion. Several names have been proposed to replace this misleading name. My own preference evolved toward anomalous energy (AE) phenomena. This name focuses on the unexplained energy puzzle without trying to impose a premature interpretation. I am still not certain that AE is real but I think that scientific papers, including the one below, should be validated scientifically. I met the author of the paper at the International Conference on Emerging Nuclear Systems (ICENES2002) and became interested. He is a Russian scientist who has been conducting AE experiments for more than a decade. Two of his presentations have been published (1,2) in the ICENES2002 report.

As a Russian speaker I was able to help the author improve his presentation in English, a language in which he is far from fluent. We talked about his paper before it was formally presented, and we discussed it afterwards. What I heard in Russian was much clearer than his English translation. That is why I decided to improve it and asked the author to send me the original Russian text. He did this and I paraphrased it. The goal was to improve the readability of what may turn out to be a very significant scientific contribution. The text that follows is based on the original Russian paper, on conversations with the author and on what was presented at the conference. Naturally, I tried to stay as close to the original as possible. The author made several corrections and allowed me to make the paper available over the Internet. A large number of relatively recent articles about the AE phenomena are now available (3).

Claims made in the article are highly unusual and I am puzzled. Will Dr. Karabut's experimental findings, said to be ~100% reproducible, be confirmed by other competent scientists? I do not know; I am a physics teacher and only a part-time nuclear researcher. It was a great challenge to learn and to write at the same time.

(1) A.B. Karabut, Research into Powerful Solid X-Ray Laser (wave lengths 0.8 to 1.1 nm) with Excitation of High Current Glow Discharge Ions.

(2) A. B. Karabut, See the article below.

(3) These articles can be downloaded from http://lenr-canr.org/Features.htm

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Generation of Heat, Long-Living Atomic Levels in the Solid Medium (1 to 3 keV) and Accumulation of Nuclear Reaction Products in a Cathode of a Glow Discharge Chamber

A.B. Karabut

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This paper was presented at the 11th International Conference on Emerging Nuclear Energy Systems, Albuquerque, New Mexico, USA, October 2002.

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Abstract:

The described experimental results, based on (1), describe processes associated with the excess heat generated in a deuterium discharge chamber. A calorimetric method was used to demonstrate that thermal energy produced in the chamber can exceed the electric energy supplied to it by as much as 10%. The excess energy is attributed to nuclear reactions taking place near the surface of the palladium cathode bombarded by positive ions of very low energy (0.5 to 2 keV). The process is accompanied by the emission of protons (3 MeV), alpha particles (14 MeV), soft X-rays (up to 100 R/s) and the production of heavy nuclides (at the rates of up to 10^{13} atoms per second). Most of the results were statistically significant and 100% reproducible.

1) Excess Heat Measurements in a Flow Calorimeter

The apparatus used in these experiments consisted of a chamber pumped out down to 10^{-3} Torr and filled with hydrogen or deuterium up to 10 Torr (see Figure 1). The glow discharge was established between parallel plates, palladium-covered cathode and a Mo anode, separated by several milimeters. Other cathode materials, and other gases, were used in control experiments. The cathode, the anode, and the body of the chamber were water-cooled using three independent circuits. Each circuit was equipped with two thermoresistors, one at the input and one at the output, plus a flow meter.

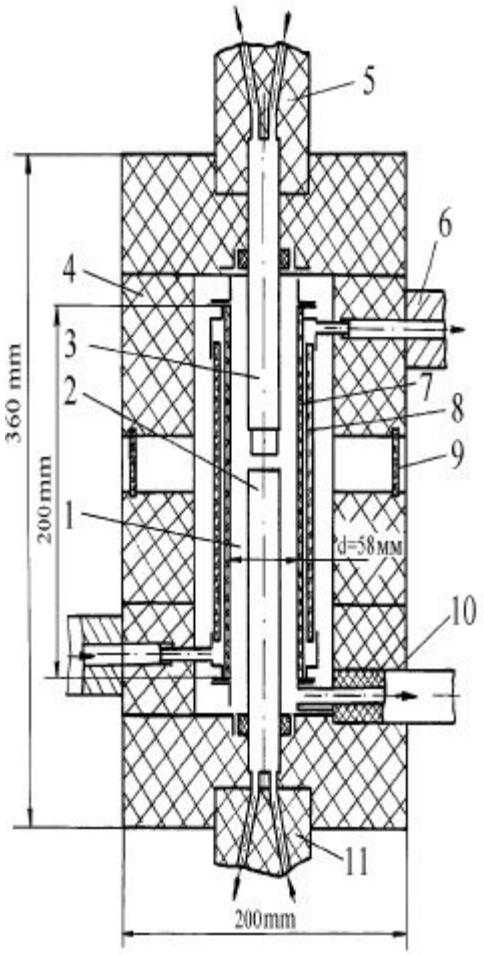

Figure 1 (left): The glow discharge chamber used as a continuous flow calorimeter. (1, the discharge chamber; 2, cathode unit; 3, anode unit; 4,thermal insulation cover; 5, insulation of the anode cooling system; 6, the chamber cooling system; 7,the discharge chamber tube; 8, the chamber jacket tube; 9, windows in thermal insulation cover; 10, the vacuum hose; 11, insulation of the cathode cooling system.)

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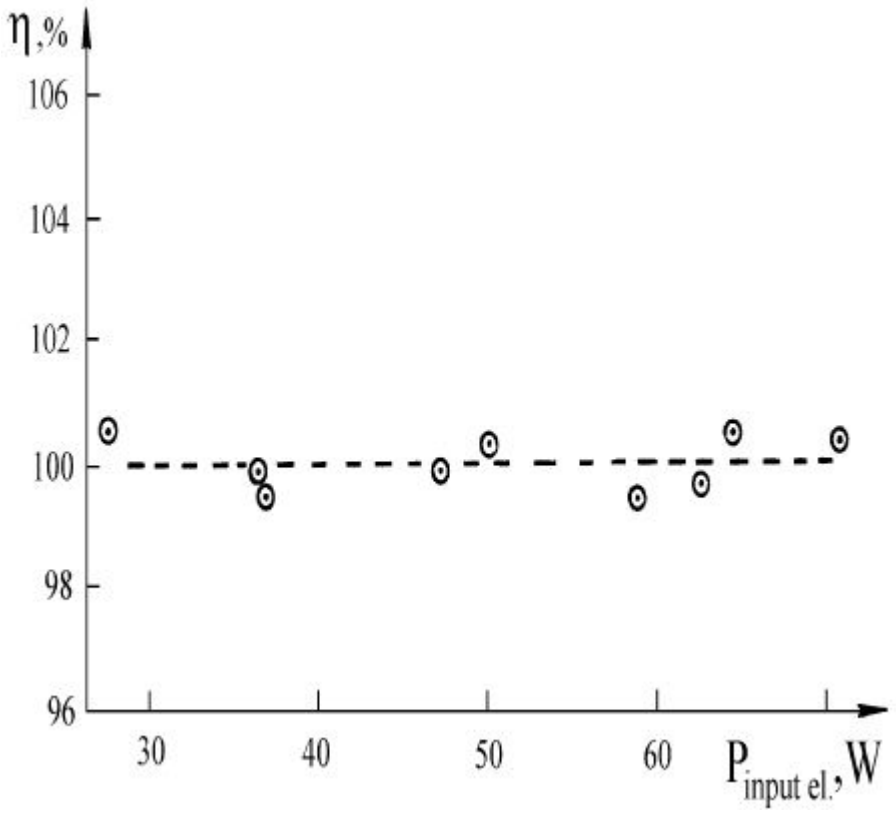


Figure 2: Results of calorimeter calibrations. Nearly 100% of electric energy supplied is removed as heat when the current was flowing through ohmic resistors placed inside the chamber.
****

The heat excess rates, that is, differences between the thermal output wattage and the electrical input wattage, were measured using the Pd cathode in the deuterium gas. The results are shown in Figure 3a. The heat excess rate depends on the following parameters: current density in the discharge chamber, applied voltage, duration of current impulses (t) and duration of intervals between the pulses (T-t). Note that T is the period of repetition, the inverse of the frequency, from 100 up to 1000 Hz, of the electric pulse generator. The value of excess wattage tends to increase with input wattage. Note that excess power also increases with the time between the current pulses, (T-t). This indicates that releases of heat in the cathode continue to take place after the current is turned off (that is, between the consecutive current pulses). The largest excess of power takes place when the difference of potential is between 1000 and 1400 volts; further increase in the voltage results in a rapid decrease of the excess power.

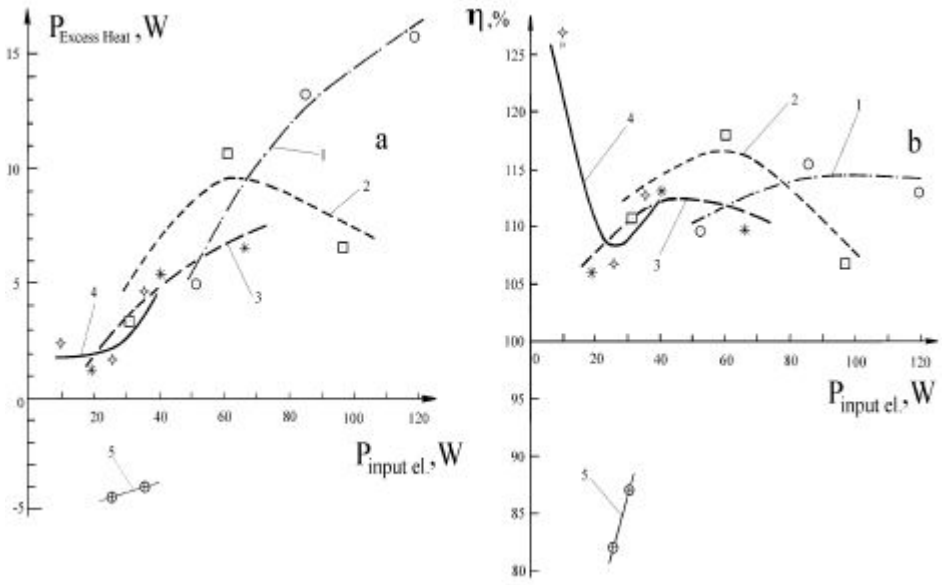


Figure 3 (left): Excess heat power versus the electrical input power for the palladium cathode in deuterium at the pressure of 10 Torrs. The distance between the anode and cathode was 11 mm. Different curves in correspond to different duty cycles of the electrical power supply.

Figure 3b (right): Heat removed as a percentage of supplied electric energy. Different curves correspond to different duty cycles of the electrical power supply.
****

Each curve in 3a or 3b corresponds to a different value of t/T, where t is the duration of the current pulse and T is the period of repetition. The curves labeled as 1,2,3 and 4 correspond to t/T being 0.22, 0.16, 0.11; and 0.054, respectively.

At some pressures (inside the discharge tube) the power excesses were found to be negative, as shown above (curves labeled as 5). Further investigations revealed that this is accompanied by a laser-like emission of X-rays, to be discussed later.

In several reproducible experiments the total measured energy released from the palladium cathode was 4 MJ during the total time of 120 hours This amounts to a mean rate of excess heating equal to 9.2 W, as illustrated in Figure 4.

Measured excess heat rates could not possibly be attributed to a chemical reaction inside the chamber. The slight decrease of the excess power was most likely associated with the progressive deterioration of the palladium cathode.

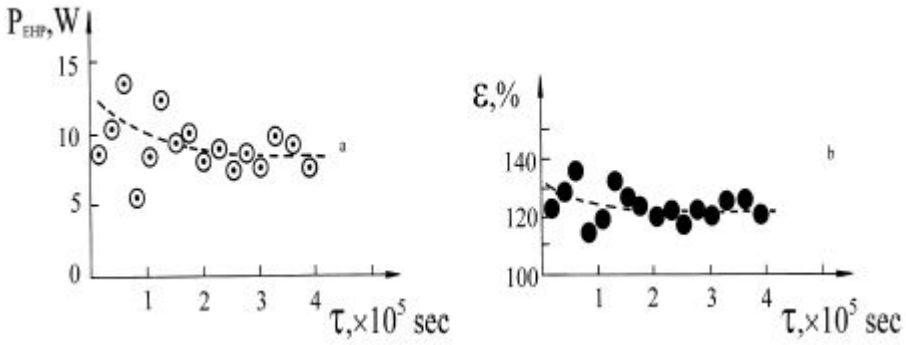


Figure 4a (left): Dependence of the excess heat power on the duration of experiments. System Pd-D_2; t/T=0.13, current 220mA.

Figure 4b (right):): Same system as in 4a. Dependence of the total heat removed (expressed in terms of percentages of input power) on the duration of experiments.
****

2) Registration of Charged Particles
Protons and alpha particles were observed in a setup with a displaced anode, as indicated in Figure 5. They were counted by using plastic track detectors (purified CR-39, Fukuvi Chemical Industry, Japan). The methodology of detection was similar to that used in testing for radon. The observed average rates of emission turned out to be between

10 and 15 particles per second (up to 250 tracks per square centimeter of the detector area and per 6 hours). Some of the detectors were positioned to see the cathode, others were used to determine the background radiation inside the apparatus. Detectors were wrapped in protective Al foils of thickness between 11 and 33 microns. Protons with energies up to 3 MeV, and alpha particles of 14 MeV, were detected in experiments with the Pd, Ti and Ni cathodes (but not the Ta cathode) exposed to the deuterium plasma. The dependence of the number of tracks on the distance from the cathode, r, was found to be consistent with the $1/r^2$ law.

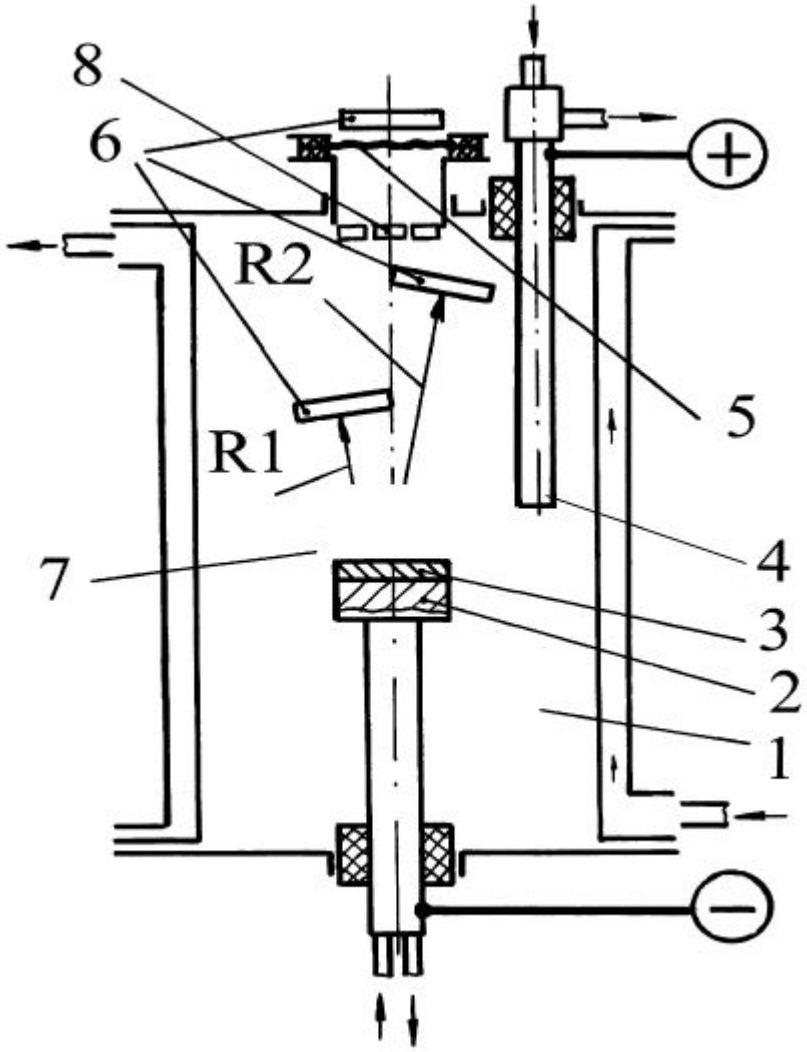


Figure 5: Experimental setup to study emission charged particles and x-rays. (1, discharge chamber, 2, cathode holder; 3, cathode, 4, anode, 5, beryllium window; 6, CR-39 track detectors; 7, glow discharge region; 8, TLD detectors.)
****

A control experiment with the Pd cathode and ordinary hydrogen showed that the density of tracks in the detectors exposed to the cathode was essentially the same as in the background. In other words, protons seemed to be emitted from the Pd cathode exposed to the D plasma but not exposed to the H plasma, as illustrated in Figure 6. The left side graphs show distributions of proton track lengths in D plasma (each for different thickness of Al foils) while the right side graphs show corresponding distributions in the H plasma. As previously indicated, tracks recorded in the H plasma can be viewed as the background not associated with the processes inside the cathode.

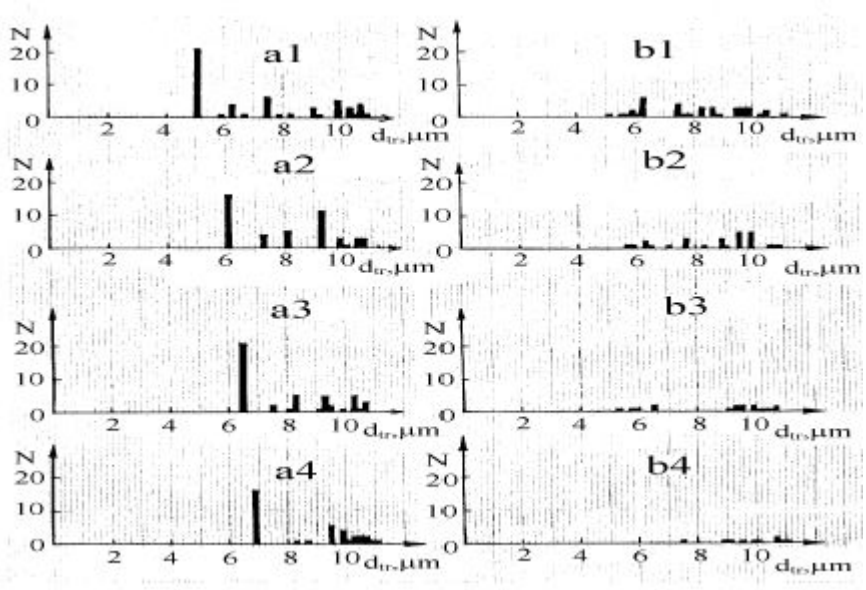


Figure 6: Distributions of lengths of tracks in the CR-39 detectors recorded in the deuterium plasma (left side spectra) and in hydrogen plasma (right side spectra). The protective covers were: 11 microns of Al for a1 and b1, 11 microns of Al plus 60 microns of polyethylene for a2 and b2, 22 microns of Al plus 60 microns of polyethylene for a3 and b3, 33 microns of Al plus 60 microns of polyethylene for a4 and b4. The left side spectra (in deuterium) were collected in ten hours of discharge while the right side spectra (in hydrogen) were collected in five and a half hours of discharge.
****

The rates at which alpha particles and protons are emitted, at different voltages, and at two different currents, are shown in Figure 7. Note that the dependence of Dn (particles per second in all directions) on the discharge voltage is more or less exponential. The recorded rates, however, are much too small to be responsible for the observed excess heat power. It is conceivable that protons are associated with DD reactions; the emission of 14 MeV alpha particles remains a mystery.

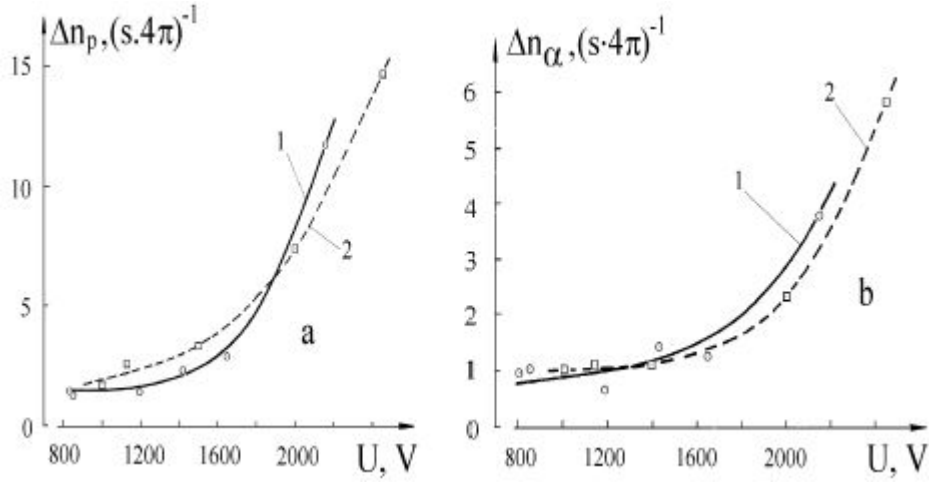


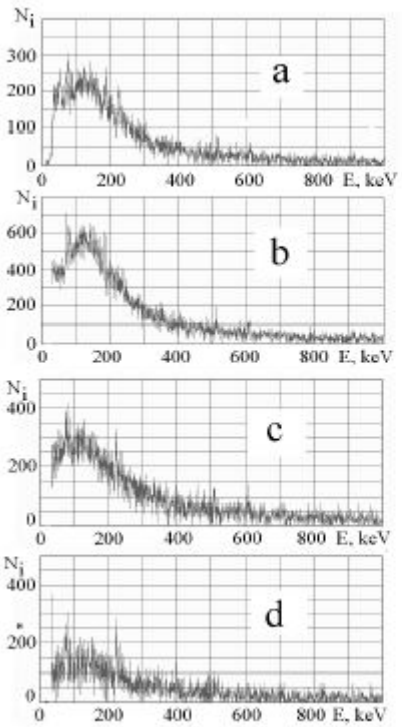
Figure 7a (left): Emission rates of 3 MeV protons at different discharge voltages. 1, for the discharge current of 220 to 300 mA; 2, for the discharge current of 370 to 450 mA (Ti cathode in D_2).

Figure 7b (right): Emission rates of 14 ± 3 MeV alpha particles at different voltages. 1, for the discharge current of 220 to 300 mA; 2, for the discharge current of 370 to 450 mA (Ti cathode in D_2).

****

3) Registration of Gamma Rays.

A Ge-Li detector connected to a multichannel analyzer was used to detect gamma radiation between 0.1 and 3 MeV. To reduce the outside background, the detector and the chamber were shielded by 10 mm of lead. Numerous radioactive isotopes responsible for the emission of gamma rays were identified on the basis of characteristic spectral lines. Gamma ray spectra, recorded during the discharge, and at different times after the discharge, are shown in Figure 8. In a typical experiment the discharge was turned off after tens of hours of the exposure to plasma and gamma rays could be observed for up to eight days before being reduced to the background level. The gamma ray background spectra were recorded during prolonged time intervals (up to several months) between the experiments (starting 10 to 14 days after turning the discharge off). The duration of a background recording could be as short as one hour or as long as 65 hours; fluctuations of background spectra were less than 10%.



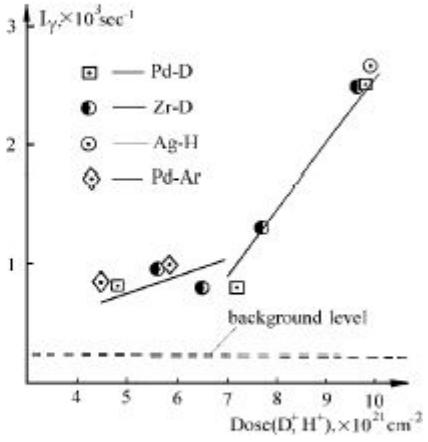


Figure 8 (left): Progressive decrease of gamma radiation after ending an experiment in which a palladium cathode was exposed to the deuterium plasma. The (a) spectrum a was recorded during the discharge, the (b) spectrum was recorded one day after turning the discharge off; the (b) spectrum was recorded two days later; the (c) spectrum was recorded six days later; the (d) spectrum was recorded eight days later.

Figure 9 (right): Dependence of the intensity of gamma rays (at the end of an experiment) on the total number of current ions per unit area. Different symbols are used to distinguish four different combinations of cathode and gas, as labeled.

****

The recorded counts per second were subsequently converted into absolute gamma ray intensities, I, expressed in counts per second, in all directions. This was done by assuming that the distribution of gamma rays is isotropic, by taking into account the geometry and by using known detector's efficiencies. The dependence of I on the exposure to plasma, immediately after a discharge, is shown in Figure 9. Note that the exposure to plasma is expressed in terms of numbers of ions per unit area. The dependence is more or less exponential and, surprisingly, not very different for the four cathode-gas combinations (Pd in D, Zr in D, Ag in H and Pd in Ar).

Groups of radioactive nuclides of half-lives ranging from 1 to 8 days were identified in the Pd cathode after its exposure to the D plasma for 8 days. The spectra shown in Figure 8 were analyzed in terms of two components: peaks and continua. The peak component exceeded the background by a small delta factor ranging from 2.5 to 5, depending on peaks. The continuous component, on the other hand, exceeded the background by a delta factor ranging from 8 to 10. The analysis of data, based on (2), led to the conclusion that gamma rays are emitted from the neutron-deficient (and thus beta-radioactive) nuclides with atomic mass numbers ranging from 16 to 136. In particular, all products of the following A=101 chain:

Rb -> Sr -> Y -> Zr -> Nb -> Mo -> Tc -> Ru

were identified on the basis of numerous peaks, as summarized below. The numbers shown are peak energies in keV.

Rb/Sr	Sr/Y	Y/Zr	Zr/Nb	Nb/Mo	Mo/Tc	Tc/Ru
111.6	128.3	98.3	119.3	157.5	119.9	306.85
271.6	163.4	118.7	140.6	180.7	533.5	545.06
251.6	474.1	216.9	373.9	276.1	590.9	623.8
363.1	510.7	133.8	597.8	280.2	1012.5	489.1
92.8	590.4	104.4	597.8	294.6	713.0	344.0
	232.7	666.6	146.9	722.2	466.3	1599.3
	1091.8	694.3	661.8	912.2	810.8	1590.1
	1362.9	744.1	668.7	1095.8	797.1	1759.8
		1124.8	729.7	1924.5	1042.6	2032.1
		1062.9		2009.5		2041.2
				2565.4		1957.6

It is remarkable that the first three nuclides are known emitters of fast neutrons (31%, 2.37% and 1.94% respectively) and that all gamma lines associated with the above A=101 chain were actually identified in the recorded spectra. Rates of production of various nuclides were calculated from the areas below the peaks on the basis of known efficiencies of detection. The beta radioactive chains of A=16,17, 23, 30, 46, 47, 51, 54, 55, 58, 63, 64, 71, 75, 80, 84, 92, 97, 99, 100, 101, 102, 103, 104, 105, 106, 107, 108, 109, 110, 118 and 123 are main contributors of the observed gamma rays. The rates of production of gamma-radioactive atoms did not exceed 10^5 per second. Most of the atoms produced in the cathode turned out to be non-radioactive, as described in the next section.

4) Accumulation of Trace Elements.

Assuming that most of the excess heat is associated with nuclear reactions taking place in the cathode, the material near its surface was often analyzed after prolonged discharges. Average rates of accumulation are displayed in Figure 10a. The horizontal dotted lines refer to the average rates of emission (of these nuclides) needed to produce the excess heat rates of 10 W and 1 W, as indicated. Drastic alterations of isotopic ratios were discovered as illustrated in Figure 10b. The percentage of ^{13}C, for example, changed from about 1% (natural) to about 20% (due to the discharge). Asterisks refer to absent isotopes. The ^{58}Ni isotope, for example, whose natural abundance is nearly 68% was not found among the trace elements produced during the discharge.

The methods used were: secondary ion mass spectrometry, secondary neutral mass spectrometry and spark mass spectrometry. The first method consisted of the following procedures: removing the outer defective layer of 1.5 nm by plasma etching and scanning the 5 nm layer to determine the concentrations of trace nuclides. Then a layer of 100 nm

was removed and another layer of 5 nm was scanned. The procedure was repeated two more times to study the distribution of trace nuclides up to the depth of 400 nm. In one case, for ^{57}Fe, steps were considerably shorter than 100 nm, as illustrated in Figure 11. The absence of ^{58}Ni, ^{70}Ge, ^{73}Ge, ^{113}Cd and ^{116}Cd isotopes, and high abundance of ^{13}C, ^{57}Fe and ^{48}Ti isotopes, seem to be typical of processes contributing to generation of the excess heat.

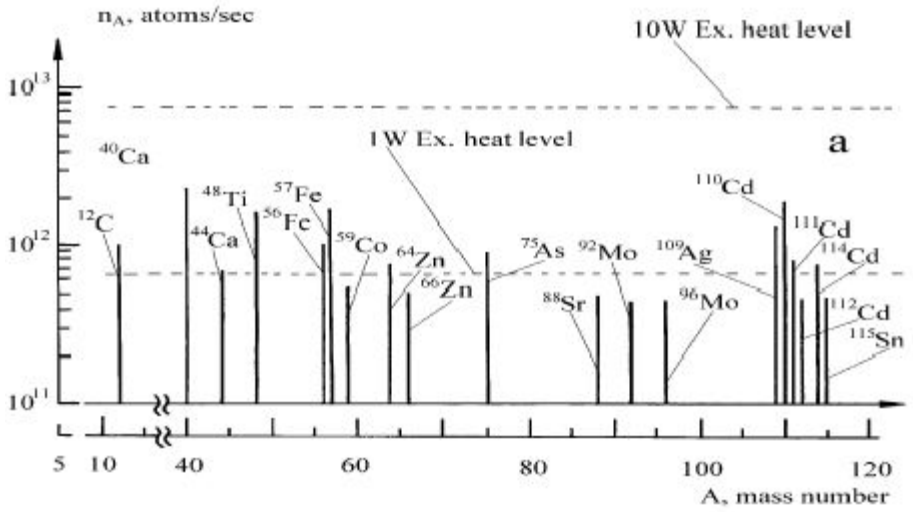


Figure 10a : Average rates of acumulation of different trace nuclides produced in the 100 nanometer layer of the palladium cathode bombarded by deuterium ions. The average current (during the four hours) was 100 mA.
****

These data were obtained after the four-hour Pd-D discharge at the current of 100 mA and pressure of 10 Torrs. The natural concentration of ^{57}Fe is known to be 2.2% while the observed concentrations were found to be much higher. They changed from 50%, near the surface, to about 30% at the depth of 400 nm. Strongly "unnatural" isotopic ratios were found in other trace elements, as illustrated in Figure 10b. Many naturally abundant isotopes, such as ^{58}Ni, ^{70}Ge, ^{73}Ge, ^{74}Ge, ^{113}Cd and ^{116}Cd, were simply not present.

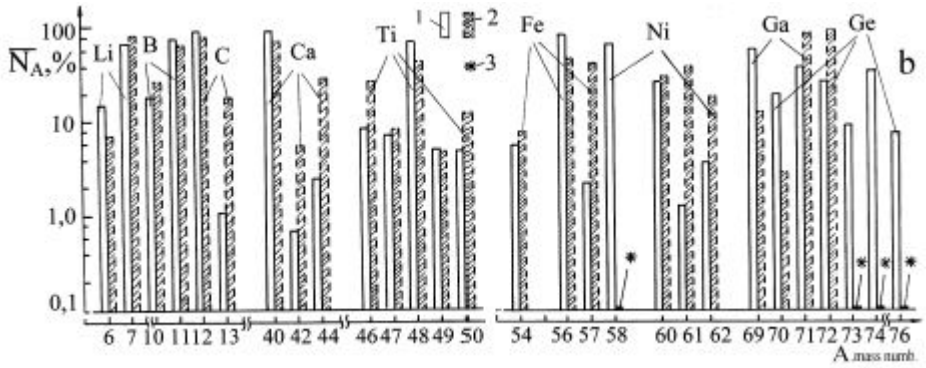


Figure 10b: Crossed bars show percentages for some isotopes at the end of one four-hours-long bombardment of a palladium cathode by deuterium ions. These can be compared with the adjacent left-side bars (clear) referring to natural isotopic percentages. Difference between heights of adjacent bars illustrate isotopic anomalies resulting from the bombardment of Pd by deuterium ions.

****

Absolute concentrations of trace elements were obtained by subtracting concentrations found in the unused palladium from the concentrations found in the cathodes exposed to the deuterium plasma. The elements accumulated in the exposed cathode could be subdivided into three groups: those whose atomic masses were close to Pd, those whose atomic masses were approximately two times smaller and those whose atomic masses were below 40. The identified isotopes, those whose abundance was higher than 1%, were: ^{7}Li, ^{12}C, ^{15}N, ^{20}Ne, ^{29}Si, ^{44}Ca, ^{48}Ca, ^{56}Fe, ^{57}Fe, ^{64}Zn, ^{66}Zn, ^{75}As, ^{107}Ag, ^{109}Ag, ^{110}Cd, ^{111}Cd, ^{112}Cd and ^{114}Cd. Most of them were found in the first 100 nm layer of the cathode. The rates of production of these isotopes, shown in Figure 10a, were estimated by dividing the total number of accumulated atoms by the duration of the experiment, 20,000 seconds. It seems that nuclear reactions producing ^{12}C, ^{44}Ca, ^{48}Ti, ^{56}Fe, ^{57}Fe, ^{59}Co, ^{64}Zn, ^{66}Zn, ^{75}As, ^{107}Ag, ^{109}Ag, ^{110}Cd, ^{111}Cd, ^{112}Cd, ^{114}Cd and ^{115}Cd are the strongest contributor to the production of the excess heat, as illustrated in Figure 10a. The dashed horizontal lines of that figure

are drawn to indicate average rates of production nuclides necessary to generate heat at the level of 1 W and 10 W, as indicated.

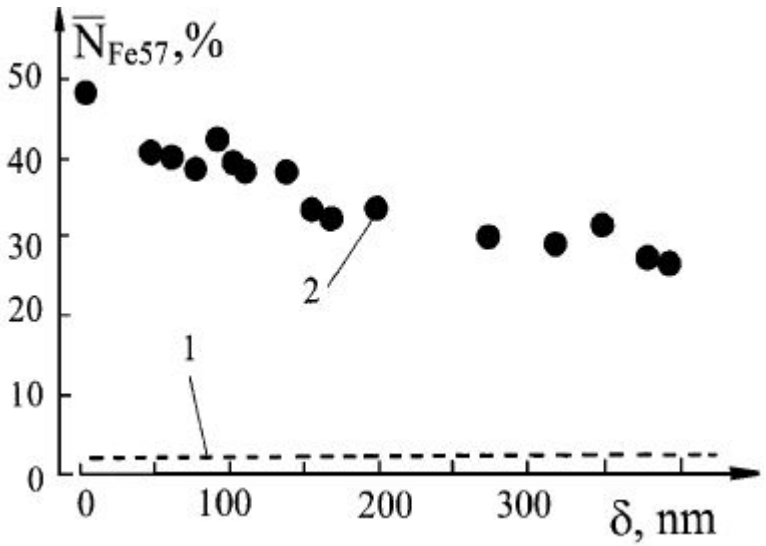


Figure 11: Dependence of the percentage of ^{57}Fe on the distance from the surface of the palladium cathode bombarded for four hours by deuterium ions (2000 volts and 100 mA).
****

5) Emission of soft X-rays.
As previously indicated, the plasma discharge was often associated with the emission of soft X-rays. They were studied with the a TLD and with the scintillation detector mounted on the photomultiplier. The intensity of X-rays was found to increase rapidly with the discharge current, as illustrated in Figure 12. The currents between 100 mA and 300 mA were obtained by changing the difference of potential from 800 V to 2000 V. The dependence of the X-rays intensity on the current is more or less the same for all cathode-gas combinations.

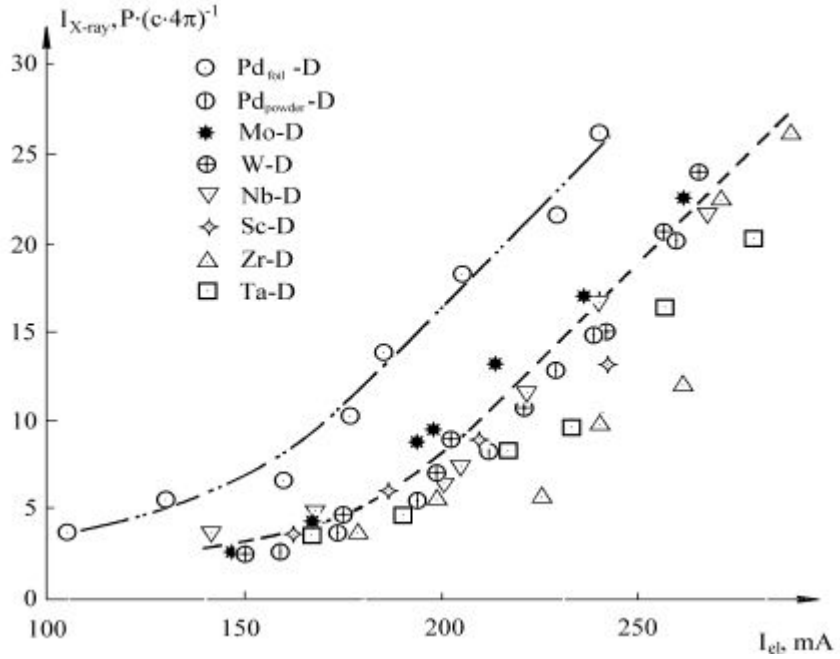


Figure 12: The dependence of the x-ray intensity, in roentgens per second per 4p, on discharge current for the palladium cathode in deuterium (open circles) and for other cathode-gas combinations (as indicated). The data were obtained using a calibrated scintillation detector. The discharge voltages ranged from 800 to 2000 V.
****

Bursts of X-ray emission were also observed several minutes to several hours after turning the current off. It was noticed that only prolonged exposures to plasma (at least 10 to 20 minutes) resulted in post-discharge bursts. Statistical analysis of the time distribution of bursts is consistent with the idea that excited levels, with energies between 1 keV and 2 keV, are populated inside the cathode during the ionic bombardment. The life times of these exited solid state levels, however, are presumably discrete (up to several msec).

6) Discussion:
The high-current glow discharge experiments, described above, revealed: 1) the existence of excess heat production, 2) progressive accumulation of small amounts of isotopes of several elements near the surface of the cathode, 3) emission of penetrating radiation and 4) emission of charged particles. These are manifestations of a previously unknown fundamental phenomenon. **It consists of the excitation of metastable states (energies 1-2 keV, lives tens of miliseconds) inside the crystaline material. These L,M states are presumably populated via inelastic interactions**

of plasma ions with the L,M electronic shells of the ions in the solid cathode. It is natural to postulate that nuclear reactions recorded in this study were initiated by formation of such states. Here is a general description of observed processes and products:

1) Under properly chosen conditions (pressure and distance between the electrodes), a region near the cathode becomes an ionic conductor. More than 90% of the applied potential difference, and more that 90% of the electric power, appears in that region. Positive ions are accelerated here up to energies between one and two keV.

2) The accelerated ions interact with the ions of the solid cathode material in several atomic layers near the surface. Such interactions are basically different from similar processes at higher energies (10 keV and above). The low energy processes can be described as:

$D^{+}* + Pd^{4+} \rightarrow Pd^{4+}* + D^{+}$

where Pd4+* is the positive palladium ion with the excited L, M electronic shells while Pd4+ is the ground state palladium ion in the crystal lattice of the cathode. The calorimetric measurements, and the evaluation of the maximum power of X-ray laser emission show that at least 20% of the power put into the discharge is used to populate the excited $Pd^{4+}*$ states, that is to initiate processes resulting in nuclear reactions.

3) The L, M excitation of ions near the solid body surface spreads into the depth of the material with the speed of an electromagnetic wave. Deexcitations (relaxations) and conversion processes take place inside the cathode.

4) It is reasonable to assume that the excited L,M levels are of a dipole oscillation nature. They deexcite along the two competing paths:

a) direct transitions to the ground state resulting in the emission of X-ray photons.

$Pd^{4+}* \rightarrow Pd^{4+} + \text{X-ray}$

b) transfer of energy to one of the electrons of the inner shell (K, L, M) resulting in the ejection of this electron. The created vacancy is filled by an electron from an outer shell and a photon of a characteristic X-ray is emitted.

$Pd^{4+}* \rightarrow Pd^{4+} + e^{-} + \text{X-ray}$

5) High current density is responsible for the high rate of populating of L,M levels and thus for the creation of an active layer with the inverted population. Fast relaxation of long-lived excited dipole-oscillation levels of ions results in X-ray. This is possible when the population density of L,M levels is sufficiently high due to a sufficiently large current density and when amplification is large. Under such conditions a laser beam burst can be generated through a single pass across the population inversion region (without mirrors).

6) Thus each current pulse results in the excitation of L,M levels of the solid material. The density of population is n v-d (cm-3) and the characteristic L,M temperature, TLM is 1-2 keV (more than 20,000,000 K). The characteristic times of these excited isomeric states, tLM, can exceed 10 ms. Among the nuclear reactions taking place in such an environment one can possibly consider:

$^{105}Pd \rightarrow [Pd]* \rightarrow {}^{48}Ca + {}^{57}Fe + 15.976\ MeV,$

splitting of the excited Pd into two transmutation products, or

$^{105}Pd + {}^{2}D \rightarrow [Pd;D]* \rightarrow {}^{107}Ag + 13.124\ MeV,$

formation of the excited silver nucleus followed by its deexcitation. Experimental data seem to indicate that the released

nuclear energy does not appear in the form of kinetic energies of splitting fragments.

7) The probability of such reactions, and the corresponding rate of excess heat, are determined by the difference between the actual value of (nLM*tLM) and some minimum value of this product. This is analogous to the well-known Lawson criterion used to evaluate the excess heat in thermonuclear reactions. Note that the population density, nLM, depends on the design of the cathode and on the parameters of discharge plasma. The characteristic time, tLM, on the other hand, depends on the intensity of X-ray emission. To maximize the excess heat the population density must be as high as possible while the emission of X-rays must be reduced to increase the tLM.

8) It is reasonable to think that the excitation of L,M levels is transmitted into the solid material in the form of transverse waves. For the flat cathode geometry (see Figure 1) the deexcitation of L,M levels is responsible for the decrease in population density (nLM) with the depth of penetration. The rate of transmutation reactions should consequently decrease with depth. Such expectation seems to be confirmed experimentally; the concentration of reaction products decreases with the depth. The situation, however, may be quite different for a cylindrical or spherical geometry. In such cases one may create conditions in which the density of population, nLM, increases with the depth leading to a possible explosion.

9) It is conceivable that a solid cathode whose metastable levels have very long lives can be used as a heat storage cell. The storage capacity of such a cell can be as high as 2108 Joule/cm3.

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14) My Phys-L Message (December 2002)

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Do you remember the radio-talk file about CF that some of us downloaded after Larry Smith posted the address?

http://audio.kuer.org:8000/file/rw112702.mp3

One of the interviewed scientists was M. McKubre from SRI (Stanford Research Institute). I just finished reading the paper which he coauthored in 2000 and I am impressed. The paper can be downloaded as a pdf file from

http://lenr-canr.org/Features.htm

I admit that many details were not clear to me; the paper should be evaluated by electrochemists and material scientists. But the overall conclusions are very impressive. Palladium was loaded with deuterium by two methods: electrochemically and in a heated pressurized gas container. Control experiments were conducted using ordinary hydrogen. The previously reported anomalies (see below) were observed with deuterium but not with ordinary hydrogen.

1) Generation of heat (in excess of known sources).
2) Accumulation of ^{4}He (and some ^{3}He) atoms.
3) The isotopic ^{3}He/^{4}He ratio in accumulated products is orders of magnitude higher than in helium from air.
4) A nearly linear relation between the rates of excess heating and the rate of accumulation of ^{4}He.
5) The slope of the line shows that the amount of energy per atom is about 76% of what would be carried away by 23.8 MeV gamma rays in the hot d+d fusion.

Let me remind you that the absence of 23.8 MeV photons, in cold fusion, is a well established experimental fact. One of the authors, Peter Hagelstein from MIT, did write four papers (back in 1990?) on how energy can possibly appear in the form of crystal oscillations instead of photons. But no reference to these theoretical papers was made in his recent overview of the CF field at:

http://lenr-canr.org/index.htm

Referring to the 76% outcome the authors write: " Evidence for near-surface retention of ^{4}He in the lattice can be used to accommodate the discrepancy between measured and expected yields of ^{4}He." In other words, a sizable fraction of ^{4}He was presumably not squeezed out from the material in the process of analyzing it. Let me add that they used a mass spectrometer capable of distinguishing ^{4}He atoms from D_2 molecules.

My main question is why those who criticize CF usually refer to what happened in 1989 and not to recent work of

highly competent scientists? I suppose that many journals still consider CF to be pseudo-science and refuse publishing work done in that field. And that similar attitude exists among funding agencies. Is this a correct guess? I plan to attend the 10th International Conference of Cold Fusion next summer and find out. Information about the conference can be found at:

http://lenr-canr.org/Features.htm

By the way, in another paper the CF was referred to as the FP effect, to honor those who discovered it. Do you think that the field will be reevaluated for us, and for general public, by an appointed panel? They did it in 1989, why not now when so much more is known? Who should be expected to do this?

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15) Another Phys-L Message (December 2002)

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Let me also refer to the Technical ONR Report 1696, September 1995, coauthored by S. J. Szpak. The ONR stands for the Office of Naval Research. The report can be downloaded, as a very long pdf file, from

http://lenr-canr.org/Features.htm

In fact it is set of many documents that only chemists and material scientists will be able to comprehend completely. Browsing through this 146-pages file I made some notes, such as:

1) Back in 1991 ONR proposed a collaborative effort between NRAB, NWC and NRL (its research labs) to set up a coordinated CF experiment. The purpose was to investigate the anomalous effects associated with the prolonged charging of the Pd/D system. But this did not happen and each lab continued to investigate CF independently. The authors wrote: "In retrospect, imposing the condition of confirmation, rather than in-depth discussions, was unfortunate in view of what was known then and is known today, i.e. that the enormous complexity of the system and the unknown triggering mechanism make the reproducibility uncertain even if the procedure is followed rigorously."

2) Reproducibility became the focus of NRAB work. They invented the so-called "codeposition" process by which the FP effect (generation of excess heat) could be always observed. Instead of loading deuterium into preexisting Pd rods, they used an electrochemical process by which Palladium ions and D ions were simultaneously deposited in the form of a layer on a metal like copper or nickel.

3) They confirmed generation of tritium. The amount accumulated, together with generation of 2500 J of excess heat, turned out to be ten times higher than the background before.

4) Generation of soft x-rays was observed during the process.

5) Scanning electron micrographs of electrodes were made and correlated with other aspects.

6) X-ray crystallography was used during the FP process in a specially designed cell. The results were described in a report submitted on February 19, 1993.

7) The effect of current density on the rate at which heat was generated was investigated. Why should this kind of systematic work be called pseudo science? What does all this have in common with homeopathy or with N rays?

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16) THIS IS REAL PSEUDO-SCIENCE

DOWNLOADED FROM
http://www.genesisworldenergy.org/pressroom.htm

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HOW CAN AN EDUCATED PERSON TAKE SUCH NONSENSE SERIOUSLY?

Press Release

Contact: Diana Echeverria
Director of Public Relations
Guy Rome & Associates, Inc. 208-345-4143

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Scientific Breakthrough Liberates Energy Users from Fossil Fuel Dependence Technology breakthrough harnesses energy from the molecular structure of water.

BOISE, ID - Dec. 5, 2002 - Genesis World Energy, a privately funded consortium created by a group of military and space program research and development specialists, today unveiled a scientific breakthrough that allows consumers to easily access the energy contained within the hydrogen and oxygen molecular structure of ordinary water. This scientific breakthrough provides a limitless, low cost and environmentally clean source of energy that can be implemented with minimal cost and effort. The viability of using water as an energy source, previously a theoretical concept, is now a reality. "Water has always been the source of life on this planet, now it will also transform the way we create energy." said Charles Shaw, corporate counsel and spokesperson for Genesis World Energy. "The implications for worldwide energy generation and consumption are nothing less than staggering."

The Genesis Project

The technology is the result of nearly two years of continuous effort by a team of more than 400 visionaries from a wide spectrum of disciplines, including science, technology and engineering. Assembled as the "Genesis Project," the team discovered a way to extract far more energy from water than the extraction process itself required; differentiating this new fuel cell technology from any other development efforts thus far. The key component of the Genesis Project technology is the Genesis gCell, which through a series of electro-chemical processes separates hydrogen and oxygen molecules in water. The resulting gas molecules can either be burned cleanly as a replacement for natural gas or various other traditional forms of fuel, or may be remarried within Genesis eCells (an advanced fuel cell) to generate large amounts of electrical current. When both Genesis gCells and eCells are combined, the entire process becomes self-generating, recycling both water and electricity from the molecular reattachment phase. A more detailed description of the process can be found in the Genesis World Energy web site www.genesisworldenergy.org.

The Edison Device

The first application of this technology is represented in the "Edison Device," a self-contained, self-sustaining energy generation unit. Roughly the size of an outdoor air conditioning system, the power source can be quickly and easily installed in any home or business to provide virtually unlimited energy from any available water source. The Edison Device utilizes the existing electrical wiring and natural gas plumbing in a home or business to replace the energy provided by utility companies. The home version of the Edison Device produces approximately 30 kilowatts of combined gas and electrical energy per day. By comparison, the typical home uses between five to six kilowatts per day. The commercial model is capable of producing 100 kilowatts of energy per day. The energy generation portion of the devices has no moving parts. In fact, the only "mechanical" aspects of the equipment are small circulation pumps and micro-valves, making the Edison Device both silent and virtually

maintenance-free. A minimum amount of water is used over an estimated 20+ years of service life.

Market Ready

The commercialization of the devices is the responsibility of two business entities, Genesis World Energy and World Energy Management. Genesis World Energy is a technology development, production and supply consortium, while World Energy Management functions as the exclusive licensing representative for the worldwide distribution of Edison Devices.

According to Nejhla Shaw, World Energy Management President, "We will make Edison Devices rapidly available to governments and industries on a worldwide basis, with special licensing opportunities for those industries that will be most affected by the technology. For the first time in the history of the world, a clean and abundant source of renewable energy is as simple as the attachment of three wires, a gas line and a water hose."

BACKGROUND INFORMATION:

The Genesis Project

Creating an abundant, clean and renewable energy source from water

From The Source of Life, A New Source of Energy

Scientists and technologists have long explored viable sources of energy that would reduce the dependency of the world on fossil fuels and other non-renewable sources. Clean, low cost and abundant energy is, in many ways, the Holy Grail of the 21st Century. The economic, environmental and geopolitical implications of such a fuel source alternative are nothing less than staggering - and it is no exaggeration to say that these implications would extend to virtually every person on the planet.

One of the most promising energy technologies has been the generation of power using the earth's most abundant natural resource -- water. Just as water has been the source and sustainer of life on our planet, it also holds the key to transforming the way we generate energy. While generating energy from hydrogen and oxygen gases has been demonstrated in recent fuel cell technologies, none of these technologies have succeeded in creating a water-based energy source that can meet the demands of homes and businesses in a scientifically efficient and economically viable manner.

The Genesis Project

In September 2000 a privately owned and funded research and development organization with a half-century history of technology development for military and space programs decided to focus its resources on the barriers preventing the use of water as a total energy source. Assembling a team of more than 400 visionaries from a wide spectrum of disciplines, including science, technology and engineering, work groups were formed in a number of locations throughout the US. Code-named "The Genesis Project," these work groups engaged in a continuous research and development effort over an 18-month period. Only after the team's goals had been realized, were the details of what had been accomplished shared with everyone on the team. In June 2002, the goal of the Genesis Project was achieved: the creation of a low cost, self-sustaining process for generating unlimited energy from any available water source.

The Science Behind Genesis

Hydrogen and oxygen contained in water have always been the most attractive, yet elusive source of energy on the planet. Water molecules consisting of two parts hydrogen and one part oxygen are pure energy, yet the process of breaking water down into its molecular state consumed more energy than could be derived as a result. Genesis' breakthrough technology cracked the scientific secret to extracting far more energy from water than the extraction process itself required, making it possible to quickly and easily replace traditional forms of energy.

The gCell Process

The gCell is the fundamental component to the technology where three processes occur simultaneously. In the first of these processes, water is passed over catalytic reactants to produce an electrical voltage that excites the hydrogen and oxygen molecules. At the same time, a thermo, electro-catalytic reaction creates an effect similar to that of magnets with opposing polarities, separating the molecules into pure hydrogen and oxygen gases. In the third process, some of the hydrogen and oxygen molecules are reattached to generate additional electrical current that subsidizes the gas generation process - thus making the process self-sustaining. Since water is recovered and reused in the reattachment, gCells require only a few ounces of water per day. A single gCell stack, which is no bigger than a car battery, is capable of producing hundreds of cubic feet of customer usable gas per day. By comparison, the average American home in cold climate areas consumes approximately five cubic feet of gas per day.

The eCell Process

The eCell generates electricity and is one tool used in the conversion of hydrogen and oxygen contained in water into a consumer usable form of energy. The hydrogen and oxygen gases created in the gCells are used by eCells in a reverse reactant process that attracts the molecules much like magnets pulling themselves together. The resultant remarrying of the hydrogen and oxygen molecules produces, in a single eCell stack, more than 1,000 amps of electricity.

View the process here.

Bringing It All Together: The Edison Device

While the gCell and eCell technology is breathtaking in its simplicity, it needed to be incorporated into a mechanism that could viably meet residential and commercial energy requirements. This has been accomplished through the Edison Device: a self-contained energy generation system consisting of stacks of gCells and eCells. The Edison Device is roughly the size of a typical residential outdoor air conditioning unit.
Since the Edison Device utilizes the electrical wiring and natural gas plumbing in a home or business, installation is simple and quick. Three wires attach the Edison Device to existing electrical service connections, while the gas application requires connecting the Edison Device's gas feed line into the customer side of an existing natural gas line. Conversion of appliances from natural gas to hydrogen gas is accomplished by the use of inexpensive gas flow restrictors.

Market Ready

The Genesis Project has developed two market ready models of the Edison Device: a residential version and a commercial version. The residential model is capable of producing up to 30 total kilowatts of combined gas and electrical power per day (a typical home uses between five to six kilowatts), and the commercial model can generate up to 100 total kilowatts of energy. For heavier commercial requirements, multiple Edison Devices can be linked together.

The design of the Edison Device has proven that less is more. The energy-generating portion of the device has no moving parts - in fact, the only "mechanical" aspects of the Edison Device are the small circulation pumps and micro-valves that control the flow of water and gases. As a result, maintenance is limited to the occasional replacement of inexpensive water filters that can easily be accomplished by consumers themselves, while water usage is minimal over the device's projected 20+ years of service life!

The Ultimate Green Machine

Using only small amounts of water to meet residential and commercial energy requirements, producing no noise or emissions beyond the creation of ultra pure water, and utilizing an energy generation technology that is self-sustaining, the Edison Device is truly a "green machine." The Edison Device, and the underlying Genesis technology, represents a pivotal moment in human history: the promise of liberation from the shackles of fossil fuel dependency. The environmental impact alone is enormous. Limitless energy production without the consumption of fuels that deplete the ozone layer and pollute the planet. The reduction of drilling and mining for new energy sources. A more decentralized management and distribution of energy resources. Future applications will allow vehicles to operate on a pollution free basis.
And then, there are the economic considerations.

Rapid Deployment

The commercialization of the Edison Device is the responsibility of two recently created business entities: Genesis World Energy and World Energy Management. The first of these entities, Genesis World Energy, is a technology development, production and supply consortium. Its essential responsibility is to further the underlying Genesis Project technology and to establish and manage a roadmap for future product enhancements. The responsibility for the proliferation of the Edison Devices falls to World Energy Management, which is the exclusive licensing representative for the worldwide distribution of the Edison Device. This organization will make Edison Devices rapidly available to governments and industries on a worldwide basis through broad licensing agreements. These agreements will entitle licensees to assemble and distribute Edison Devices through the purchase of critical components from Genesis World Energy - allowing the technology originators to protect vital intellectual property.

Given the profound impact that the Genesis Project technology is likely to have on traditional producers and consumers of generated energy, World Energy Management will provide special licensing opportunities to those industries most affected by the introduction of the Edison Device. In the case of utility companies, for example, World Energy Management believes that the proliferation of Edison Devices will create a more stable and profitable business model. With the regulatory measures imposed on utility companies in the purchase and sale of energy, these companies would now have the option of charging the consumer a flat monthly rate for the rental of Edison Devices at a fraction of the cost of current power usage. While this would represent a smaller revenue stream, it would essentially be pure margin, since there would be no cost to the utility companies for the generated energy. Depreciation of the Edison Device equipment would also create a tax benefit. Consumers, for their part, would have the option of purchasing Edison Devices from other licensed manufacturers, thus forgoing monthly utility bills altogether.

An American Legacy

The scope and impact of the Genesis Project may be difficult to comprehend outside the context of a large and well-publicized government or industry initiative. Indeed, the efforts made by the Genesis Project to protect the anonymity of its members have, in part, been out of the desire to keep its technology firmly in the control of free market forces. Nevertheless, to view the Genesis Project as an unprecedented phenomenon is to ignore American history. Nearly a century ago, Thomas Edison brought together a small band of dedicated visionaries who, outside of any governmental agencies, taxpayer funded programs or corporate R&D structures, created technologies and products that transformed the world. The Edison Device is an heir to this uniquely American legacy of scientific inquiry, Yankee ingenuity, bold innovation and altruistic impulse.

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17) Nuclear side of cold fusion

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Let me summarize a paper of an Italian physicist T. Bressani "Nuclear Physics Aspects of Cold Fusion Experiments." It was presented at the Seventh International Conference on Cold Fusion in Canada (1998). The pdf file containing the article can be downloaded from < http://lenr-canr.org/Features.htm >

The author identifies **three questions** concerning cold fusion. They are:

1) How can Coulomb barrier be lowered between two deuterons to allow as many as about 10 to 20 fusion events per second (necessary to explain the actually measured excess heating rate.)

2) Why are branching ratios in cold fusion completely different from those in the free *(d + d)* fusion? Recall that the branch in which ^{4}He is produced is extremely rare in free fusion and that it is associated with the emission of 23.8 MeV gamma rays. The most common free fusion branches are: emission of neutrons plus ^{3}He, and emission of protons plus ^{3}T.

3) Do nuclear transmutation reactions take place in cold fusion, and why are their dominant byproducts stable rather than radioactive?

Before addressing the first issue the author refers to a study of Kasagi et al. in which palladium was bombarded with a beam of deuterium ions from an electrostatic accelerator. They found that the *(d+d)* cross section, measured down to 2.5 KeV, is 50 times larger when *d* is embedded in PdO than when in pure Pd or Ti! This experiment, writes Bressani, "is very important for at least two aspects. The first one, quite obvious, is that it is a dramatic proof of the **influence that condensed matter effects may have** on nuclear observables. The "Condensed Matter Nuclear Physics", whose first milestone is the Mössbauer Effect, may consider this experiment as the second milestone. The second aspect is that the enhancement of the cross section for *(d + d),* as measured following the method of Kasagi *et al.*, could be considered as one of the quality parameters needed in the choice of metals/compounds/alloys best suited to reach reproducible results in Cold Fusion Experiments."

The second issue, in my opinion, was not addressed, except by reviewing experimental results concerning light byproducts of cold fusion. These results confirm that dominant light byproducts are indeed very different from those resulting from free fusion. Experiments confirming the accumulation of 4He are described but the issue of 23.8 MeV gamma rays is not addressed.

Referring to **the third issue** the author writes: “I have always been absolutely skeptic about the possible existence of transmutations in Cold Fusion experiments. .....The experiment of Iwamura *et al.* 15 however is quite [promising]. Their cell, in which deuterium atoms are flowing through a Pd plate separating an electrolytic solution of D_2O/LiOD from a vacuum chamber, showed quite **strange phenomena**, like the production of Ti in quantities larger than the maximum contamination present in the samples and an isotope shift of the Fe atoms after the run.” I find this very interesting because the “isotope shift” **was also observed**, in a completely different setup, by Karabut et al.. I agree with the author that one should now be less skeptical about the reality of transmutation reactions.

It is probably too early to address the three basic questions formulated in this paper. We must first be certain that nuclear processes do take place in condensed matter without accelerated projectiles. On the other hand, blaming everything on “condensed matter” is not reasonable; nuclear reactions induced by accelerated projectiles nearly always occur in solid targets.

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18) It is not pseudoscience

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Look at the titles of these recent books; they are revealing.

1) C. Beaudette, "Excess Heat. Why Cold Fusion Research Prevailed." Concord, NH, 2000.
2) R.L. Park "Voodoo Science: The Road from Foolishness to Fraud," Oxford University Press, New York, 2000.

One reviewer wrote: “Professor Park does more than debunk, he crucifies... You'll never again waste time or your money on astrologers, quantum healers, homeopaths, spoon benders, perpetual motion merchants, or alien-abduction fantasists.”

But isn't "cold fusion" different from the above? I do not exclude the possibility that some "cold fusion" claims may have been fraudulent; con artists are naturally attracted to scientific controversies. But most of those who do research in the AE area ("anomalous energy" is a better term than "cold fusion') are likely to be honest. In fact, I suspect that Fleischmann and Pons might become Nobel laureates.

What makes the AE area different from voodoo science?

1) A large number (several hundred) of cooperating scientist in about 10 countries are actively involved.
2) Two Nobel laureates (Teller and Schwinger) were theorizing about AE at one time. Have they given up? I do not know.
3) Nearly all of the AE researchers have doctorates; many of them are (or were) associated with highly prestigious laboratories and universities. Many of them, including Fleischmann, are (or were) recognized leaders of disciplines.
4) These researchers organize one international conference each year and make results of their findings known to all who are interested.
5) Their methodology of validation is not different from that practiced by so-called "mainstream" scientists. They experiment, they hypothesize, they change their minds, they try to construct theories, they publish.
6) They are not secretive; they want to be heard and be criticized scientifically.
7) They want to have access (as authors) to all mainstream

journals in order to benefit from the peer-review process.
8) They want their proposals to be fairly evaluated by
NSF, DOE and other granting agencies.
9) They are highly unhappy about the "excommunication"
of the entire field caused by "heretical" mistakes made
by those who announced the discovery in 1989.

Is it not obvious that claims made by “astrologers, quantum healers, homeopaths, spoon benders, perpetual motion merchants, or alien-abduction fantasists” are completely different from those made by AE scientists?

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19) A Reference for Electrochemists

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I am not a chemist and I cannot fully appreciate the article of G. Lonchampt et al. presented at the sixth International Conference on Cold Fusion in 1996. The three French authors were from "CEA - Centre d'Etudes Nucleaires" and "Ecole Nationale Superieure d'Electrochimie et d'Electrometallurgie" These institutions impress me; they are "impeccable." The title of the paper was: "Reproduction of Fleischmann and Pons experiments." It can be downloaded (in the pdf format) from:

http://lenr-canr.org/Features.htm

Here is the abstract: "The objective of this work is to check the reliability of the initial Fleischmann and Pons calorimeter for studying cold fusion from ambient to boiling temperature. After describing our experimental set up, the assessment of excess heat from the enthalpy balance is discussed. We have observed deposits on the electrodes after electrolysis, which, in our opinion, have a determining role in the excess heat generation. We show raw data from three runs. It is concluded that this calorimeter is well adapted for such cold fusion investigation. "

This paper is significant for two reasons. First, the original report of F&P, was often criticized for being incomplete. The calorimeter they used was considered inappropriate by some critics. And second, F&P were working in France at that time. In fact their assistance is recognized at the end of the article. It appears that what was missing in the original publication can be found in the French article together with many additional comments. I hope this information will be useful to electrochemists interested in methodology through which the original discovery was made. Let me finish this short item by quoting the conclusions.

"Our experience during this last three years, leads us to conclude that the Fleischmann and Pons calorimeter is very accurate and well adapted to study cold fusion phenomenon. It is simple and precise. However precautionary measures must be taken:
a) the Dewar must be of excellent quality, i.e. good vacuum, in order to
eliminate heat losses by conduction . . ..
b) temperature calibration of the thermistors must be done very precisely.
c) all electrical feedthroughs must be sealed off in order to eliminate
spilling off of electrolyte by capillarity.

Our results concerning the relative excess heat (percentage of excess heat to enthalpy input) can be summarized as follows:

a) below 70°C, between 0 and 5%
b) between 70°C and 99°C, about 10%
c) at boiling, up to 150% especially in the final phase which appears
as the best condition to get a large amount of excess heat.

As already done by S. Pons, with ICARUS 9, it is necessary to operate at boiling on a permanent basis to obtain the most significant results.

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20) Ockham Razor

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I downloaded several "cold fusion" publications from the

http://lenr-canr.com/Features.htm

web site. One of them, written in 2000 and authored by Scott R. Chubb from Research Systems Inc., is an excellent summary of social aspects of the "cold fusion conflict" as it evolved since 1989. I strongly recommend this article to educated non-scientists. This is an introduction to a compilation of seven articles published by those who were personally involved in cold fusion research. Was the compilation published? I do not know. A theoretically oriented physicist may be interested in another paper on "cold fusion," coauthored by Chubb; it can be downloaded from the above web site as (ChubbSRtheoretica.pdf).

Here are the sins for which "heretical cold fusion" was excommunicated when it was only seven months old:

1) Naming the phenomenon "cold fusion" without having any evidence for it.
2) Secrecy, presumably imposed by patent lawyers and administrators.
3) Saying that the method was "very simple to reproduce."
4) Suggesting that commercial harvesting was just behind the corner.
5) Announcing the discovery via a "press release."

The date of the "premature birth of CF" was March 23, 1989. Evidence for the (D+D --> He) fusion is now available, but that fusion, if it is fusion, does not resemble the so-called 'hot fusion" in plasma. In my opinion the essence of the ERAB report (November 1989) can be reduced to "it is not fusion we are familiar with, because of this and that." Importance of additional research was actually emphasized in the report. I agree with those who feel that the field is ready for another evaluation. The baby is now 13 years old! Will it live or will it be allowed to die?

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21) On Reevaluation of AE Claims

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The decade of excommunication of the so-called “cold fusion” was not good for science and for its reputation. Why should anomalous energy (AE) be treated differently than any other area of interest? Nobody benefited from mutual accusations, such as “pathological science” or “pathological skepticism.” How can the scientific consensus about AE be reached? If I were an influential member of the Academy of Sciences I would call for the creation of two panels to reevaluate the entire field. One panel would consist of electrochemists and material scientists while the other would consist of experimental nuclear physicists. Each panel would have one well defined task, and a time limit, for example, two years, to accomplish it.

The chemists would be asked to answer one question: “is generation of excess heat real or not?” By definition, excess heat is thermal energy that can not possibly be due to chemical reactions. Physicists, on the other hand, would be asked a totally different question: “are chemically induced nuclear processes real or not?” Many claims have been made about the induced emission of 14 MeV alpha particles and of 3 MeV protons; this should not be difficult to either confirm or negate. The same applies to other processes, such as accumulation of tritium or big changes in the isotopic composition of some stable nuclides.

Confirmation of only one of these processes would be sufficient to validate a claim that a new phenomenon has been identified and that it should be studied. Non-confirmation, likewise, would be very significant. If the results reported by so many workers are not validated then the phenomenon of self-deception among hundreds of highly educated investigators would have to be examined. It would be an alert indicating a possibility that other areas of science might be in danger of being transformed into pseudoscientific “societies of mutual admiration.”

The modus operandus of the panels would be more or less similar to that established by ERAB, a panel created in 1989 to investigate the field of “cold fusion” two months after the excess heat discovery was announced. The members of the panel would travel to centers of AE research, observe experiments and be allowed to perform additional tests, if necessary. The burden of proof, naturally, would be on those who claim the phenomena to be real. The focus, at this early stage, would be on experimental facts and not on their practical or theoretical significance.

Selection of panel members would be made on the basis of qualifications. Every effort should be made to allow “cold fusion believers” to defend the claims, to repeat experiments, to use consultants, etc. But basic scientific methodology should be followed. Only recognized experts would be allowed to participate. Chemists, for example, would be asked to make sure that those who perform experiments, and those who critically evaluate them, have sufficient skill in electrochemistry, in metallurgy, in surface chemistry, in microanalysis, in spectroscopy, etc., according to what is being investigation. Without this neither negative nor positive conclusions can be trusted. Unfortunately, many self-appointed investigators of “cold fusion” in 1989 were amateurs whose conclusions were taken seriously.

The Academy of Sciences, acting on behalf of the entire scientific community, would agree in advance that the field of AE should be rehabilitated if at least one claim were validated. Failure to validate a single claim, on the other hand, would show that the field has not yet matured to the level of science. To rehabilitate means to deliver a general apology, in the name of mainstream science, for unfair treatments which occurred in the past. The purpose would be to make clear that research proposals in the area of AE are as desirable as in any other area of science. Naturally, I would not object if the reevaluation of the AE field were sponsored by another organizations, such as NSF, DOE, ACS or APS.

The reunification of AE with mainstream science, if it occurs, should not mean that every claim made under the banner of "cold fusion" is automatically validated. It should only mean that all claims should have an equal right to be

examined. The reunification would strengthen, rather than weaken, science and its respect in societies. Who is against it?

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22) Excess Heat and Helium Again

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To appreciate experimental difficulties associated with AE (anomalous energy) electrochemical experiments, one may be advised to read the paper of B. Bush and J. Lagowski. The paper was presented at the 7th International Conference on Cold Fusion (1998). It can be downloaded from:

http://lenr-canr.com/Features.htm

The paper describes two stages of a typical experiment. First is the preparatory loading stage (introducing a sufficient amount of D ions into the crystal structure of Pd), second is the excess heat generation stage. The loading may take several days and it is performed at law current densities, such as 1 mA/cm^2. To switch to the second mode the current density must be increased to about 20 mA/cm^2. This often, but not always, leads to generation of excess heat for as long as the cathode remains loaded. Some cathodes last much longer than others. It depends on the origin of palladium and on its surface was prepared.

The authors write: “Many laboratories have reported generating excess heat during deuterium oxide electrolysis at palladium cathodes, many of these reports being extremely convincing. The major difficulty with the topic of excess heat generation is not reproducibility, but rather control. The ability to control the circumstances of the electrolysis so that the excess heat can be ‘turned on, and turned up or down’ has eluded us as yet.” Let me add that, according to more recent reports, the reproducibility is now much higher than it used to be. Microscopic examinations of surfaces often helps to reject potentially unproductive cathodes.

A section on calorimetry is also instructive; it warns that some kinds of calorimeters are more suitable than others. The authors write: “thermal flux calorimeters based on Peltier devices (e.g. Melcor refrigichips) will fail in the most insidious fashion possible , several man years have been wasted on Peltier devices between the Navy and ourselves. We prefer to operate with the calorimeters submerged in a water bath, because this provides for reliable long term temperature stability. Further, because we are concerned with the helium produced by the excess heat generating reaction, we find it particularly useful to submerge the calorimeters in water, because helium is less soluble in water than in air and we can sparge the helium out of the water by bubbling liquid nitrogen boil-off gas through the enclosed water continuously to keep atmospheric contamination out.”

The section of the paper dealing with generation of helium is indeed very interesting. The authors write: “We entered this field by performing helium analysis as a nuclear products analysis. Our first effort was qualitative: 8 times during the generation of excess heat, helium was detectable in the electrolysis off-gas; 6 times when no excess heat was being generated, no helium was detectable in the electrolysis off-gas. This qualitative finding showed that the Pons and Fleischmann effect is a nuclear process occurring at the surface of the cathode. Subsequent quantitative helium versus energy analysis performed by ourselves,[2] and Dr. M. H. Miles working independently indicated that the excess heat and helium was produced via the D + D --> ^4He + 23.82 MeV(heat) reaction pathway.” I was particularly impressed by a demonstration that the observed $^4He/^3He$ ratio was found to be significantly different from that of helium in air. This

rules out a possibility that the progressive accumulation of He can be attributed to atmospheric contamination.

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23) Palladium Powder

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Another paper worth reading is that of Arata and Zhang; it can be downloaded from:
<P>
http://lenr-canr.com/Features.htm
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Recognizing that surface atomic structure plays a key role in the production of AE (anomalous energy) and that lattice imperfections (such as lattice defects, amorphous structures, intense local stress and micro cracks) are very important the authors decided to use a bottle-like palladium cathode filled Pd-black powder. The electrochemical process was used to supply deuterium to the bottle. Deuterium entered the "bottle" through its thin walls and interacted with powder particles. In the abstract of their article (Jpn. Acad., Ser. B, 1994. 70 ser. B: p. 106.) the authors wrote:

"Using this cathode, the authors confirmed the sustained production of a significantly abnormal amount of energy over a period of several months that could not be ascribed to chemical reaction energy. The chemical reaction energy of 0.1 [mol] Pd-black used is only 4[kJ], but more than 200[MJ] of excess energy was continuously produced for over 3000 [hr] at an average rate of 50-100 [kJ/hr] using a DS-cathode with a same quantity of Pd-black. Intermittent operation over a period of two years using this structure proved the complete reproducibility of these results."

I know of one person who is now trying to get excess heat from Pd-black in a totally different way. He is a recognized leader of great US projects and is highly qualified for that kind of study. The outcome is likely to be described at the 10th International Cold Fusion Conference this summer. I plan to attend the conference and will share what I learn there.

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24) Three Tutorials

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What follows are three introductory lessons on what used to be called cold fusion, plus one slightly more advanced lesson on the same topic. The authors are top scientists. The first is a professor from MIT, the other two are from the US government research laboratories. All three are of AE (anomalous energy) veterans from the year one. What follows are three introductory lessons on what used to be called cold fusion, plus one slightly more advanced lesson on the same topic. The authors are top scientists. I think that AE is a better term than “cold fusion” but some refer to it as LENR (low energy nuclear reactions) and CANR (chemically activated nuclear reactions). For more information go to < http://lenr-anr.org/index.html >
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It Started in 1989 . . .

by Peter Hagelstein

Many of us recall the controversy surrounding the announcement of claims of observations of fusion reactions in a test tube that were made in 1989. At the time, these claims were greeted with considerable skepticism on the part of the physics community and the scientific community in general.

The principal claim of Pons and Fleischmann

The principal claim of Pons and Fleischmann in 1989 was that power was produced in palladium cathodes that were loaded electrochemically in a heavy water electrolyte. The evidence in support of this was a measured increase in the temperature in the electrochemical cell. There was no obvious evidence for nuclear reaction products commensurate with the claimed heat production. Fleischmann speculated that perhaps two deuterons were somehow fusing to He-4 through some kind of new mechanism.

Rejection by the physics community

This claim was not accepted by the physics community on theoretical grounds for several reasons: First, there was no mechanism known by which two deuterons might approach one another close enough to fuse, since the Coulomb barrier prevents them from approaching at room temperature.

Second, if they did approach close enough to fuse, one would expect the conventional dd-fusion reaction products to be observed, since these happen very fast. Essentially, once two deuterons get close enough to touch, reactions occur with near unity probability, and the reaction products (p+t and n+He-3) leave immediately at high relative velocity consistent with the reaction energy released. To account for Fleischmann's claim, the proposed new reaction would seemingly somehow have to make He-4 quietly and cleanly, without any of the conventional reaction products showing up, and would somehow have to arrange for this to happen a billion times faster than the conventional reaction pathway. Most physicists bet against the existence of such a magical new effect.

Third, the normal pathway by which two deuterons fuse to make He-4 normally occurs with the emission of a gamma ray near 24 MeV. There was no evidence for the presence of any such high energy gamma emission from the sample,

hence no reason to believe that any helium had been made. Finally, if one rejects the possibility that any new mechanisms might be operative, then the claim that power was being produced by fusion must be supported by the detection of a commensurate amount of fusion reaction products. Pons and Fleischmann found no significant reaction products, which, given the rejection of new mechanisms, implied an absence of fusion reactions.

An alternate explanation is proposed

The physicists decided in 1989 that the most likely reason that Pons and Fleischmann observed a temperature increase was that they had made an error of some sort in their measurements. When many groups tried to observe the effect and failed, this led most of the physics community to conclude that there was nothing to it whatsoever other than some bad experiments.

The claim of Jones

A second very different claim was made at the same time in 1989 by Steve Jones. This work also involved electrochemistry in heavy water and the observation of reaction products corresponding to the conventional dd-fusion reactions. The initial publication showed a spectrum of neutron emission that Jones had detected from a titanium deuteride cathode loaded electrochemically. The response of the physics community was skeptical, as the signal to noise ratio was not particularly impressive. Given the polarization of the physics community in opposition to the claims of Pons and Fleischmann (which were announced essentially simultaneously), the physicists were not of a mood to accept much of any claims that fusion could happen in an electrochemical experiment at all. Jones went to great lengths to assure fellow scientists that his effect was completely unrelated to the claims of Pons and Fleischmann, and was much more reasonable.

Also rejected

Physicists had reason to be skeptical. Theoretical considerations indicated that the screening effects that Jones was relying on were not expected to be as strong as needed to account for the fusion rates claimed. As this experiment could not seem to be replicated by others at the time, it was easy for the physics community to reject this claim as well.

Cold fusion, weighed and rejected with prejudice

Cold fusion, as the two different claims were termed, was dismissed with prejudice in 1989. The initial claims were made near the end of March in Utah, and the public refutation of the claims was made at the beginning of May. It only took about 40 days for the physics community to consider the new claims, test them experimentally, and then announce loudly to the world that they had been carefully weighed and rejected.

Following this rejection, physicists have treated cold fusion rather badly. For example, Professor John Huizenga of Rochester University was selected to be co-chair of the DOE ERAB committee that met to review cold fusion and issue a report. Shortly afterward, he wrote a book entitled Cold Fusion, The Scientific Fiasco of the Century, in which he discusses the claims, the experiments, and the extreme skepticism with which the new claims were greeted. Robert Park discusses the subject in his book entitled Voodoo Science. You can find many places where physicists and other scientists happily place the cold fusion claims together with claims of UFOs and psychic phenomena.

COLD FUSION: What is it and what does it mean to science and society?

by Edmund Storms

Cold fusion is important because it promises to be a new source of pollution-free, inexhaustible energy. In addition, it is important because it reveals the existence of a new way nuclei can interact that conventional scientific theory predicts is

impossible. What then is this phenomenon that suffers such promise and rejection?

Energy can be obtained from the nucleus in two different ways. On the one hand, a large nucleus can be broken into smaller pieces, such as is experienced by uranium in a conventional nuclear reactor and by the material in an atom bomb. This is called fission. On the other hand, two very small nuclei can be joined together, such as occurs during fusion of deuterium and tritium in a Hot Fusion reactor and in a hydrogen bomb. This process, called fusion, also takes place in stars to produce much of the light we see.

The fission reaction is caused to happen by adding neutrons to the nucleus of uranium or plutonium to make it unstable. The unstable nucleus splits into two nearly equal pieces, thereby releasing more neutrons, which continue the process. As every one now knows, this process produces considerable waste that is highly radioactive. The uranium used as fuel also occurs in limited amounts in the earth's crust. As a result, this source of energy is not ideal, although widely used at the present time.

The normal hot fusion reaction requires two deuterium or tritium nuclei to be smashed together with great energy. This is accomplished by raising their temperature. However, this temperature is so high that the reactants cannot be held in a solid container, but must be retained by a magnetic field. This process has proven to be very difficult to accomplish for a time sufficient to generate useable energy. In spite of this difficulty, attempts have been under way for the last 40 years and with the expenditure of many billions of dollars. Success continues to be elusive while the effort continues.

Cold fusion, on the other hand, attempts to cause the same process, but by using solid materials as the container held at normal temperatures. The container consists of various metals, including palladium, with which the deuterium is reacted to form a chemical compound. While in this environment, the barrier between the deuterium nuclei is reduced so that two nuclei can fuse without having to be forced together. Because the process causing this to happen is not well understood, the possibility is rejected by many conventional scientists. Difficulty in producing the process on command has intensified the rejection. While this difficulty is real, it has not, as many skeptics have claimed, prevented the process from being reproduced hundreds of times in laboratories all over the world for the past 13 years. As you will see by reading the reviews and papers in our library, the process continues to be reproduced with increasing ease using a variety of methods and materials.

What is the nature of this process and why has it been so hard to understand? To answer this question, a person needs to understand the nature of the barrier that exists between all nuclei. Because all nuclei have a positive charge in proportion to their atomic number, all nuclei repeal each other. It is only the surrounding electrons that hold normal matter together, with the nuclei being at considerable distance from each other, at least on the scale of an atom. When attempts are made to push the nuclei closer, the required energy increases as the nuclei approach one another. However, when deuterium dissolves in a metal, it experiences several unique conditions. The surrounding metal atoms produce a regular array that is able to support waves of various kinds. These waves can be based on vibration of the atoms (phonons), vibration of the electrons, standing waves of electromagnetic energy, or a wave resulting from conversion of the deuterium nuclei to a wave. In addition, the high density of electrons can neutralize some of the positive charge on the deuterium nuclei allowing a process called tunneling, i.e.. allowing passage through the barrier rather than over it. The mechanism of this neutralization process is proposed to involve a novel coherent wave structure that can occur between electrons under certain conditions. All of these wave processes have been observed in the past under various conventional conditions, but applying them to the cold fusion phenomenon has been a subject of debate and general rejection.

While the debate based on wave action has been underway, people have proposed other mechanisms. These include the presence of neutrons within the lattice. Normally, neutrons are unstable outside of the nucleus, decomposing into a proton, an electron, and a neutrino. Presumably, this reaction can be reversed so that neutrons might be created in a lattice containing many free electrons and protons. Having no charge, the neutron could then interact with various atoms in the lattice to produce energy. These neutrons might also be hidden in the lattice by being attached to other nuclei in a stabilized form, to be released when conditions were right. Several particles normally not detected in nature also have been proposed to trigger fusion and other nuclear reactions.

While search for a suitable mechanism has been underway, an understanding of the environment that triggers the mechanism has been sought, the so-called nuclear-active-environment. Initially, this environment was thought to exist in the bulk of the palladium cathode used in the Pons-Fleischmann method to produce cold fusion. It is now agreed that the nuclear reactions only occur in the surface region. Recent arguments suggest that this surface layer does not even require palladium for it to be nuclear-active. Nuclear reactions have now been produced in a variety of materials using many methods. The only common feature found in all of these methods is the presence of nano-sized particles of material on the active surface. If this observation is correct, four conditions seem required to produce the nuclear reactions. First, the particle must have a critical small size; second, it must contain a critical concentration of deuterium or hydrogen; third, it must be constructed of certain atoms; and fourth, it must be exposed to a source of energy. This energy can take the form of a sufficiently high temperature, a significant high flux of hydrogen through the particle, application of energetic electrons or charged particles, or application of laser light of the proper frequency. Until, the importance of these factors is understood, the effect will continue to be difficult to replicate.

A Science Tutorial

By Talbot Chubb

First it is important to recognize that there are four distinct types of energy production:

1) chemical energy, that powers our cars and most of our civilization
2) nuclear fission energy, as used to generate about 15% or our electricity
3) hot fusion nuclear energy, which powers the sun and most stars
4) cold fusion nuclear energy, which appears as unexplained heat in a few experimenter's laboratory studies and which most scientists believe is impossible.

The three types of nuclear energy produce 10 million times as much heat per pound of fuel than occurs with chemical energy. How do these types of energy differ? To understand this question you need to know some chemistry and physics.

Lesson 1
Nature has provided us with two types of stable charged particles, the proton and the electron. The proton is heavy, normally tiny, and has a positive charge. The electron is light, normally large and fuzzy, and has a negative charge. The positive charge and the negative charge attract each other, just like the north pole of a magnet attracts the south pole of a magnet. When you bring two magnets together with the north pole of one facing the south pole of the other, they pull together, bang! When they bang into each other they release a little bit of energy in the form of heat, but it is too small an amount to easily measure.

To pull the magnets apart you have to do work, which is another way of saying you have to use up energy. It's almost like pulling a rock back up a hill. Rolling the rock down a hill actually creates a little heat, and pulling the rock back up the hill takes energy. In the same way the positive charge of the proton pulls on the negative charge of the electron and they stick together releasing energy in the process. The result is a hydrogen atom, designated H. A hydrogen atom is nothing but a fuzzy electron hugging a compact proton. The proton is the nucleus of the hydrogen atom. If you knock the electron off the hydrogen atom you get a positive ion H^+, which is nothing more than the original proton. An ion is the name applied to an atom or molecule that has lost or gained one or more electrons, hence is no longer electrically neutral.

Lesson 2
As you know, nature has provided us with more than one type of atom. We have oxygen atoms, nitrogen atoms, iron atoms, helium atoms, etc.. How do these atoms differ? The answer is that they all have different types of nuclei (plural of nucleus, from the Latin). And these different nuclei all have different numbers of protons inside them, which means they all have different plus charges.

The nucleus of the helium atom has 2 protons inside it, hence has plus 2 charge, and requires 2 electrons to neutralize its charge. When 2 electrons stick to it, it becomes a helium atom. The oxygen nucleus has 8 protons and has charge 8. When 8 electrons stick to it, it becomes an oxygen atom. The nitrogen atom has 7 electrons, and the iron atoms something like 26. But all the atoms are built more or less the same way, with a compact positively charged nucleus embedded in a cloud of fuzzy electrons. The difference in size between the compact nucleus and the fuzzy electrons is enormous. The sun has a diameter only about 100 times that of the earth. The electron cloud on an atom has a diameter which is about 100,000 times that of the nucleus. Cube these numbers to get the difference in volumes.

Lesson 3

We now are in a position to understand what chemical energy is. The atoms, all electrically neutral, can actually join with each other and release more energy. This is another way of saying that they can join into more stable configurations. The electrons in an atom try to configure themselves so as to get as close as possible to their nucleus, but their fuzzy nature requires that they take up a certain volume of space. However, if they join together with the electrons of another atom they can usually find a tighter configuration that leaves them closer to their beloved nuclei. For example, 2 hydrogen atoms can join together into a more compact configuration if each hydrogen atom contributes its electron to a 2-electron cloud, which the separate protons share. In this manner they form a grouping of the 2 electrons in a single cloud, together with the 2 isolated protons spaced apart from each other but still within the electron cloud.

The result is a heat-producing chemical reaction H + H => H_2. (The => means "goes to" or "becomes".) The H_2 configuration is the hydrogen molecule, and when you buy a tank of hydrogen gas, H_2 molecules is what you get. Furthermore, the 2 electrons of the H_2 molecule and the 8 electrons of the O atom can find a still more compact configuration by combining their electrons to create the water molecule H_2O, plus heat. The water molecule is really a single cloud of electrons in which are embedded the three point-like nuclei to form a minimum energy configuration. So when we burn oil or coal we reconfigure the electrons to produce more stable configurations of point-like nuclei embedded in electron clouds, liberating heat. So much for chemical energy.

Lesson 4

We have slid over one point in the above discussion. How does Nature make a nucleus containing two or more protons in the first place. After all, each of the protons has a positive charge, and the positive charges repel each other very strongly when they are separated by a tiny distance, equal to the distance across a nucleus. The repulsion of like charges is just like the repulsion between the north poles of two magnets when they are pushed together the wrong way. Something must overcome this repulsion, or else the only kind of atoms we would have would be those of hydrogen. Fortunately, this is not what we observe. The answer is that there is a second kind of force which acts on protons. This is the nuclear force. The nuclear force is very strong but requires particles to almost sit on each other to have any effect.

Also, there is a second kind of heavy particle, which is just like a proton, except that it has no positive or negative charge. It is not pushed away by the proton's plus charge. This other kind of particle is called the neutron, since it is electrically neutral. A peculiar fact of life is that it exists in stable form only inside a nucleus. When not in the nucleus it changes into a proton, an electron and a very light anti-neutrino in about 10 minutes. But it lasts forever inside a nucleus. Anyway, the neutron and the proton very strongly attract each other once they get close enough together, and then they combine to form a highly stable pair called a deuteron, which we designate D^+. The single deuteron, when it combines with a single electron, forms the heavy hydrogen atom called deuterium, designated D.

A second nuclear reaction occurs when two deuterons make contact. When they can be forced together so as to make contact, the 2 deuterons fuse, making a doubly charged particle. The grouping of 2 protons and 2 neutrons is even tighter than the proton-neutron grouping in the deuteron. The new particle, when neutralized by 2 electrons, is the nucleus of the helium atom, designated He. Larger groupings of neutrons and protons exist in nature and serve as the nuclei of carbon, nitrogen, oxygen, and iron, etc. atoms. All of these groupings are made possible by the very strong nuclear force, which is felt between particles only when they are in contact or share the same nucleus-size volume of space.

Lesson 5

We can now understand normal nuclear energy, which is really nuclear fission energy. During the early history of the

universe massive stars were formed. In the explosion of these massive stars, lots of different types of nuclei were formed and exploded back into space. Second and later generation stars and planets were formed from this mix, including the sun. In the explosion process probably every possible stable configuration of protons and neutrons was produced, plus some almost-stable groupings, such as the nucleus of the uranium atom. There are actually 3 different types of uranium atom nuclei, called uranium-234, uranium-235, and uranium-238. These "isotopes" differ in their number of neutrons, but they all have 92 protons.

The nuclei of all uranium atoms can go to a lower energy configuration by ejecting a helium nucleus, but this process occurs so rarely that the Earth's uranium has already lasted over 4 billion years. But the uranium nuclei are unstable in another way. In general, groupings of protons and neutrons are happiest if they have about 60 protons-plus-neutrons. The uranium nuclei contain more than three times this number. So they would like to split in two, which would release a lot of heat. But nature doesn't provide a way for them to split apart. They have to first go to a higher energy configuration before splitting in two. However, one of the three forms of uranium nucleus found in nature called uranium-235 and designated ^{235}U, gains the needed energy if it captures a neutron. The energized nucleus that results from neutron capture then splits apart with the release of an enormous amount of energy, and incidentally with release of additional neutrons. The additional neutrons can then split more uranium-235 nuclei, keeping the reaction going. This is what happens in nuclear power plants, where the heat, which is the end product of the nuclear splitting process, is used to boil water, generate steam, and turn electrical generators. (One also gets lots of radioactive products, which are a nuisance to dispose of safely.)

Lesson 6

We are now also in a position to understand hot fusion nuclear energy. As mentioned in lesson 5, the groupings of protons plus neutrons is most stable when the numbers of neutrons and protons approximate those found in the nucleus of an iron atom. Just as uranium has too many neutrons plus protons to be comfortable, so the light elements like hydrogen, helium, carbon, nitrogen and oxygen have too few. If the nuclei can be made to make contact under proper conditions, they can combine to create more stable groupings, plus heat. This is the process of fusion. Nature has found a way of doing this in stars like the sun. All Nature has to do is heat compressed hydrogen hot enough and wait long enough and hot fusion will occur.

If Nature were to start with deuterium, which already has a paired proton and neutron, the task would be relatively easy in a star. Temperature is a measure of how much speed an atom of a given type has as it bangs around inside a cloud of such atoms. The higher the temperature, the higher the speed and the closer the atoms get to each other momentarily during a collision. In a star the temperatures are high enough that all the electrons quickly get knocked off the atoms, so one is really dealing with a mixed cloud of electrons and nuclei. At very high temperature the nuclei occasionally get close enough during collisions for the pulling-together short range nuclear force to turn on. Then the nuclei can stick together and go to a lower energy grouping of protons plus neutrons, releasing heat.

Hot fusion nuclear energy is an attempt do carry out this process in the lab, using deuterium and mass-3 hydrogen (whose nucleus is a compact grouping of 1 proton and 2 neutrons) as the gas. Hot fusion requires that the gas be contained at temperatures of hundreds of millions of degrees, which can be done with the help of magnetic fields, but only for 1 or 2 seconds. The hope is to contain the gas for longer times. During the period of high temperature containment nuclear reactions occur during collisions. The main form of energy release is ejection of high energy neutrons and protons. The proton energy quickly converts to heat. The neutron energy can also be converted to heat but makes the equipment highly radioactive. It then becomes difficult to repair the equipment, which could make hot fusion a poor candidate for commercial power production. In any case hot fusion power is a dream that is still probably at least 50 years away. But most scientists view hot fusion as the only way to achieve fusion power. Hot fusion produces less radioactivity than fission power, is environmentally benign, and has a virtually limitless fuel supply on earth. (many millions of years at present energy usage rates).

Lesson 7

So now we come to cold fusion. Cold fusion may provide an easier and non-radioactive way of releasing nuclear fusion energy. Cold fusion relies on a different way of letting the protons and neutrons in one nucleus make contact with those in another nucleus, so that the nuclear force can bring them into a more stable configuration. The requirement for any

nuclear reaction to occur is that the reacting nuclei occupy the same volume of space. This condition is called particle overlap. In hot fusion particle overlap is brought about briefly by banging the nuclei together so as to overcome momentarily the repulsion of the two positive charges which try to keep the particles apart. In cold fusion particle overlap conditions are achieved by making deuterium nuclei act as fuzzy objects like electrons in atoms, instead of like tiny points.

When either light or heavy hydrogen is added to a heavy metal, each hydrogen "atom" occupies a position inside the metal where it is surrounded by heavy metal atoms. This form of hydrogen is called interstitial hydrogen. With interstitial hydrogen the electrons of the hydrogen atom become part of the pool of electrons of the metal. Each hydrogen nucleus oscillates back and forth through a negatively charged electron cloud provided by the electrons of the metal. They can be thought of as moving back and forth like the pendulum in a grandfather clock. This vibration exists even at very low temperature, due to a peculiarity of a branch of physics called quantum mechanics. The vibration is called zero point motion. The nucleus then becomes a fuzzy object, like the electrons in an atom. But this amount of fuzziness is not enough to permit a hydrogen nucleus to make contact with another hydrogen nucleus. To get two or more hydrogen nuclei to share the same volume one most go one step further. In a metal electrical current is carried by electrons that act more like vibrating matter waves than like point particles. If electrons did not become wave-like inside solids, there would be no transistors and no present day computers. This wave-like kind of electron is called a Bloch function electron.

The secret of cold fusion is that one needs Bloch function deuterons. One needs wave-like deuterons inside or on the surface of a solid in order that two or more deuterons share the same volume of space. But once the Bloch function deuterons are created, the nuclear force comes into play and the protons and neutrons making up the deuterons can rearrange themselves into the more nuclearly stable Bloch function helium configuration, with release of heat. To study cold fusion the experimenter has to force deuterons to assume the wave-like form and keep them in the wave-like state. Cold fusion experiments demonstrating release of excess heat show that this can be done. But at present no one knows how to do it reliably. Since cold fusion promises millions of years of energy without the problems of global warming or radioactivity, a real effort should be made to learn how.

For more tutorials go to www.hometown.aol.com/cffuture1 and www.hometown.aol.com/cffuture2

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25) A Technical Tutorial

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What follows is a technical tutorial prepared by Dr. E Storms, a retired scientist from LANL (Los Alamos National Laboratory).

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Technical Introduction to LENR-CANR

by Edmund Storms

At low energies, the Coulomb barrier prevents nuclei from coming together and fusing to form a single nucleus. To initiate a nuclear reaction, several methods are used. Nuclear reactions are normally initiated by pushing two atoms together with enough force to overcome the Coulomb barrier by brute force, or by using neutrons which penetrate the nuclei without seeing a barrier. (Neutrons have no electrical charge, so the Coulomb barrier does not stop them.) These forces are normally provided by a high-temperature plasma or by accelerating ions to high energies. In contrast, LENR describes the mechanism and conditions that cause a variety of nuclear reactions to take place with a relatively low activation energy. These unique conditions reduce the need for excessive energy. The normal method forces the nuclei together, while the new method encourages them to come together. The challenge has been to understand the unique characteristics of the necessary solid structure such that this structure could be generated at will.

Because the proposed method is unique, at odds with current nuclear theory, and is still difficult to reproduce, support for studies in many countries, but not all, has been very limited. Nevertheless, considerable information has accumulated over the last 13 years since Profs. Stanley Pons and Martin Fleischmann showed the world the possibilities inherent in this phenomenon. Much understanding is buried in conference proceedings and reports that are not available to a serious student. This information will, as time permits, be made available on this site. Students of the subject are also encouraged to use this site to interact with other people in the field and provide objective critiques of the work published here.

PHENOMENA DISCUSSED IN SOME OF THE PAPERS

At least 10 ways have been demonstrated to produce anomalous heat and/or anomalous elemental synthesis. A few of these methods will be described here. For course, not all of the claims are worthy of belief nor are they accepted by many people. Nevertheless, the claims will be described without qualifications in order to provide the reader with the latest understanding.

The most studied method involves the use of an electrolytic cell containing a LiOD electrolyte and a palladium cathode. Current passing through such a cell generates D^+ ions at the cathode, with a very high effective pressure. These ions enter the palladium and, if all conditions are correct, join in a fusion reaction that produces He-4. Initially palladium wire and plate were used, but these were found to form micro cracks, which allowed the required high concentration of deuterium to escape. Later work shows that the actual nuclear reaction occurs on the surface within a very thin layer of deposited impurities. Therefore, control of this impurity layer is very important, but rather difficult. The use of

palladium is also not important because gold and platinum appear to be better metals on which to deposit the impurity layer. This method is found, on rare occasions, to generate tritium within the electrolyte and transmutation products on the cathode surface. Different nuclear reactions are seen when light water (H_2O) is used instead of D_2O, although the amount of anomalous energy is less when H_2O is used. These observations have been duplicated hundreds of times in dozens of laboratories, as described in several of the review articles available on this website.

Application of deuterium gas to finely divided palladium, and perhaps other metals, has been found to generate anomalous energy along with helium-4. Both palladium-black as well as palladium deposited as nanocrystals on carbon have shown similar anomalous behavior. In both cases. the material must be suitably purified. Palladium deposited on carbon can and must be heated to above 200/260°C for the effect to be seen. When deuterium is caused to diffuse through a palladium membrane on which is deposited a thin layer of various compounds, isotopes that were not previously present are generated with isotopic ratios unlike those occurring naturally.

A plasma discharge under H_2O or D_2O between various materials generates many elements that were not previously present. When the electrodes are carbon and the plasma is formed in H_2O, the main anomalous element is iron. This experiment is relatively easy to duplicate.

Several complex oxides, including several superconductors, can dissolve D_2 when heated. When a potential is applied across a sheet of such material, the D^+ ions are caused to move and anomalous heat is generated.

If deuterium ions, having a modest energy, are caused to bombard various metals, tritium as well as other elements not previously present are generated. These ions can be generated in a pulsed plasma or as a beam.

When water, either light or heavy, is subjected to intense acoustic waves, collapse of the generated bubbles on the surrounding solid walls can generate nuclear reactions. This process is different from the fusion reaction claimed to occur within a bubble just before it disappears within the liquid because neutrons are not produced in the former case, but are produced in the latter case. This method has been applied to various metals in heavy water using an acoustic transducer and in light water using a rotating vane which generates similar acoustic waves.

HOW TO EXPLAIN THE CLAIMS

A major problem in deciding which model might be correct is the absence of any direct information about the nature of the nuclear-active-environment. At this time, two important features seem to be important, the size of the nanodomain in which the reactions occur and the presence of a deuterium flux through this domain. The domain can apparently be made of any material in which hydrogen or deuterium can dissolve. Until the nature of the nuclear-active-state (NAS) is known, no theory will properly explain the effect and replication of the claims will remain difficult.

When fusion is initiated using conventional methods, significant tritium and neutrons are produced. In addition, when other elements are generated, they tend to be radioactive. This is in direct contrast to the experience using low energy methods. These products are almost completely absent and, instead, helium-4 is produced. When radiation is detected, it has a very low energy. This contrasting behavior, as well as the amount of anomalous energy, has made the claims hard to explain using conventional models. This difficulty has been amplified by a failure of many skeptics to recognize the contrasting effect of the environment, a plasma being used in the older studies and a solid lattice of periodic atoms being present as the new environment.

Over 500 models and their variations have been proposed, some of which are very novel and some are variations on conventional ideas. Most models attempt to explain the nuclear reaction once the required environment has been created, without addressing what that unique environment might be like. These models involve conversion of a proton (deuteron) to a neutron (dineutron), creation of an electron structure that is able to neutralize the barrier, conversion of deuterium to a wave which interacts without charge, and the presence of otherwise overlooked neutrons and/or novel particles. Many of the models will have to be abandoned or seriously modified once the nature of the nuclear active

environment is understood.

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26) Reading the ERAB report again

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I have already written about the 1989 ERAB report -- a document prepared by the Energy Research Advisory Board for the DOE (U.S. Department of Energy). Detailed elaborations on various aspects of this report can be found in "Cold Fusion: The Scientific Fiasco of the Century," Oxford University Press, 2nd edition, Oxford, 1993. The author, John Huizenga, is a leading nuclear chemist from the University of Rochester; he was the chairman of ERAB. What follows is the executive summary of the report, as extracted from:

http://www.ncas.org/erab

Let me say again that, in my opinion, criticism of initial cold fusion claims was justified. There was no evidence that "excess heat" could easily be produced (even in a high school laboratory, as claimed by Fleischmann and Pons in March of 1989) or that the process producing excess heat was nuclear fusion responsible, for example, for solar energy. Most of those who now write about the so-called "cold fusion" agree that a high level of knowledge and skill are important to conduct research in that area. And nobody disagrees with the report main point that nuclear processes responsible for generation of anomalous energy (AE), if any, are very different from so-called "hot fusion." The report conclusion about the lack of convincing evidence for the practicality of AE also remains uncontested.

On the other hand, I think that it would be much better if the field was allowed to develop under ordinary conditions, for at least five years, before subjecting it to a special investigation by an officially appointed board. In my opinion, the report was not totally negative; it recognized the need for additional research and left the door open for possible later reconsideration. To make this clear I will underline significant phrases (see below) and insert short comments, using **BOLD CAPITAL LETTERS.**

* * * * * * * * * *

EXECUTIVE SUMMARY OF THE ERAB REPORT

As a result of the startling announcements in March 1989 by Utah scientists claiming the attainment of cold fusion, the Secretary of Energy requested (see Appendix 1.A) that the Energy Research Advisory Board (ERAB) convene a panel to assess the possibility of cold fusion. Since early May 1989, the Panel or subgroups thereof have participated in the Workshop on Cold Fusion in Santa Fe, have visited several laboratories, have studied the open literature and numerous privately distributed reports, and have participated in many discussions. The Panel meetings and schedule of laboratory visits are summarized in Appendix 1.B.

Since the above announcement, many laboratories worldwide have initiated research in cold fusion. In the United States, a major effort has been undertaken to search for cold fusion by a large number of research groups at university and national and industrial laboratories. Some laboratories support the Utah claims of excess heat production, usually for intermittent periods, but most report negative results. Those who claim excess heat do not find commensurate quantities of fusion products, such as neutrons or tritium, that should be by far the most sensitive signatures of fusion. Some laboratories have reported excess tritium. However, in these cases, no secondary or other primary nuclear particles are found, ruling out the known D+D reaction as the source of tritium. **IN OTHER WORDS, SOMETHING TOTALLY NEW MIGHT BE TAKING PLACE**.

The Panel concludes that the experimental results on excess heat from calorimetric cells reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. In addition, the Panel concludes that experiments reported to date do not present convincing evidence to associate the reported anomalous heat with a nuclear process.

Neutrons near background levels have been reported in some D_2O electrolysis and pressurized D_2 gas experiments, but at levels 10^{12} below the amounts required to explain the experiments claiming excess heat. Although these experiments have no apparent application to the production of useful energy, they would be of scientific interest if confirmed. Recent experiments, some employing more sophisticated counter arrangements and improved backgrounds, found no fusion products and placed upper limits on the fusion probability for these experiments, at levels well below the initial positive results. Hence, the Panel concludes that the present evidence for the discovery of a new nuclear process termed cold fusion is not persuasive.

The Panel also concludes that some observations attributed to cold fusion are not yet invalidated.

The Panel recommends against the establishment of special programs or research centers to develop cold fusion. However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system.

Following an introductory chapter, calorimetry, fusion products and materials are assessed in the next three chapters. Conclusions and recommendations are summarized in the final chapter.

CONCLUSIONS AND RECOMMENDATIONS

A. PREAMBLE

Ordinarily, new scientific discoveries are claimed to be consistent and reproducible; as a result, if the experiments are not complicated, the discovery can usually be confirmed or disproved in a few months. The claims of cold fusion, however, are unusual in that even the strongest proponents of cold fusion assert that the experiments, for unknown reasons, are not consistent and reproducible at the present time.

However, even a single short but valid cold fusion period would be revolutionary. As a result, it is difficult convincingly to resolve all cold fusion claims since, for example, any good experiment that fails to find cold fusion can be discounted as merely not working for unknown reasons. Likewise the failure of a theory to account for cold fusion can be discounted on the grounds that the correct explanation and theory has not been provided. Consequently, with the many contradictory existing claims it is not possible at this time to state categorically that all the claims for cold fusion have been convincingly either proved or disproved. Nonetheless, on balance, the Panel has reached the following conclusions and recommendations.

B. CONCLUSIONS

1. Based on the examination of published reports, reprints, numerous communications to the Panel and several site visits, the Panel concludes that the experimental results of excess heat from calorimetric cells reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion.

2. A major fraction of experimenters making calorimetric measurements, either with open or closed cells, using Pd cathodes and D_2O, report neither excess heat nor fusion products. Others, however, report excess heat production and either no fusion products or fusion products at a level well below that implied by reported heat production. Internal inconsistencies and lack of predictability and reproducibility remain serious concerns.

In no case is the yield of fusion products commensurate with the claimed excess heat. In cases where tritium is reported, no secondary or primary nuclear particles are observed, ruling out the known D+D reaction as the source of tritium. The Panel concludes that the experiments reported to date do not present convincing evidence to associate the reported

anomalous heat with a nuclear process.

3. The early claims of fusion products (neutrons) at very low levels near background from DO electrolysis and D_2 gas experiments have no apparent application to the production of useful energy. if confirmed, these results would be of scientific interest. Recent experiments, some employing more sophisticated counter arrangements and improved backgrounds, found no fusion products and placed upper limits on the fusion probability for these experiments at levels well below the initial positive results. Based on these many

negative results and the marginal statistical significance of reported positive results the Panel concludes that the present evidence for the discovery of a new nuclear process termed cold fusion is not persuasive.

4. Current understanding of the very extensive literature of experimental and theoretical results for hydrogen in solids gives no support for the occurrence of cold fusion in solids. Specifically, no theoretical or experimental evidence suggests the existence of D-D distances shorter than that in the molecule D_2 or the achievement of "confinement" pressure above relatively modest levels. The known behavior of deuterium in solids does not give any support for the supposition that the fusion probability is enhanced by the presence of the palladium, titanium, or other elements.

5. Nuclear fusion at room temperature, of the type discussed in this report, would be contrary to all understanding gained of nuclear reactions in the last half century; it would require the invention of an entirely new nuclear process.

RECOMMENDATIONS

1. The Panel recommends against any special funding for the investigation of phenomena attributed to cold fusion. Hence, we recommend against the establishment of special programs or research centers to develop cold fusion. **BUT ORDINARY FUNDING WILL BE NEEDED.**
2. The Panel is sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system. **IN OTHER WORDS, COLD FUSION IS NOT A SPECIAL AREA OF SCIENCE, IT IS SCIENCE TO BE SUPPORTED BY FUNDING AGENCIES.**

3. The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. Emphasis should be placed on calorimetry with closed systems and total gas recombination, use of alternative calorimetric methods, use of reasonably well characterized materials, exchange of materials between groups, and careful estimation of systematic and random errors. Cooperative experiments are encouraged to resolve some of the claims and counterclaims in calorimetry. **EXPERIMENTAL DATA OF THAT KIND ARE NOW AVAILABLE.**
4. A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for the production of fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels. **EXPERIMENTAL DATA OF THAT KIND ARE NOW AVAILABLE.**
5. Investigations designed to check the reported observations of excess tritium in electrolytic cells are desirable.

6. Experiments reporting fusion products (e.g., neutrons) at a very low level, if confirmed, are of scientific interest but have no apparent current application to the production of useful energy. In view of the difficulty of these experiments, collaborative efforts are encouraged to maximize the detection efficiencies an to minimize the background. **DO BASIC SCIENCE AND WAIT FOR THE RESULTS BEFORE PROMISING ANYTHING PRACTICAL. IN OTHER WORDS DO NOT REPEAT MISTAKES MADE IN THE AREA OF HOT FUSION.**

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27) Who Benefits from the Conspiracy?

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About a week ago a teacher from an Internet discussion list wrote that a conspiracy against "cold fusion," if its claims are correct, can not possibly be effective, in a long run. This made me think about the institutional conspiracy against the religious reputation of Galileo. Two days ago I got a private message on the topic of institutional conspiracy from Dr. Edmund Storms. He is the one whose letter to the editors of Scientific American was posted as item #9 on my web site:

http://csam.montclair.edu/~kowalski/cf/

The reply was a reaction to what I wrote in items # 26 and #21. Thinking that those who read my “cold fusion” items might be interested in the episode described by Storms I asked for (and received) permission to share it. What follows is his story, and additional comments. In reading the story keep in mind that DOE stands for the (US Department of Energy), ERAB stands for the highly negative 1989 report of the Energy Research Advisory Board and BYU stands for Brigham Young University where S. Jones was conducting research. Storms wrote:

You might like to know that in 1995, Steve Jones and I submitted a proposal to the DOE to test the claims of Pons and Fleischmann. I, as a believer, would show Jones and Hansen at BYU, as skeptics, how to make the effect work and they would measure the resulting energy.

We did this believing the DOE would abide by the statement in the ERAB report that "The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system". This proposal was turned down. As far as I know, every proposal having anything to do with cold fusion was also turned down. For all practical purposes, the ERAB report killed the field in the US no matter what they said to the contrary.

The message with the permission to post had a comment which, I suppose, can be added as an elaboration on the above. I wrote:

4) . . . The fact that DOE took a very hard stand in 1995 does not mean one should give up. Let us hope that 2003 will the year of the beginning of a reconciliation. . . .

Dr. Storms responded:
Like you, I hope for sooner rather than later. However, government bureaucrats and academics have invested so much in rejecting this idea that it is not possible for

them to change. I expect the US will change only after Japan solves the problem and threatens to create a commercial product based on the process. Meanwhile the old will die off and administrations will change, allowing new people to take control of science. It is a very slow process to make such profound changes. Being retired, I look upon this as an interesting process with very little likelihood of an end any time soon.

Hmm, more than 1000 scientific papers supporting the reality of highly unusual phenomena are available but the leaders of our scientific establishments refuse to have another look. Something is not right. What should a confused science teacher do? Avoid the topic because authorities declared it to be non scientific 13 years ago? Risk his or her reputation and try to discuss the issue objectively? Play it safe and support official pronouncements? Those invited to look into Galileo's telescope were in a similar situation.

The more I think about it the more I am convinced that something similar to what was suggested in item #21 (on my web site above) is urgently needed. Read again what Dr. Storms wrote in the letter to the editors of Scientific American (item #9 on my web site) and think about it critically. Is he right or is he wrong that the issue is important in the context of support for science in our society?

By the way, a TV program last night was devoted to illnesses. They produced an example of institutional conspiracy against a researcher. The man had data proving that children's exposure to lead (mainly from gasoline emission) affected mental functioning. But the powerful lead industry launched an attack against him, and tried to discredit him. It took three years to show that his claims were not pseudo-science. Lead was removed from gasoline and its concentration in air has been reduced significantly. The motive of conspiracy, in this case, was obvious. But what motivates the DOE? Why was "every proposal having anything to do with cold fusion ... turned down" by our own government? Why do they ignore hundreds of serious papers authored, mostly (I assume) by highly trained Ph.D.. scientists? Is the scientific establishment trying to protect us from some dangers? Why do they oppose a fair examination of the AE claims, in view of new evidence? Despite its criticism, which has been mostly justified, the ERAB report was “sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system." Is it true that such support has not been available to reputable US scientists? Why not?

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28) A summary of another recent report

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Do you remember the article of A.B. Karabut which I translated in item # 13? He showed that generation of excess heat in a gas discharge tube (in which a Pd cathode was bombarded by ~ 1KeV D ions) was accompanied by emission of highly unusual alpha particles (14 MeV) and protons (3 MeV). Such particles were also observed in an electrochemical setup producing excess heat, as reported by A.G. Lipson et al. at the 9th International Conference on Cold Fusion (May, 2002, Beijing, China). The report can be downloaded from the library at the LENR-CANR site:

http://lenr-canr.org/Features.htm

Before summarizing the paper let me mention two prestigious laboratories with which the authors are affiliated. One lab is at the Department of Nuclear, Plasma and Radiological Engineering at the University of Illinois at Urbaba-Champaign and another is at Lebedev Physics Institute in Moscow. The authors refer to an earlier study (2001) in which alpha particles and protons were found to accompany generation of heat from the "Au/Pd/PdO:D(H) heterostructure." Alpha particles with energies between 8 and 14 MeV were identified with a set Si detectors, after the Pd cathode was removed from the electrolyte. In this study, on the other hand, the emission of unusual nuclear particles was observed during the electrolytic loading. Use of Si detectors in the liquid electrolyte would be very difficult and a different method of detection was used to identify particles, and to estimate their energies.

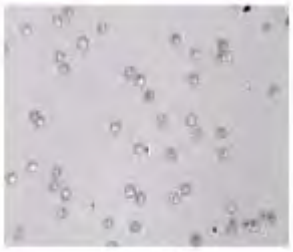


Figure 3

Note:
Figures are numbered
as in the original paper.

That method is often used to detect alpha particles (energies below 8 MeV) emitted from radon and thoron, present in air, and from their descendants. A special plastic material, commercially available under the

name of CR-39, is exposed to alpha particles and then etched in hot NaOH for several hours. After that procedure the plastic sheet is examined under the microscope. It turns out that places damaged by alpha particles become visibly pits, as shown in Figure 3 below. Thus, the number of intercepted particles can be determined by counting the number of pits per unit area. By using alpha particles with energies between 2 and 29 MeV the authors established a relation between the pit diameters and energies, as illustrated in Figure 1. A similar curve was obtained for protons with energies between 0.7 and 3 MeV. Beams of alpha particles and protons, used to calibrate the CR-39 detectors, were produced with accelerators in Dubna, Russia.

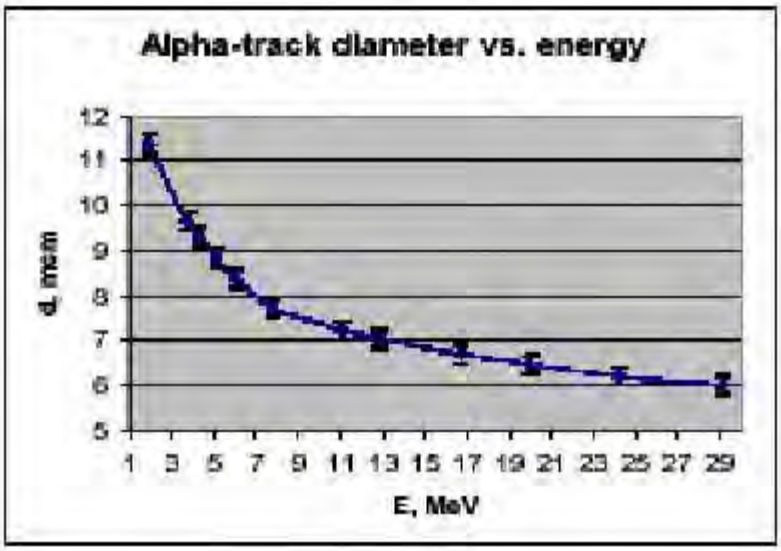


Figure 1

In the electrolysis experiments some sheets of CR-39 were in contact with the Pd cathode while others were kept away from the cathode to measure the background. As expected, background sheets revealed presence of a small number of alpha particles the radon and thoron series. Plastic sheets designed to detect protons were coated with Cu foils whose thickness was 25 microns. Such foils stop all alpha particles below 9 MeV but transmit protons above 2.3 MeV.

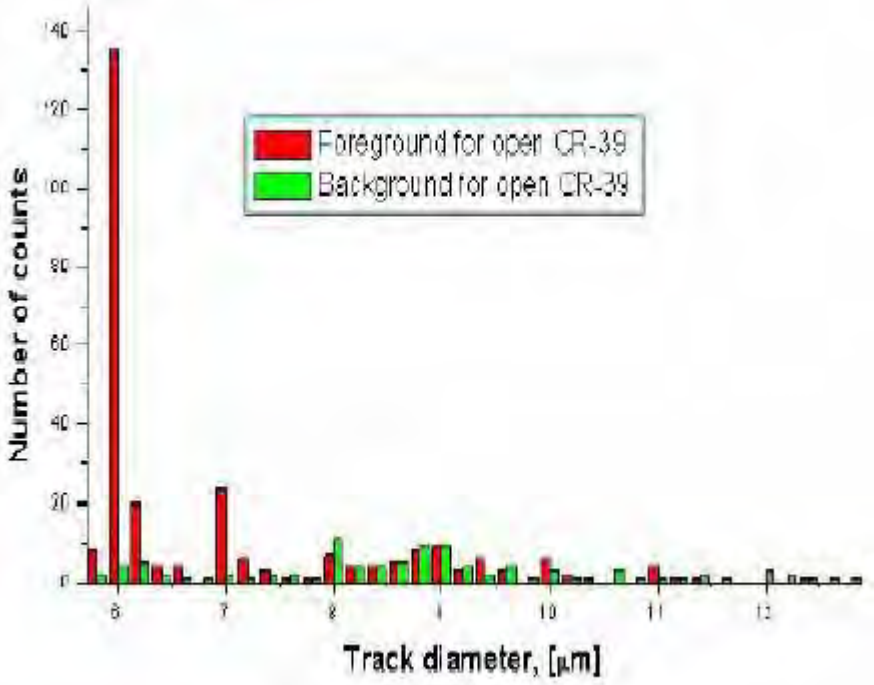
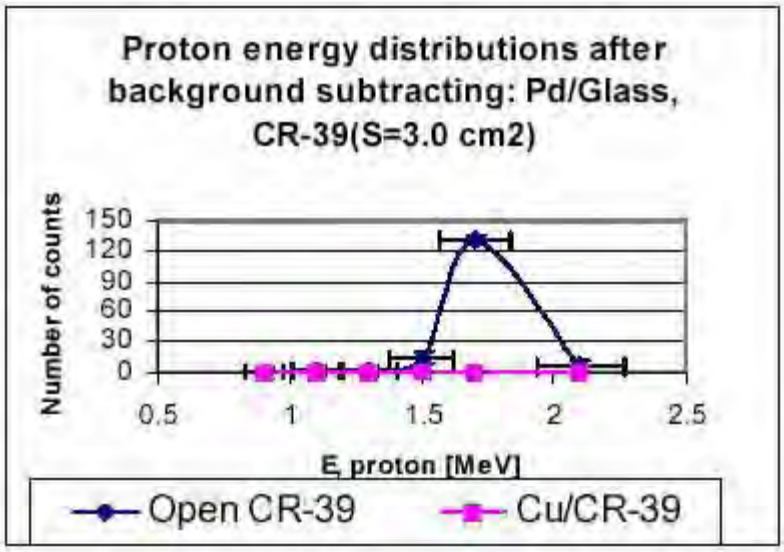


Figure 6

The results, for the CR-39 sheets which were not coated with copper, are shown in Figure 6. Red bars refer to sheets which were in contact with the cathode while green bars refer to sheets measuring the background. It is clear that in the area of small pits (high energy alpha particles and nearly all protons) the background is very small in comparison with the emission from the cathode. The mean energy of dominant alpha particles was close to 14 MeV while that of protons was close to 1.6 MeV. The estimated emission rates were about 7 (per m^2 per second) for alpha particles and about twice as much for protons. The energy distribution of protons is shown in the next figure.



The authors conclude that the "observed effect of long-range alpha and proton emission cannot be described in terms of nor natural alpha-nuclides contamination in Pd neither the cosmic ray interactions with cathode." I was really impressed; the study does confirm the reality of an unusual nuclear process. It made me think about the old Geiger-Nuttall rule (the inverse correlation between the energy and half-life, later explained in terms of quantum mechanical tunneling). I think that the above observations are sufficiently important to justify building a special Si detectors telescope with a thin window and using it

instead of CR-39, in the liquid.

How do researchers who conducted this study differ from mainstream scientists? Would the DOE or NSF approve a research grant seeking to further investigate the unexplained phenomenon?

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29) Another manifestation of excess heat

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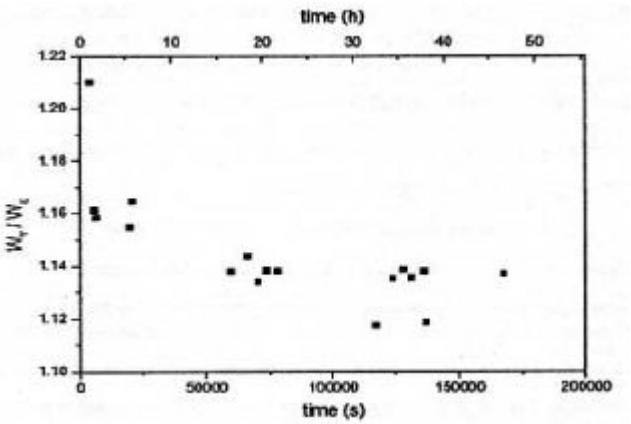
A strange device for generation of excess heat was built by Jack Dufour et al. from the Laboratoire Des Sciences Nucleaires at CNAM in Paris. The device was described at ICENES2002 (11th International Conference on Emerging Nuclear Energy Systems in Albuquerque, NM, October 2002.) It was a copper pipe inside a steel pipe with some Sn/Pb solder between them (70% lead). The length of each pipe was about 17 cm. The outer diameter of the copper pipe was 14 mm while the inner diameter of the steel pipe was 14.2 mm.

The set of two pipes was used as a conductor through which a known electric current was allowed to flow from a dc generator. The potential difference, typically 5 V, across the pipes, as well as the current, were constantly monitored. The amount of heat produced in the pipes was measured by using a flow calorimeter. Water was flowing through the copper pipe, at a measured rate, and the temperature of pipes was allowed to reach a constant value (typically one hour) . The water temperature was measured at the input and at the output. The calorimetric parameters were then used to calculate W_t, the rate at which heat was generated. It turned out that W_t was higher that the rate at which electric energy was supplied, W_e (for example, 12 W at 5 V).



The excess heat ratio, defined as $p=(W_t/We)$, was found to increase with the magnitude of the electric current. At the highest current used in the experiment the value of p was 1.21. A progressive decrease of p, attributed to a decrease of current during a two-days-long experiment, is illustrated in the figure. The values of p, in a steady state, are always larger than unity; the excess heat was never smaller that twelve percent of supplied energy.

My description of the apparatus was slightly simplified; the outer surface of the steel pipe was also cooled by circulated water and temperatures were measured in several different locations (to determine gradients). Furthermore, the system was equipped with a calibration solenoid; it was used to heat the pipes inductively rather than ohmically. The authors claim that the excess heat is not an artifact due to a systematic error; I

am taking this for granted. What else can one do without having a possibility to examine the apparatus, and the procedures used to evaluate W_e and W_t? Speculations about the nuclear origin of excess heat were presented but I am ignoring them because no evidence was offered. This, however, should not be a justification for rejecting what has actually been established -- the excess heat.

In one experiment, lasting several days, the excess heat was 520 kJ. The authors argued that the oxidation of metals, which were only slightly tarnished, or any other chemical process, could not possible be responsible for a significant contribution to excess heat. The pipes would have to be 91% oxidized to release 520 kJ of chemical energy. Tarnishing, however, indicates that the oxidation affected only a small fraction of one percent of metals. That is why the chemical origin of the anomalous energy was rejected by the authors.

My summary is based on what has been published in the ICENES2002 proceedings (pages 367 to 373). The e-mail address of Jacques Dufour, listed in the proceedings, is dufourj@cnam.fr I attended the conference and had a chance of talking with Jacques after his presentation. He said that the attitude of French mainstream scientists toward the so-called “cold fusion” field is becoming more and more positive.

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30) American Physical Society Guidelines

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The January 2003 issue of Physics Today has an interesting item entitled "New APS Ethics Guidelines Address Research, Misconduct and Professional Responsibilities." The author, Jim Dawson, summarizes main points of the **new ethics guidelines** recently adopted by the American Physical Society (APS) panel on public affairs. I was particularly interested in this segment:

"You should hang on to your data, you should respond to inquiries from other scientists, and you should be responsible as a referee. . . The new guidelines, approved on 10 November, come in several parts. A policy statement on how to handle allegations of research misconduct defines misconduct as 'fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results. .....' Such behavior is termed an 'egregious departure from the expected norms of scientific conduct that can lead other scientists along fruitless paths.' It also 'diminishes the vital trust that scientists have in each other' and undermines public confidence in science. The statement goes on to say, 'It is imperative ..... that the institutions responsible for funding and performance of scientific research, as well as the relevant professional societies, take appropriate steps to discourage such conduct and have policies and procedures in place to deal with allegations of misconduct.' "

I find these remarks encouraging; they indicate that APS wants to defend fairness. But how should one interpret the above in the context of the so-called "cold fusion" issue? Considering what has been accomplished in that area (hundreds of scientific reports from recognized experts in several countries) I would like to suggest that the APS initiate the process of reevaluation of the entire field. The 1989 ERAB report was correct in stating that no convincing evidence existed to support the premature claim of Fleischmann and Pons that a new source of useful energy had been discovered.

But the situation has changed in the last ten years. Scientists conducting research in this area have accepted the criticism; they no longer claim that demonstrations of excess heat are easy (as initially announced) or that the underlying mechanism is simply a fusion of two nuclei, as in hot plasma. But they "hang on to data" indicating that something new was discovered, but not understood, in 1989. I see no evidence that the data were "fabricated" or that the methodologies they now use constitute a "departure from the expected norms." Is it true that editors of many peer reviewed journals automatically reject manuscripts dealing with "cold fusion?" Is it true that "institutions responsible for the funding and performance of

scientific research," such as DOE and NSF, automatically reject research proposals dealing with "cold fusion?"

I have heard such allegations from several "cold fusion" scientists. They claim that the entire field has been blacklisted in the US. If this is true, then, in my opinion, the situation should be reviewed in light of new ethical guidelines. Those guilty of falsifications should be exposed as pseudo-scientists while those who made "honest errors" should be criticized, as in any other field of science. And those whose claims are accepted as valid should be rewarded (in the form of published papers and financial support) as in any other area of science.

As a physics teacher I am confused by the situation. What should we tell students when they ask about the discovery of Fleischmann and Pons? Most teachers have no time and no means to validate claims made in the area of "cold fusion," and need guidance. An objective summary of what has been done in that field in the last ten years would help us to describe it correctly. The issue is not only scientific; it is a topic of general interest.
Most educated people know about the "cold fusion episode" and opinions about it are divided. Some say it was "a fiasco" while others say it was a "real discovery" of something very important. How should teachers address this topic in the context of "public affairs between science and society," or in the context of "institutional support for new ideas and innovations?"

PS (April 29, 2003):
Last January, immediately after Dawson's article was published, I sent a letter to the editor to Physics Today. It was a suggestion to reevaluate the field of cold fusion (for the benefit of teachers). The letter linked this issue with "new ethics guidelines" adopted by APS. I have not heard from the editor; will they publish my letter? Will they write to me explaining why the letter was not published or will they simply ignore me? I also wrote a note about cold fusion to The Physics Teacher. The editor did sent it to two referees and informed me about their advice for not publishing the note. I will wait another month or two. After that my letter to the editor of Physics today will be appended here.

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31) My own excess heat experiments

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There was no evidence for "excess heat" in my experiments. That is the bottom line. And here are some details. Inspired by the paper of Jacques Dufour (see item #29) I decided to conduct my own search for excess heat. Is it conceivable that excess heat is more common than we think? How do we know that a TV set, for example, produces as much heat as the amount of electric energy it receives in any specified time interval? In principle this can be verified by measuring both the heat released, W_t and the electric energy received, W_e. But I doubt that anyone has ever bothered to accurately measure the W_t released from a TV set. We just know that it should be the same as W_e. The equality of W_t and W_e is often verified in an introductory physics laboratory by using the so-called electric calorimeter and a metallic resistor. I do not know of a similar test performed by using a resistor made from a composite material.

Such tests were probably performed many times. Instead of looking for a reference I conducted my own simple experiment. I took a radio resistor (nominally 10,000 ohms but in reality 9,720 ohms, at room temperature) and placed it into a common water-based student calorimeter. The resistance in water dropped immediately, indicating that some current was flowing through the water. To prevent this the metallic leads of the resistor were coated with a nonconductive glue. I was surprised to find that W_e and W_t were identical to within 0.1%; the accuracy of my measurements was at the level of about one or two percent. But, as fortune cookies tell us, one can be lucky on some days.

This made me think of a possible physics research project for a group of high school students. The topic can be introduced by saying that "scientists claim that excess heat can be generated in some electrical setups and that not all conceivable setups have been tested." This would give students an opportunity to learn about the history of the so-called "cold fusion" and lead to more sophisticated experiments in which W_t is said to be larger that W_e. I know of one scientist who is working on a document describing elementary excess heat experiments. He said that laboratory instructions should be ready at the end of this month. Such materials would be of great value to us. I would be happy to post his pamphlet as an item on this web site for teachers interested in "cold fusion."

I will, of course, try to perform the experiments myself. This would give me an opportunity to be in a much better position to argue about the non reality or reality of the so-called "anomalous energy." If I am convinced, on the basis of my own measurements, that the excess heat is real then I would be more interested in theoretical explanations which are being offered to explain it. I would also become more optimistic about the prospect of using nuclear energy without producing dangerous nuclear waste products. Inspired by their own observations students are likely to think in the same way. And who knows, perhaps a biography of another Marie, in the next century, will begin by describing a high school physics project which inspired her. Will the scientist's name be Marie or will it be Thomas?

P.S.
Another conducting material tested for the hypothetical “excess heat’ was carbon-impregnated paper available from Pasco Scientific. That paper is often used by by students to model electrostatic fields. I allowed a direct current of about 50 mA to pass through a flat 3.2 kiloohms resistor inside a calorimeter for 40 minutes. The amount of heat released did not differ from the electric energy supplied by more than 3%; the expected sensitivity of the experiment. A student project of that kind can be very instructive.

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32) Another case

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In reading anomalous energy (AE) publications, available over the Internet, I keep asking myself the same questions. Why do many scientists still claim that the AE field is mostly nonsense? What makes the AE publications different from those found in mainstream journals? Why is the status of the AE not reevaluated in light of new data? Will the new APS Ethics Guidelines (Physics Today, January, 2003, p20) change the attitude of individuals and organizations toward a field which seems to be interesting and important? The article which prompted me to ask these questions again was written in 1999. The title is "My Life With Cold Fusion as a Reluctant Mistress. " You can find the article on the web site of E. Storms:

http://home.netcom.com/~storms2/index.html

Even those who are not prepared to deal with some technical details (like myself) will appreciate the main points, and interesting reflections of a highly competent scientist. I strongly recommend this article to all those who are interested in AE. Storms writes that "the phenomenon called 'cold fusion' has been duplicated hundreds of times in laboratories throughout the world and the subject has been discussed in over 3000 papers . . . Unfortunately, because of the rejecting attitude of conventional scientists, much of this information is not available in scientific journals." After listing previously unrecognized metallurgical factors, influencing either success of failure of an AE experiment in the electrolytic PdD systems, Storms writes:

"What can I conclude from this experience? First, the phenomenon claimed by Pons and Fleischmann is real, but it is only a small part of a much larger picture. The reality of this phenomena has an even greater importance to science and technology than was ordinarily proposed. Second, the method used by Pons-Fleischmann is useless for eventual production of commercial power. Active palladium is too difficult to find and conditions are too sensitive to impurities. Nevertheless, it is a very useful and inexpensive method to explore certain aspects of the phenomena. . . . And third, the field is sick and on life support. . . .

The amazing claims for [nuclear] transmutation are getting increased attention and are accumulating experimental support, but acceptance is hard to find even in the cold fusion community. These claims require expensive tools to show their reality - money that is not generally available. The only solutions are for the Patent Office to change its approach and/or for individuals or companies to provide funds toward a basic understanding without an immediate guarantee of financial return; several very unlikely possibilities."

By the way, Dr. Edmund Storms is a radiochemist who worked on important projects at Los Alamos

National Laboratory for thirty-four years. He is a recognized authority in the field of material science, as reflected in several books, and in over seventy research publications. But even a person of that caliber was not able to publish an important paper in Physical Review, in Review of Modern Physics and in two other journals. The PR editor rejected the paper because “we do not publish reviews” while the editor of RMP wrote “Cold fusion is a classic example of pathological science. I will certainly not publish articles supporting its disproven claims.” Apparently this is a typical case; AE scientists are not given a chance of sharing their data with the rest of us. We are being protected from them by editors and administrators. Something is not right with this.

The title of the rejected 2001 paper was: “Cold Fusion: an Objective Assement.” It is avaialble as a downloadable file: StormsEcoldfusionc.pdf at

http://lenr-canr.org./Features.htm

Why was this paper labeled “pathological science?” Read about the new APS Ethics Guidelines; they do address the issue of professional responsibilities. (The article summarizing the guidlines, as indicated above, can be found on page 20 of the January 2003 issue of Physics Today.)

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33) Julian Schwinger and cold fusion

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1) Let me insert the entire message e-mailed to me be Edmund Storms; I only quoted from it last night (see item 31). Referring to my inquiry into unjustified rejections of "cold fusion" papers Storms wrote:

REJECTION HISTORY OF THE PAPER "COLD FUSION: AN OBJECTIVE ASSESSMENT" by Edmund Storms

A paper called "Whatever Happened to Cold Fusion" was submitted to the following journals as a proposed paper. The full text paper was offered if interest were expressed. No requests were made. Submission was done in series, with a new journal chosen after a rejection. During this process, the paper was completed and retitled "Cold Fusion: An Objective Assessment". The purpose of this exercise was to discover whether a scholarly review of cold fusion could be published in a major scientific journal and the reasons why not if it were rejected. When objections were raised about the reasons for rejection, these were not answered by the rejecting editor.

Journal	Submitted	Rejected	Reasons
Phys Rev. B	6/10/01	7/11/01	Said the Phys. Rev. does not publish reviews, although this is not true if the subject is to their liking.
Rev. Modern Phys.	7/25/01	8/10/01	Editor rejected the paper with the comment "Cold fusion is a classic example of pathological science. I will certainly not publish articles supporting its disproven claims."
Chem. Rev.	8/24/01	11/20/01	Three of four reviewers rejected the paper because they did not think cold fusion is real and could not trust me to be unbiased in arguing this belief.
J.Electroanal.Chem.	11/23/01	12/3/01	Editors did not think the subject was appropriate

to the journal.

2) How would Richard Feynman react to the announcement of the so-called “cold fusion” if he did not die a year earlier? I suspect he would not reject an experimental discovery on the basis of the absence of a theoretical explanation. He would probably do the same things, more or less, as Julian Schwinger, his 1965 Nobel prize colaureate. Schwinger’s two addresses on the subject can be downloaded as pdf files from the library at:

http://lenr-canr.com/Features.htm

In one of the addresses the great theoretician wrote: “It may not be too much of an exaggeration to say that, early in April, 1989, everyone – including those who, like myself, had to look up the meaning of enthalpy – had thrown together and electrolysis apparatus and was waiting for dividends. After a few weeks, with no reward, they quit in disgust, and denounced it all as incompetence, or fraud. Their votes are irrelevant.

Reproducibility is often cited as a canon of science. And so it is, in established areas. But, early in a study of a new phenomenon that involves an ill-understood macroscopic control of a microscopic mechanism, irreproducibility is not unknown. That was so at the onset of microchip studies. It also appeared in the initial phase of the discovery of high temperature superconductivity, which, by the way, is a prime example of “embracing the concept” without having “to understand the mechanism.”

After discussing objections raised against “cold fusion” Schwinger observed: “Imagine, then, a small but macroscopic piece of the lattice absorbs the excess energy of the HD or DD reaction. Please--I beg of you--do not rise in high dudgeon to protest that this is impossible because of the great disparity between atomic and nuclear energy scales. That is a primitive reaction to what may be a very sophisticated mechanism. And do not forget the failure of theory to predict, and then to account for the phenomenon of high temperature superconductivity. I advance the idea of the lattice playing a vital role as a *hypothesis*. Past experience dictates that I remind you that a hypothesis is not something to be proved mathematically. Rather, it is a basis for correlating data and for proposing new tests, which, by their success or failure, support or discredit the validity of the hypothesis. It is the essence of the scientific method..... The pressure for conformity is enormous. I have experienced it in editors’ rejection of submitted papers, based on venomous criticism of anonymous referees. The replacement of impartial reviewing by censorship will be the death of science.” Schwinger died in 1994.

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34) A way to demonstrate reality of CANR

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After reading the "**Cold fusion: an objective assessment**" again I would like to focus on some aspects of this four-times-rejected paper of Edmund Storms. It was a reasonable summary of progress, about three years ago.

Storms has a table listing various experimental approaches to chemically assisted nuclear reactions (CANR). These approaches are:

1. Electrolysis of D_2O (H_2O)-based electrolyte using a Pd, Pt, Ti, or Ni cathode; (This is the original P-F method, which has been duplicated hundreds of times to produce claimed AE and NP in every country where the method has been studied.)

2. Electrolysis of KCl-LiCl-LiD (fused salt) electrolyte using a Pd anode; (This method has been difficult to duplicate.)

3. Electrolysis of various solid compounds in D_2 (Proton conduction); (This method has been duplicated in the US, Japan and France to produce AE.)

4. Gas discharge (low energy ions) using Pd electrodes in D_2 (H_2); (Variations on this method have reported AE and NP in the US, Russia and Japan.)

5. Ion bombardment (high energy ions) of various metals by D+; (Variations on this method have reported NP in Russia and Japan.)

6. Gas reaction (H_2) with Ni under special conditions; (Replicated independently several times in Italy to produce NP and AE.)

7. Cavitation reaction involving D_2O and various metals using an acoustic field. (This method has been replicated in the US to produce NP and AE.)

8. Cavitation reaction in H_2O using micro bubble formation; (Several attempts to duplicate variations on the method have failed.)

9. Reaction of finely divided palladium with pressurized deuterium gas; (Variations on this method have produced NP and AE in the US and Japan.)

10. Plasma discharge under D_2O or H_2O; (Variations on this method have produced AP and NP in the US, Italy and Japan.)

11. Phase change or a chemical reaction, both involving compounds of deuterium; (NP production has been reported in the US and in Russia)

12. Biological Systems based on living cultures; (NP has been reported in Japan, Russia and France.)

After producing this list the author describes some of the methods and the results obtained. Addressing the issue of the nuclear origin of excess heat he points out that neutrons and tritium, the main reaction product of the so-called “hot fusion”, are relatively rare in CANR. Their rates of emission would have to increased by many orders of magnitude in order to be commensurable with the rate of generation of “excess heat.” The same seems to be true for the 14 MeV alpha particles and 3 MeV protons. But ^{4}He is accumulated at much higher rate and it may be the main reaction product. The same seems to be true for some heavy nuclides (according to Karabut).

The difficulty with the main products, as emphasized by Storms, is their very low level of concentration, typically several hundreds of ppm, after hundreds of hours of accumulation. If it were up to me I would focus on one product, alpha particles of 14 MeV. I know of two papers in which such particles (and 3 MeV protons) were observed. The authors of these papers (Lipson ... and Karabut...) used very different CANR setups but they observed essentially the same kind of energetic light particles. What can be more convincing, of the reality of CANR, than alpha particles of 14 MeV? That energy can not be attributed concentration of naturally occurring emitters, such Rn, Tn and their daughters. I also read a paper of J. Dash, from Portland State University, in which a chamber similar that of Karabut was used. A uranium foil, attached to the cathode, was found to be four times more alpha-radioactive after 550 hours of bombardment by D_2 ions (500 V and 5 mA) than before the bombardment. The energies of alpha particles, however, were not reported.

Unfortunately, the most direct method of particle identifications (a setup of two Si detectors) could not be used in the reacting environment (electrolytic cell of Lipson or a gas discharge chamber of Karabut). Therefore the authors used less direct methods of measuring energies of alpha particles. They did the best they could under the limitations of methods #1 and #4 listed by Storm. The method # 9, however, is likely to be amenable to the use of a single solid state detector. A silicon detector, placed one or two millimeters away from a layer of powder, could be used, even when the gas pressure is increased to several atmospheres. If it were up to me I would mobilize several teams to conduct such experiments. Observation of 14 MeV alpha particles would be a highly convincing argument that nuclear reactions can be induced by chemical means. This would be an important scientific contribution, the first step toward a normal process of exploring a phenomenon by going from known to unknown. The unfortunate period in which “cold fusion” investigators were treated as pseudo scientists, or as dishonest manipulators, would end by recognizing the reality CANR.

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35) Another nuclear signature

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Another unique nuclear signature of the so-called "cold fusion," able to prove the reality of CANR, is emission of gamma rays, as reported by many researchers. According to Karabut, for example, gamma rays can be observed from a gas discharge tube filled with deuterium and equipped with a palladium cathode. Presence of gamma rays should be relatively easy to either confirm or to deny. The low level gamma radiation takes place during the discharge, and up to height days after the discharge. Looking for gamma rays after the discharge can first be conducted by using a common Geiger counter (with appropriate filters) and then with a more sophisticated detector able to identify numerous peaks presumably due to specific transmutation products.

The methodology for trying to establish reality CANR (chemically assisted nuclear events) should be similar to that used in statistics. Start with the so-called "null hypothesis," that is assume that an observed phenomenon is not due to a nuclear process. Do everything possible to show that it can be explained by a non-nuclear effect. Failure to show this, (not only by me but also by knowledgeable scientists investigating the results), would be an acceptable argument in favor of CANR. In the case of the low level gamma rays, for example, one would have to rule out a possibility that they come from a contamination of some kind or that what is believed to be gamma rays is actually noise, etc.

The opposite approach would be to take seriously all indications which can possibly be interpreted in terms of what one wants to demonstrate. Let me give an illustrations of this. A claim is made that the operation a TV is associated with nuclear reactions. A person who believes in this claim sets up a demonstration. Two identical TV sets are placed in a room. One of them is on while another off. After a month or so a paper tissue is used to remove the dust accumulated on each screen. The tissue used to clean the operating TV turns out to be radioactive (about five times above the background level, as in my home) while the other tissue is not radioactive. This reproducible fact, however, has nothing to do with nuclear processes being activated by the operation of a TV set. It has to do with ions of radon (and its daughters) in the air, and with their natural migration along the electric field lines toward the charged surface of the operating TV set.

I am assuming that honest scientists conducting research in the area of "cold fusion" are objective and that they do not fall into a trap of ignoring possibilities of alternative explanations. But suppose that this assumption is contradicted and that the apparently nuclear signatures, claimed to be highly significant by hundreds of specialists, are finally shown to be either nonexistent or misinterpreted. This would indicate that a phenomenon of large scale self-deception, and mutual support, is a real possibility in any area of science. I wander if criticism of other people work is common within the "cold fusion" community. How does this community deal with real fraud and deception likely to occur in any area? I suspect that there may be a tendency not to publicize disagreements, and fraudulent data, in order to protect the field from possible "bad publicity." But who knows, perhaps I am wrong on this.

Let me end with an interesting observation; it was influenced by Jacques Dufour's paper which I summarized as item # 29. He asked: why is it that CANR are so difficult to prove? And the answer was: "[because] "trace amounts of byproducts are expected. To eliminate all problems of initial impurities present in the metal, and to be well above the detection limits of the instruments, will require to further increase the energy production and to run experiments on very long periods." It is well known that detection limits for radioactive substances are usually by many orders of magnitude lower than for stable substances. But the reaction products of "cold fusion" are mostly stable. This presents experimental difficulties. On the other hand, nuclear reactions generating energy without producing radioactive waste are highly desirable. According to Dufour, "this is a drawback to convince of the reality of the phenomenon, but a considerable advantage when uses are envisaged."

Here are simple numerical illustration for an introductory science course. Suppose that excess heat is generated at the rate of 10 W, as in some “cold fusion” experiments. Also suppose that each nuclear event produces 6 MeV of energy; this translates into about 10^{-12} joules per event. How many events would occur in each second? The answer is: $10/10^{-12}$ or 10^{13}. Suppose that X is the only byproduct of a nuclear event. In that case the amount of X accumulated in each second would be 10^{13} atoms. The number of atoms accumulated in ten hours would be $3.6*10^{17}$ (or 2.4 micrograms, if X were ^{4}He). If X were a radioactive product, characterized by the half-life of ten thousand years, then the disintegration rate would be $7.88*10^{5}$ per second.

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36) Personal reflections of George Miley

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Those who are interested in sociology of cold fusion (ethical and political aspects), should definitely read the article of George Miley. That article deals with ethical issues associated with "cold fusion." It can be downloaded (as a 10-pages long pdf file) from the library at this web site:

http://canr-lenr.org/Features.htm

I met George at the recent International Conference on Emerging Nuclear Energy Systems (Albuquerque, 2002) where he presented a paper entitled: "Low Energy Reaction Cell for Portable Power." Miley thinks that "Low Energy Nuclear Reactions (LENR) potentially offer a radical new approach that could provide portable power units in the 1-50 kW range." He is a coauthor of the paper which I summarized in item #28. This paper, and earlier references found there, describe experiments demonstrating nuclear processes in cold fusion setups.

Section 1 of the article, entitled "Personal Reflection," describes the early cold fusion meetings at which Miley participated; he was the editor of three scientific journals: Fusion Technology (FT), Laser and Particle Beams, and Journal of Plasma Physics. He wrote, for example,: "Then there was the famous NSF-EPRI meeting in Washington DC where the NSF ended up withdrawing 'official' sponsorship at the last moment due to the swing in opinion against CF. Despite this controversy, Edward Teller attended this meeting in a wheel chair (due to a recent operation) and provided a guiding example of an open scientific mind by freely entering the discussion." Section 2, entitled "Publication Policies and Related Issues" deals with important issues facing editors of scientific journals.

In that section Miley wrote: "Soon scientific sentiment turned against CF, and editors of *Nature* and the APS Physics journals quickly took the stance that CF did not have a "scientific" base. Thus, they would not even send papers on the topic out for review, shutting the door for any CF papers in these key journals. Despite that example, I stuck with the original decision that papers passing review should appear in *FT*. As a result, by default, *FT* virtually "cornered" the market for CF papers! A backlash quickly followed, with "hot fusion" members of the *FT* editorial advisory board and some readers vocally questioning my decision. Some declared these papers would "destroy" the journal. At that time, I strongly reiterated (and continue to do so) that the purpose of a journal is to communicate basic science and technology so that papers which can pass review should be published as long as the topic is consistent with journal coverage. I emphasized that I did not feel that I had the right as editor to arbitrarily turn papers away because they were from a 'questionable' field. . . .

Another criticism of my editorial policy on CF has been that since I have done research on the topic, I must be biased in favor of it. It's true that I have had papers in most ICCF meetings, starting from the original LANL meeting in Santa Fe. This criticism, in my view, amounts to a double standard. My initial selection as *FT's* editor, and the other two journals, was based on my recognized research on fusion, lasers, and plasma physics. This track record was assumed to provide me with better insight into the technical content of the papers, and allow me to select top reviewers. In universities, teaching and research are well recognized as reinforcing each other. The same is certainly true for editing and research. Why wouldn't the same be true for CF? Again, this ethical issue is left to the reader to consider, namely, do we want general managers as journal editors or, do we want experts from the field, despite possible conflicts of interest? In conclusion, the issue of whether my *FT* position, as opposed to *Nature*'s closed-door policy, is proper for a scientific journal must be left to the reader."

The next three sections deal with the role of the Internet as a vehicle of scientific communication. Miley writes: “Clearly the web bulletin boards fill a very important role for rapid scientific exchanges. However, this is best used in a concurrent flow of papers through peer-reviewed journals. The review process is time consuming, but it serves to sort out and distill the fundamental results, providing for a more calmed deliberate interchange that ultimately enhances scientific progress in the field. The unfortunate refusal of editors to receive CF papers disrupted the normal system, and left the bulletin board crowd in charge. Without a counter-balance for peer review (except for *FT* which could not handle this volume and variety of topics involved), CF was left in a confused state. It is difficult, if not impossible, to sort through the bulletin board materials to focus on real issues. Now that bulletin boards of this type have spread widely, we can expect an even more explosive and disastrous episode if a situation like the CF news announcement occurs again. The best defense against reoccurrence of a CF-type episode in the future is for the major journals to assume their rightful role of an “open door” for papers passing peer review.”

Section 6, entitled Scientific Integrity and Openness,” is also very interesting. Integrity means honesty, openness means not hiding proprietary information before patenting and not having industrial secrets. The author writes: “Let us now turn to issues of scientific integrity. Integrity, in the sense of avoiding fraud, has been an all too frequent topic of discussion in the CF field. Some even accused Pons and Fleischmann of fraud, or of purposely misleading others trying to replicate their results. To my knowledge, there is absolutely no truth to these innuendoes. As Pons and Fleischmann stated early on, and history has verified, their experiments were not reproducible due to unidentified factors in the materials science of the electrodes. Generally, when an electrode “worked,” all from that batch of Pd did so, and conversely if it did not work, none did. (This problem is now generally thought to be associated with micro cracking that occurs in some electrode materials during the expansion and stresses caused by loading. Thus, in my own research I have tried to avoid this problem by the use of thin sputtered films for the electrodes. These films have more elasticity so that the tendency to crack during loading is reduced.) While Pons/Fleischmann explained this problem at various meetings, many refused to accept their explanation, claiming something was being withheld. As time passed, it became clear that Pons/Fleischmann had indeed provided all of the factual information known about the electrode problem. However, they were significantly hampered in “openness” in some aspects of the research by overzealous sponsors requesting tight reigns on intellectual property, a situation that remains all too common in the company-dominated field of CF.

Others have somehow tried to associate fraud with the initial introduction of CF via a public news announcement. That view is that the news announcement was purposely distorted for personal gain. To my knowledge, that is simply not true- The information provided was a factual presentation of the data as these researches saw it at the time. However, the news release approach is a most serious break from traditional behavior in any scientific field. In retrospect, it must be noted that the pressures on Pons/Fleischmann at that time were tremendous. Indeed, I would suspect that others who have been so vocally critical of them may have turned to this route if they were placed in a similar situation. Still, the disclosure of scientific results via new releases is certainly to be avoided if at all humanly possible. Such actions are certain to create a “backlash” in the community that interferes with (or may even stop) the scientific search for truth. Everything from the scientific community’s evaluation of the basic science to funding for the field can become grossly distorted by the emotions set in force. Indeed, in the case of CF, the resulting “backlash” soon isolated the field from the mainstream scientific community.”

In the last section, entitled “Conclusion” Milye wrote: “With the growing pressures on researchers in modern society, we must work hard to preserve an atmosphere where the primary objective is to “seek the truth”. Clearly, the turmoil and divisions in the CF area created by persons both within and without the field confused and retarded this search for truth. With human nature being as it is, it is hard to believe that we can prevent a repeat of the CF episode in future areas where high stakes of money and prestige are involved. The education of upcoming scientists, journalists, research managers, etc. in scientific ethics is the best defense. Indeed, my only formal training in the area was a one-hour course on “professional ethics” required of all science/engineering students when I was a senior in college. “

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37) Coulomb barrier lowering?

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How can two deuterons fuse without having sufficiently high kinetic energy to overcome repulsion? By the tunneling "under the barrier." The theory of than QM effect is well developed and its predictions, for D+D fusion in plasma, agree with experimental data at all kinetic energies above 10,000 eV. But what about even lower energies in solid substances, for example, at 0.05 eV, which is typical? At such energies, according to theoretical calculations, the probability of fusion is about 50 orders of magnitude lower that what would be consistent with the amounts of excess heat. Therefore, argue the critics of "cold fusion," what is observed is not ordinary nuclear fusion. Those who believe in reality of "cold fusion" accept the criticism but claim that Coulomb barrier is somehow lowered in certain chemical environments. But they do not know why this happens.

According to conventional tunneling theory the probabilities of nuclear reactions should not be influenced by changes in chemical arrangements of atomic nuclei in solids. But a study conducted by Kosagi, in 1998, contradicted this expectation, as described by Storms (see my item # 32). Kasagi confirmed that the theoretical predictions of D+D fusion probabilities agree with experimental data at kinetic energies above 10,000 eV. At lower energies, however, the measured probabilities (in the PdO solid) were found to be higher that theoretically predicted. At 2,500 eV, for example, the experimental probabilities (technically expressed in the form of cross sections) are fifty times higher than theoretically predicted. The discrepancy grows very rapidly when kinetic energies become lower.

Unfortunately, no experimental data on D+D fusion probabilities are available below 2,500 eV. Is it reasonable to think that in going from that kinetic energy to 0.05 eV the discrepancy may become much higher, especially when the PdO is replaced by a material able to generate excess heat? Here is how all this was described by Storms (in a review rejected by four journals):

"Enhanced Cross Section: The observations imply a higher than expected cross-section for fusion at very low energy. To explore this possibility, Kasagi et al.[162] bombarded various metals and compounds with deuterons of various energies down to 2.5 keV and measured how much enhancement was produced in the reaction D(d,p)T, as shown in Figure 9. Apparently, an increase in cross-section does occur at low energy when the reaction occurs in a solid, in contrast to when the reaction is initiated in a plasma. In addition, enhancement is greater in PdO than in a number of pure elements[163], thereby showing that the nature of the chemical environment is important. Because the solids were not expected to be nuclear-active under the conditions of the study, the measured cross-sections represent only a lower limit for what might be possible when the "correct" solid is used. Other studies indicate that the branching ratio between tritium and neutron production might also change at low energy.[164-166] Once these possibilities are acknowledged, the next problem is to propose how the cross-sections might be increased at low energy by the surrounding atoms."

This kind of reasoning can be used to suggest experiments designed to probe the region of lower energies. It should be used to promote new investigations of the tunneling effect at very low kinetic energies. I see nothing unusual in such constructive speculations. Why did the editor of Review of Modern Physics label Storms' article as pathological science (see item #33)?

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38) On production of tritium

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I have just read “Tritium Production from Low Voltage Deuterium Discharge on Palladium and Other Metals” (by Claytor et al.) and am impressed. Why did this paper appear in the marginal, “Journal of New Energy” (vol 1, #1, 1996, p111-118) rather than, for example, in Physical Review? Did a main-stream journal editor reject the paper as fraudulent science? Would the paper be rejected today?

You may recall my translation of a Russian paper (item #13) describing nuclear reactions occurring in a gas discharge chamber. The paper of T.N. Claytor, D.D. Jackson and D.G. Tuggle is similar, except it focuses on one aspect only -- production of the radioactive material, tritum. The authors are researchers from Los Alamos National Laboratory. (It is no longer a secret that tritium is an explosive material of a hydrogen bomb and that LANL is a place focusing on the design of nuclear weapons. Where else can you find better expertise for dealing with tritium then in that laboratory?)

The gas discharge apparatus consisted of a very thin Pd wire (diameter of 100-250 microns), used as a cathode, and a flat Pd plate, used as an anode. The gap between the wire, perpendicular to the plate, and the plate was several millimeters. The deuterium gas pressure was 300 torr. To prevent excessive heating short pulses of current (10 microseconds) were applied at the rate of 20 per second (2000 volts, 3 to 5 A). Tritium was produced when the wire was exposed to discharge plasma; typical accumulation rates were between 0.1 and 0.2 nCi/hr. In one experiment 102 nCi was produced in about 300 hrs. The article describes numerous precautions, and tests conducted, to make sure that observed generation of tritium was real (rather than due to contamination).

What can be a better proof that a nuclear process can take place at nearly room temperatures? Journal of New Energy, in which the article was published, is edited by Hal Fox. I met Hal in Salt Lake City and he showed me his laboratory. But this deserves a separate item.

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39) Other strong cases

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You may recall my translation of a Russian paper (item #13) describing nuclear reactions occurring in a gas discharge chamber. The most convincing argument of nuclear processes, as far as I am concerned, was the discovery of iron whose isotopic isotopic composition was highly abnormal. I am now reading a summary of a Japanese paper reporting similar observations. The title of the paper is "**Changes in Isotopic Distribution of the Elements on Palladium Cathode after Electrolysis in D_2O Solution**;" the first (one out of nine) authors is T. Mizuno.

The isotopic composition of Cr, collected from the surface of a palladium electrode, after it was used to produce excess heat, was unnatural, as shown below:

A --> 50 52 53 54

from Pd --> 14% 51% 2.4% 11%
natural --> 4.3% 84% 9.5% 2.4%

According to the summary "essentially the same phenomenon was confirmed eight times, with high reproducibility, at the cathodic current density of about 0.2 A/cm^2." How can the isotopic composition be changed without a nuclear process? These finding were presented in 1997, as paper #71198, at IECEC. I do not know what the IECEC stands for, probably for a conference.

In the paper #97373, entitled: "**Nuclear Transmutations Induced by Light water Electrolysis with Gold Electrodes**," presented at the same conference, the authors reported finding iron of highly unnatural isotopic composition. Their 50 % abundance of ^{57}Fe (instead of natural 2.2%) was essentially the same as that reported by Karabut (see item #13). Note that Karabut analyzed products accumulated on the palladium cathode in the deuterium discharge chamber while the Japanese were studying the electrolysis of ordinary water. According to the summary "the amount of Fe reached some 10 micrograms after the electrolysis for 20-30 days."
The authors' affiliations are:

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b) Catalysis Res. Center, Hokkaido University, Saporo, Japan.
b) Hakadate Natl. Coll. Technol, Hakadate, Japan.

How would a pathological skeptic react to such data? Here are my guesses:

a) The data might have been invented to deceive (a case of fraud).
b) The authors do not know how to measure isotopic compositions.
c) Somebody “spiked” their cathode, just for the fun of it.
d) The data are wrong because they conflict with accepted theories.
f) The data should be ignored because they are pseudo scientific.

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40) A Cold Fusion Essay

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1) What follows is a lecture outline for the second semester of General Physics. My goal is to focus on conceptual aspects of CF rather than on its historical and social aspects. I think that trying to avoid the phrase "cold fusion" is like trying not to use the noun "heat." Cold fusion is not the best name for the phenomenon of LENR (low energy nuclear reactions) but it is the name under which it was introduced, and under which it is remembered by most people. Some prefer CANR (chemically activated nuclear reactions), others prefer AEP (anomalous energy) phenomenon. These are more descriptive names but CF is likely to be used most frequently.

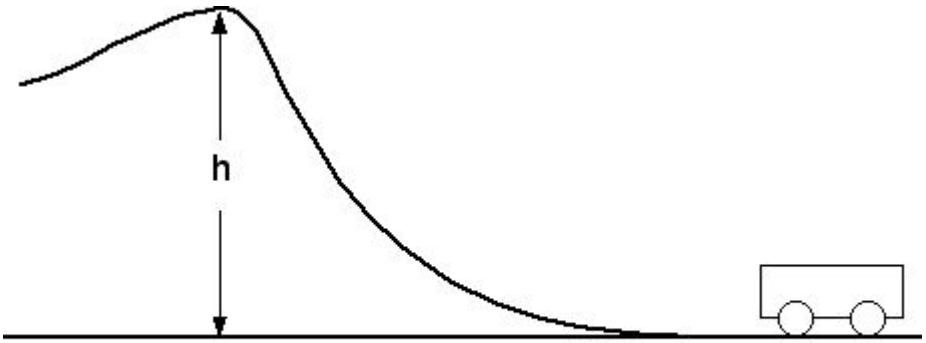

Figure 1

2) The term fusion will be introduced as an act of a perfectly inelastic collision between two atomic nuclei moving toward each other. The nuclei will be visualized as uniformly charged spheres. This is a mental picture, and a mathematical model, responsible for naming the phenomenon CF. What is necessary to make two positively charged nuclei fuse like two drops of water? They repel each other with Coulomb force and that force must be overcome by the cohesive nuclear force. The nuclear force is known to be negligibly small (with respect to repulsion) when the gap between the spheres is about 2 F ($2*10^{-15}$ m) but it becomes dominant at shorter distances.

3) After discussing nuclear fusion in terms of forces I will start describing it in terms of energies. To facilitate the transition I will refer to a familiar example of a frictionless cart pushed along a horizontal road toward a hill, as illustrated in Figure 1. Yes, the size of the cart is exaggerated. Will the cart go over the hill (this represents fusion) or will it start rolling back, after reaching some elevation? Students know that the answer depends on the initial kinetic energy of the cart, KEi. The car will go over the hill when KEi>PEt, where PEt=m*g*h is the potential energy at the top of the hill. Otherwise it will roll back without reaching the top. The energy diagram to discuss nuclear fusion is shown in Figure 2.

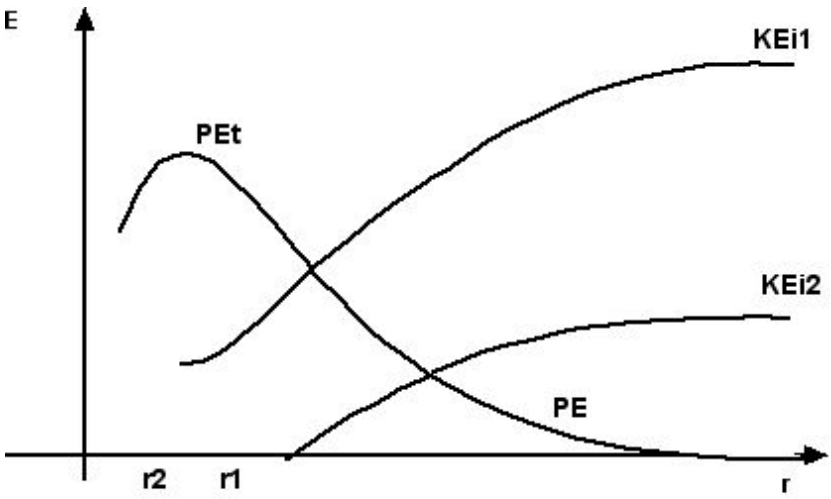

Figure 2

The horizontal axis of that figure, r, is for distances between the centers of the spheres; the vertical axis. E, is for energies, both potential and kinetic. The line with a maximum shows how the potential energy, PE, depends on the distance r. By convention, potential energy associated with repulsive (electric) forces is positive while the PE associated with attractive (nuclear) forces is negative. Repulsion is dominant at $r>r_1$ while nuclear forces are dominant when $r<r_2$. The maximum potential energy occurs between r_1 and r_2. The lines labeled KEi1 and KEi2 show how kinetic energies change with r, in two different cases. In one case the initial kinetic energy is smaller than the maximum potential energy; in another it is larger. The total energy PE+KE must remain constant at any r>r2 (before fusion), as for the cart in Figure 1. This explains why fission is energetically forbidden unless KEi>PEt.

The maximum potential energy, PEt is often referred to as the Coulomb barrier. It turns out that for two deuterons the PEt is slightly lower than 1 MeV ($1.6*10^{-13}$ J) while r2 and r1 are about 1 F and 2 F, respectively. The non-sphericity of deuterons, and the centrifugal potential energies, can be ignored in a preliminary, semi-qualitative, description.

4) After introducing the energy diagram I will digress toward two ways of increasing the initial kinetic energies (to satisfy the KEi>PEt condition). The first way is to increase the gas temperature and the second is to accelerate ions, for example, by using an electrostatic generator. Referring to kinetic theory of gasses, and assuming that the absolute temperature is ten billion K, I will show that only a very small fraction of particles has enough kinetic energy to exceed 1 MeV. Electric accelerators will be mentioned because they are often used to study probabilities of fusion at different kinetic energies. These experiments showed that fusion occasionally occurs even when KEi is less than the Coulomb barrier, PEt. This is schematically illustrated in Figure 3.

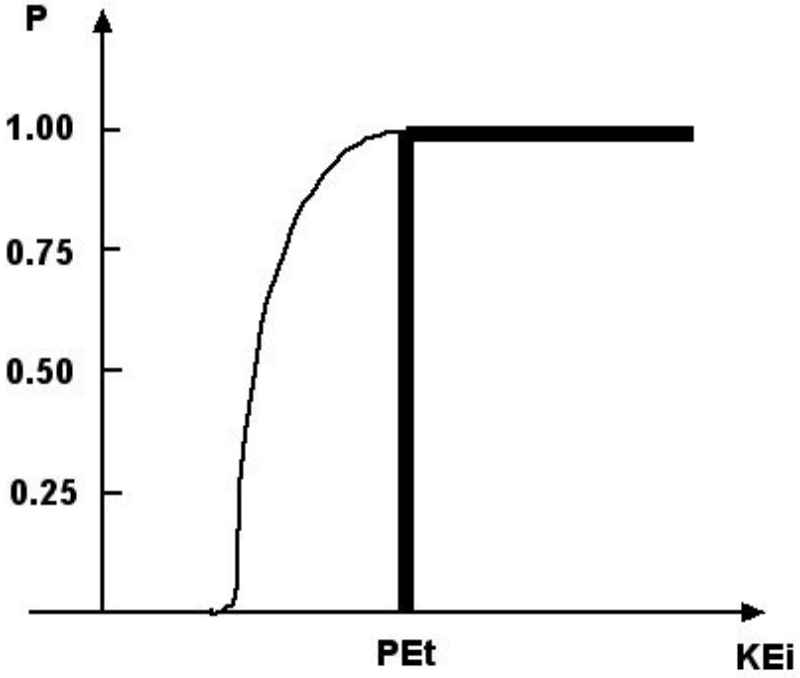

Figure 3

The thick line, in that figure, corresponds to predictions of classical theory while the thin line corresponds to experimental data. Our classical theory predicts that the probability of fusion, p, changes suddenly from zero to one at KEi=PEt. The experimental data, on the other hand, show that the transition is gradual (the energy scale strongly expanded in Figure 3).

5) I will tell students that in physics, unlike in mathematics, theories are validated by experimental data. The data show that the familiar classical model, highly reliable in macroscopic physics, is no longer a good description of what actually happens in a submicroscopic process. This observation led, in the 1920's, to the development of a new theory known as quantum mechanics, QM. That theory is in good agreement with experimental data for kinetic energies above 10,000 eV. For any given KEi the theory can be used to calculate the probability of fusion, p. The value p=0.1, for example, would indicate that, on the average, only one out of ten collisions result in fusion while the rest result in rebounding (scattering).

Referring back to Figure 1 I will ask: "what is needed to allow the cart to pass to the other side of the hill when KEi is smaller than PEt?" And I will wait till the word "tunnel" is mentioned. The QM under-barrier fusion is often referred to as a tunneling effect. The new theory can be naively interpreted by saying that p is a probability of "finding a tunnel." Detailed calculations show that p decreases very rapidly when D=PEt-KEi becomes larger and larger. The consequence of this is that fusion at ordinary temperatures should be extremely rare, p is much smaller than 10^{-50}. In other words, according to the tunneling theory, fusion at room temperature should be practically impossible. That is why most physicists were skeptical when a claim was made, in 1989, that experimental data contradict this prediction. The controversy, as far as numbers are concerned, has to do with the probability of fusion. Is it smaller than 10^{-50} or is the order of magnitude closer to 10^{-20}? Note that $p=10^{-20}$ still means that only one out of 10^{20} collisions results in a nuclear fusion event. But considering how many atoms move toward each other at any given moment, and the amount of energy released (for example 10 MeV per event), the $p=10^{-20}$ is large enough to produce detectable heat.

Criticizing cold fusion experiments was easy in the first two or three years after its premature announcement because the phenomenon turned out to be more complex than anticipated. The numerous factors influencing outcomes of experiments, such as the role of impurities, were not known and "failures to confirm" were not rare. But the situation changed and experimental data are said to be much more reproducible today. The consensus of scientists working in the field of cold fusion is that nuclear processes do occur with measurable probabilities. They claim that the QM tunneling theory is no longer applicable at very low kinetic energies and when ions are oscillating about their equilibrium on metallic surfaces. On the other hand, a generally accepted theory of cold fusion does not exist.

It is important to emphasize that the tunneling theory has not been developed to deal with kinetic energies as low as 0.05 eV (room temperatures). An attempt to verify the tunneling theory, at very low kinetic energies, was made in 1998 (Kasagi et al.). By using beams of accelerated particles the authors showed that theoretical predictions agree with experimental data at KEi>10,000 eV but not below that limit. At 2,500 eV, for example, the experimentally measured p was found to be over fifty times higher than what was predicted. The main argument against the idea of cold fusion was based on the assumption that the tunneling theory, and its parameters, are valid down to kinetic energies as small as 0.05 eV?

What should scientists do when experimental data seem to disagree with a model in an area in which the model has never been tested? The first thing to do is to make sure that data are reproducible, the second is to agree on the interpretation of data. For example, if excess heat is confirmed then what evidence do we have that it is due to nuclear reactions? If alpha particles, or tritium, are observed then we must be sure that this is not due to a contamination. And the third thing to do is to reanalyze and to improve the theory. A theory which disagrees with confirmed and reconfirmed facts should be declared to inapplicable.

6) The overall (scientific and social) picture of the situation in the field of cold fusion can be summarized as follows.

a) The scientific community is divided into supporters and skeptics.

b) Skeptics, representing mainstream science, claim that supporters are not true scientists. The skeptics refuse to conduct cold fusion experiments.

c) Supporters continue experimenting and discussing significance of their findings. Evidence backing reality of nuclear processes influenced by changes in chemical (atomic) arrangements is mounting. But most of that evidence is not described in mainstream journals.

d) Conducting research in the area of cold fusion is a “risky business” for young scientists. This is caused by practically non-existing financial support, for example, from NSF or DOE, and by unjustified accusations that anything connected with cold fusion is “pathological science”. Most scientists conducting cold fusion research are at least sixty years old.

e) In my opinion, the time is ripe to conduct an objective evaluation of what has been accomplished in the last ten years, and to either confirm or deny that the area is unscientific. If the interdisciplinary cold fusion field is found to be scientific then research in it should be encouraged, as in any other area.

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41) A message from a friend

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An Internet friend, physics teacher an electrical engineer and an observer of cold fusion, sent me a message about that field last night. I think his wise comments and quotations are worth adding as an item to my list. He wrote:

“ I'm still running the forum for Cold Fusion and physics anomalies, "vortex-L." A couple of the CF scientists are current subscribers, as are the ones running the lenr-canr.org site. See

http://amasci.com/weird/wvort.html

The biggest issue I have with the CF controversy is that each experiment supposedly takes upwards of six months, since it takes that long to build up sufficient protons in the palladium electrodes. Simply designing the appropriate equipment take more months. Yet the "ERAB" report condemning Cold Fusion was submitted only 8 months after the very first Pons-Fleischmann announcement. Cold fusion supporters label this historical
event as "the rush to judgment," and point out that the CF claims couldn't possibly have been given honest testing. I can't see how it could be anything else. WHETHER OR NOT THE "CF" EFFECT EXISTS, the controversy appears to be a classic example of intellectual suppression.

I like Arthur C. Clarke's prediction, that Drs. Pons and Fleischmann will be
the only scientists in history to win both the Ignoble and the Nobel
prizes. Take a look at Julian Schwinger's [Nobel laureate] talk on CF:

http://www.lenr-canr.org/acrobat/SchwingerJcoldfusiona.pdf

(Unfortunately Dr. Schwinger is no longer around to defend CF against disbelievers.)

> In fact your old message will be reposted there soon. I wander what you think about cold fusion now? Your comments and observations will be appreciated.

If 500 researchers fail to replicate, while 5 researchers claim success, does this proves that the claimed phenomenon doesn't exist? When doing science, majority rules? Consensus leads the day? The real world doesn't work like that. Yes, maybe those 5 researchers made mistakes, so that their success was an artifact. But maybe the 5 were right, while the 500 unsuccessful attempts only prove that replication is extremely difficult. We supposedly have ways to cut through to the truth, but they cannot work if the field is filled with covert irrationality driven by hidden emotions, or by huge glaring conflicts of interest. Fourteen years later the heat has died down and perhaps reality can finally be seen behind the noise caused by the CF-bashers and CF-lovers.

Ed Storms claims that CF is now easily replicated (well, easy for those with the skills and equipment.) He gets anomalous heat and and tritium production but not the neutron production which is expected from hot-fusion reaction paths (see Schwinger's comments on neutrons in his paper linked above). It would be very interesting if groups outside the current CF community would agree to look into this. Yet there's still the chance that such an attempt, if successful, would do nothing but ruin careers. After all, attaining success at "cold fusion" experiments supposedly DOESN'T add to the body of evidence supporting the existence of CF, instead it just proves that the experimenter is at best mistaken, or at worst has changed sides and gone to join all the other "loonies;" those professional scientists who claim repeated

success. If CF is a genuine phenomenon, such a controversy is certainly not the first of its kind...

1) "If I want to stop a research program I can always do it by getting a few experts to sit in on the subject, because they know right away that it was a fool thing to try in the first place." ...- Charles Kettering, GM

2) "...By far the most usual way of handling phenomena so novel that they would make for a serious rearrangement of our preconceptions is to ignore them altogether, or to abuse those who bear witness for them." - William James

3) "When adults first become conscious of something new, they usually either attack or try to escape from it... Attack includes such mild forms as ridicule, and escape includes merely putting out of mind." - W. I. Beveridge

4) "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die and a new generation grows up that is familiar with it." - M. Planck

5) "Theories have four stages of acceptance: i) this is worthless nonsense; ii) this is an interesting, but perverse, point of view; iii) this is true, but quite unimportant; iv) I always said so." -J.B.S. Haldane, 1963

6) "New and stirring things are belittled because if they are not belittled, the humiliating question arises, 'Why then are you not taking part in them?' " - H. G. Wells

7) "The mind likes a strange idea as little as the body likes a strange protein and resists it with similar energy. It would not perhaps be too fanciful to say that a new idea is the most quickly acting antigen known to science." - Wilfred Trotter, 1941

8) "Be not astonished at new ideas; for it is well known to you that a thing does not therefore cease to be true because it is not accepted by many." - Spinoza

9) "If we watch ourselves honestly we shall often find that we have begun to argue against a new idea even before it has been completely stated." - Wilfred Trotter

10) And most frightening, a quote from a paragraph about instances of suppression in the history of science: "Many [genuine] discoveries must have been stillborn or smothered at birth. We know only those which survived." - W. I. Beveridge, THE ART OF SCI. INVESTIGATION, 1950"

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42) Four Points of View

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1) In criticizing somebody who made an unreasonable theoretical claim about capacitors (on the Phys-L list) a physics teacher wrote:

> . . . It became transparently obvious pretty early on that you have
> little or no interest in being educated and almost no ability to
> listen to what others are saying. You seem to have developed an
> impermeable shield to criticism that allows you to discount it as
> misguided when patiently offered and malevolent when it (inevitably)
> becomes more blunt. This is a characteristic trait, as Bob mentioned
> in an earlier post, of practitioners of pseudoscience. .....In any event,
> you shouldn't expect anyone to show the least interest in a theory
> that predicts something VERY different unless you come armed
> with some convincing and reproducible experimental evidence and
> can show how your theory squares with the current well-verified
> predictions in most every other situation. . .

A message addressed to a cold fusion researcher would also emphasize the need for reproducible experimental evidence. But that evidence would be used to validate experiments rather than to validate a theory. Why? Because no acceptable theory dealing with chemically activated nuclear reactions emerged. One of the blunders of Fleischmann and Pons was to name the strange phenomenon cold fusion. The nuclear fusion theory exists but it seem to have nothing to do with (presumably discovered) new kind of nuclear reactions.

2) In a private message to me an active cold fusion researcher wrote:

>Today a new physical phenomenon must to be based on a
> theoretical model. I am trying to develop a model focusing on a
> possible mechanism of a nuclear reaction trigger. It is a new
> area of science -- physics of optically active solid media in the
> energy range of 1 - 2 keV. But experimental data are hard to get.

Who can deny that a theoretical model is desirable? Who can deny that a researcher armed with a model is likely to be more effective than those who are not? But saying that experimental facts can not be taken seriously unless they are described by a theoretical model is an exaggeration. Most experimentalists are not well equipped to develop sophisticated theoretical models. Physics is an experimental science. To me this implies that, in the final analysis, experiments are validated by other experiments, not by theories.

3) This brings me back to a Phys-L list message posted on December 14, 2002. My slightly edited reply to the anonymous friend was in capital letters.

Ludwik:
As you state repeatedly, the whole atmosphere around CF has been filled with poisonous material, some valid and some

emotional. One must be very careful, on entry into such an atmosphere, to be protected by a useful theoretical proposal or at least a plausible explanation that can be subjected to experimental tests. On the basic level there are two obvious questions:(1) How could hydrogen atoms fuse at such a low temperature?(2) If they do fuse, how is the energy released (if not in gamma rays, then how) i.e. what reaction occurred?

If one has no proposed answer or proposed experiment to get an answer, then one is in a state of massive weakness. Your message seems to be that there is new evidence for an interesting mystery, and the early workers were not fairly treated. The author of such a message will be classified as an apologist or defender, no matter how he qualifies such words. If, however, he has a plausible proposal, it could possibly be different. I infer that the major skepticism in the mainstream nuclear science community stems from the silence on the basic two questions above. Such skepticism seems to me to be justified until something reasonable is proposed or, better yet, demonstrated. Until then, essentially all responses will be "impurities or errors".

BOTH QUESTIONS CAN ONLY BE ASKED IF A CLAIM IS MADE THAT FUSION TAKES PLACE. IT WAS FOOLISH TO MAKE SUCH A CLAIM IN 1989 AND IT IS FOOLISH TO MAKE IT TODAY. THAT IS WHY NAMING THE EFFECT "COLD FUSION" WAS HIGHLY INAPPROPRIATE.

THE CLAIMS, AS FAR AS I CAN TELL, ARE:

A) EXCESSIVE (UNACCOUNTABLE) HEAT IS GENERATED IN SOME EXPERIMENTS. B) SOME NUCLEAR PROCESSES ARE TAKING PLACE WHEN EXCESS HEAT IS GENERATED.

YES, THE ONLY WAY TO VALIDATE SUCH CLAIMS IS TO PERFORM EXPERIMENTS. IF IT WERE UP TO ME I WOULD ASK PHYSICISTS TO FOCUS ON A SINGLE ASPECT OF THE SECOND CLAIM, FOR EXAMPLE, EMISSION OF ALPHA PARTICLES. INSTRUMENTS FOR CONTRADICTING OR CONFIRMING EMISSION OF ~14 MEV ALPHA PARTICLES AND ~3 MEV PROTONS (PRESUMABLY OBSERVED BY MANY) ARE WIDELY AVAILABLE. IN OTHER WORDS, THEY WOULD BE ASKED TO VERIFY WHAT IS EASY, AND WHAT COULD BECOME A HIGHLY SIGNIFICANT INDICATOR OF SOMETHING NEW AND INTERESTING. CONFIRMATIONS WOULD START THE USUAL PROCESS OF GOING FROM KNOWN TO UNKNOWN, FROM SIMPLE TO MORE COMPLEX, FROM EXPERIMENTAL FACTS TO THEORIES, ETC.

A LACK OF CONFIRMATION WOULD INDICATE THAT THOSE WHO MADE CLAIMS WERE WRONG. I WOULD THEN ASK PHYSICISTS TO FOCUS ON ANOTHER, PRESUMABLY OFTEN OBSERVED, "NUCLEAR SIGNATURE" AND TRY TO VALIDATE OR CONTRADICT IT. THREE FAILURES TO CONFIRM WOULD PROBABLY BE SUFFICIENT TO SAY THAT EXCESS HEAT, IF ANY, HAS NO NUCLEAR ORIGIN. TROUBLES BEGAN WHEN CHEMISTS, UNFAMILIAR WITH NUCLEAR PHYSICS, STARTED TO MAKE PRONOUNCEMENTS ABOUT IT, AND WHEN PHYSICISTS, UNFAMILIAR WITH CHEMISTRY, ENTERED THE COMPLEX FIELD OF ELECTROCHEMISTRY.

I visited the anonymous friend recently and we talked about cold fusion again. I described a hypothetical scenario in which a totally unexplained phenomenon is observed. Thorium nitrate (radioactive salt) is dissolved in pure water inside a sealed metallic container. The container plus solution are initially radioactive, as measured by a Geiger counter, or by a sophisticated Ge(Li) gamma ray spectrometer. But it is no longer radioactive after the experiment. A scientist who performed the experiment wants to publish the observation in a scientific journal. If you were the editor of that journal, I asked, would you allow the submitted paper to be published? The answer was “no I would not publish it.”

Then I said, suppose that the researcher takes you to his lab and performs the experiment in front of you. He then offers you a chance to set up a similar experiment from scratch and to perform it by yourself, perhaps using your own instruments with your own assistants. You do all this and you are convinced that ‘this incredible claim” seems to be real. Would you now allow the paper to be published? His answer was negative; he believes that experimental evidence, no matter how strong, should not be published, unless some kind of simple ‘theory” is provided. The issue of conservation of mass and energy must be addressed by the author.

I said that mainstream science publications, for example, in astronomy, are often “purely experimental.” Experimentalist

discover facts, theoretically inclined scientists try to explain them in terms of what is already known or to develop new models of reality. Why should a cold fusion publication be treated differently in that respect? I also reminded him about Pierre Curie's 1903 paper about the unexplained heat coming out of radium. It was a purely experimental paper; the only "theoretical" comment was that it must be something totally new. Yes, answered my friend, but this was one hundred years ago. What was acceptable then is no longer acceptable in nuclear science today.

Yes, the first discovery of excess heat was made exactly one hundred years ago. It would be nice if more recent "excess heat" claims were given a chance to be examined in 2003.

4) Let me end this item with an interesting observation made a biochemist, in a private conversation, "Trying to publish something in a scientific journal means convincing the editor, and reviewers, that the expressed ideas fit the existing paradigm. That is why those who discover unexpected things often encounter difficulties in trying to publish articles. But trying to patent an invention is just the opposite. The patent bureau wants ideas which are really new; something based on what is already known is not acceptable."

This is an interesting observation, except for one thing. Is it true that a special order has been given, by the US government to the Patent Office, not to accept patents based on cold fusion ideas? I heard about this from two people. I also heard that the legal situation changed very recently; cold fusion patents rejected in the past are ordered to be reevaluated. I would appreciate it if somebody familiar with patent issues (in the area of cold fusion) could describe the situation briefly. Please e-mail this description to me <kowalskil@mail.montclair.edu>and I will append it as an item on my web site.

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43) An Embarrassing Editorial

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In browsing the Internet with Google I found a letter to the editor of Nature from my classmate, M. Gryzinski. We (at the Polytechnic Institute of Warsaw) were proud of him when he constructed a Frank-Hertz tube and showed us the peaks. I haven't heard from him since our graduation in 1957 and was curious to find out what he wrote. Less than one month after the announcement of cold fusion he was referring to his earlier theoretical paper on how the Coulomb barrier between two atoms in H_2 could be lowered by a trapped electron. That was in Nature, vol. 338, April 27, 1989.

Looking through other issues of Nature I found David Lindley's editorial (volume 344, March 29, 1990) entitled "The Embarrassment of Cold Fusion." This is an example of a very negative evaluation of the field, one year after Fleischmann and Pons announced their discovery, and several months after the ERAB report. The author was the associate editor of Nature. I think that his article, see below, is worth adding to my collage.

* * * * * * * * * * * * * * *

Those readers who consider that this journal has published too much on cold fusion' should be grateful for what they have been spared.

In the year since Stanley Pons and Martin Fleischmann made their famous announcement at a press conference at the University of Utah, we have received a barely imaginable quantity of letters— most of them respectably typed, one or two handwritten entirely in capital letters —attesting to remarkable latent powers of creative thinking by scientists around the world. For only a few of these offerings was it. thought necessary to consult expert reviewers; fewer still survived that scrutiny.

Of the fraction of papers submitted that have thus reached print, the account appearing this week (see page 401) is of special significance. Michael Salamon and his colleagues, like the teams from Yale University and Brookhaven Laboratory, from the California Institute of Technology, and from the UK Atomic Energy Authority Laboratory at Harwell, have searched for nuclear emissions (neutrons, gamma-rays, electrons and protons) from cold fusion cells, and found nothing. The difference is that Salamon works at the University of Utah, and the cold fusion cells he examined were in the laboratory of Pons and Fleischmann.

This is not quite the same as saying that what Salamon et al. Iooked at were Pons and Fleischmann's cold fusion cells. As the article makes clear, Pons will not concede that any of the electrolytic cells were, at the time they were being examined, in fact producing anomalous heat. There was a two-hour period when, according to Pons, a cell under examination was producing excess heat -- but at that time the detectors and computers of Salamon and his colleagues were not working, having been put out of action by a power cut.

To this, Salamon et al. provide a clever response. Any neutrons emitted by the working cold fusion cell would have created some secondary radioactivity in the neutron detector, which should have been measurable once the equipment was switched on again after the power loss. But no such secondary activity was found. which allows Salamon et al. to conclude that no conventional fusion reactions could have produced the claimed excess heat.

The tidy summary of their paper is therefore that during a five-week period which came some time after the press announcement of Pons and Fleischmann on 23 March last year, cold fusion cells which may or may not have been producing excess heat certainly produced no anomalous nuclear emissions.

In the light of the known history of cold fusion, one has to ask whether this news will change the terms of the debate.

During the past year, the original claims of Pons and Fleischmann have diminished; the experimental evidence has been subtracted from not added to. In their 'preliminary note' (J. electroanalyt. Chem. 261, 301-308;1989 and erratum 263, 187 -188; 1989) Pons and Fleischmann said that they had found, as well as excess heat, production of tritium in the cell, and the emission of neutrons, detected by characteristic secondary gamma-rays from the surrounding water-bath. But the tritium concentration in a working cell turned out to be only three and a half times greater than that in the original stock of heavy water. Critics were quick to point out that such an increase is consistent with a recognized difference in the electrolysis rate of deuterium and tritium on account of their different masses.

Later, Petrasso (see Nature 339, 183; 1989) argued that the gamma-ray signals presented by Pons and Fleischmann as evidence of neutron emission had the wrong characteristics to be genuine detections, and were more likely to be instrumental artifacts.

This left only the claims of excess heat, which were themselves challenged at the' Baltimore meeting of the American Physical Society (see Nature 339, 4;1989)

Pons and Fleischmann must be given credit for declaring from the outset that the amount of excess heat they saw was too great, by orders of magnitude, to be consistent even with the levels of nuclear emission that they described in their preliminary note.

Supporters of heat production by cold fusion have always said that some new physical process, by which deuterons can fuse in secret without giving off telltale nuclear byproducts, was indicated by their results.

Thus arises the schism that separates those who believe in cold fusion from those who put the whole episode down to wishful thinking. On one side, nonbelievers see the negative results of Salamon et al. as incontrovertible proof that nothing unusual is happening; on the other, believers take the same results as affirmation that cold fusion indeed demands new physics, which they knew already.

The two sides are separated by a matter of faith, not one of science.

Efforts to bridge this divide have been made all the more difficult by the unceasing refusal of Pons and Fleischmann to say clearly and fully what their experimental evidence for cold fusion is. In the wake of their preliminary note and its published errata (which were themselves an extended version of a preliminary list of errata handed out as a photocopied sheet with the 10 April 1989 copy of Journal of Electroanalytical Chemistry), there came rumors (subsequently and vehemently denied at the Los Angeles meeting of the Electrochemical Society on 8 May last year) of heat generation by cells containing ordinary water rather than heavy water, and of the production of 'He in significant amounts. The discovery of ^{4}He in the gases coming from a cold fusion cell was briefly trumpeted as a demonstration that deuterons absorbed into the palladium electrode were indeed fusing, but by a reaction that produced 4He and a gamma ray rather than tritium and proton or ^{3}He and a neutron, as conventional nuclear physics would have it.

Inconveniently, however, materials scientists pointed out that if helium had been formed in the palladium electrode, it would have stayed there; the correct test would have been to look for helium in the palladium itself, not in the gases evolved by the cell. But because Pons and Fleischmann had given out the suggestion about helium production in their usual teasing and informal way, it could be retracted without much pain on their parts. During April and May last year, there were constant assurances that a detailed account of their experimental methods and results was in preparation, would be properly submitted to a scientific journal, and would appear during the summer, or in the autumn -- or perhaps a little later.

But what was reprehensible a year ago has now become absurd.

Still there are whispers of a hundred-page manuscript, replete with facts and figures, which the world will soon see. Most of the world, sadly for Pons and Fleischmann, is unlikely to care, except perhaps out of historical curiosity and a desire that the tale be neatly ended.

The waxing and waning of these various pieces of ancillary evidence, not to mention the sporadic nature of the successes achieved even by expert cold fusion researchers, was a wonderful liberation to those who rummaged through their undergraduate physics textbooks in search of forgotten phenomena that could be adapted into theories of cold fusion. It was a boon too that solid-state physics and quantum mechanics conspire to offer such a variety of unexpected and counterintuitive effects to the eager inquirer.

If the ideas submitted to this journal are a representative sample, theories of cold fusion generally have the same logical structure as the assertion: "If we had some ham, we could make a ham sandwich, assuming there's some bread handy". Starting from some bona fide physical effect, one argues that if it were many orders of magnitude more important than it actually is, cold fusion would be possible; left unspoken is the assumption that the effect in question is of some relevance in palladium saturated with hydrogen.

In the proliferation of theoretical proposals, there were surprisingly few basic ideas. A number of early suggestions referred to the Oppenheimer-Phillips effect: at very low energies, two deuterons in close proximity tend to orientate themselves so that their constituent neutrons approach each other and keep the protons apart, which enhances the fusion rate because it permits the neutrons to interact while minimizing the electrostatic repulsion of the protons.

Explanations of this sort illustrate a common feature of cold fusion theories. The discrepancy between the standard fusion rate and what was needed to generate the heat seen by Pons and Fleischmann amounts to some 60 orders of magnitude. and the Oppenheimer-Phillips enhancement is a matter of ten per cent or so; nevertheless the reader was invited to agree that things were moving in the right direction, and accept that a bit of numerical fine-tuning would be needed to take care of the details.

A somewhat more sophisticated clutch of ideas emerged as people latched on to the idea of effective masses. Two deuterons bound together in a molecule have a tiny but calculable chance of fusing and if, as a thought-experiment, the mass of the orbiting electrons is increased, the molecule becomes more tightly bound and physically smaller, and the fusion rate rises. It was then noted that electrons in palladium can have effective masses several times greater than that of a free electron; if two deuterons could be bound together with one of these 'heavier' electrons, an interesting fusion rate would result.

But a little learning is a dangerous thing. Electrons in a metal obtain their effective masses because they are not bound to a single atomic site, but move in concert throughout the lattice: push against one, and you push against them all. But if one of these lattice electrons is detached from the system so it can bind two deuterons into a molecule, it is no longer part of the electron collective, and reverts to having its normal mass. In other words, the effective mass of an electron in a solid depends on what you are doing to it, and if you want it to bind an isolated molecule, it can no longer have a high effective mass.

The third broad category of cold fusion theories rested on more sophisticated uses of collective effects in solid-state physics.

The general drift was that deuterons, being panicles of zero spin, could fall into a Bose-condensed state, in which their wave functions were all identical and periodic throughout the lattice. (This condensation can occur only if the temperature of the system is less than a few degrees above absolute zero but, as with the Oppenheimer-Phillips mechanism, it was the principle that mattered.) Once in this condensed state, one deuteron could interact with another at a different lattice site in the palladium, because all the identical periodic wave functions of all the deuterons would have their maxima at the same places.

These theories had the special attraction that they could easily be decorated with the jargon, at once forbidding and enticing, of solid-state physics: Bose-condensates, Bloch states, Wannier functions . . . like the Paris fashions, they outface mockery.

Nevertheless they were all wrong, and for another straightforward reason. The fusion rate for two deuterons is calculated from their two wave functions, multiplied by the nuclear interaction rate. The latter is a very short range force; only at separations of a few nuclear radii is the nuclear reaction rate significant. The only important contribution

to the fusion rate, therefore, comes ham the product of the wave functions when the deuterons are very close. But the wave function when the Bose-condensed state are calculated explicitly by ignoring the nuclear interactions; they are valid everywhere except at close range. Where the assumptions that lead to Bose-condensation are correct, the fusion rate is negligible; where the fusion rate might be significant, the Bose-condensation model is wrong,

All cold fusion theories put forward so far can be demolished one way or another, but it takes some effort. Although cold fusion was, in terms of 'ordinary' physics, absurd, it was not obviously so; it contravened no fundamental laws of nature. This made it easy for advocates of cold fusion to insinuate that some arcane but genuine phenomenon of quantummechanical solid-state physics might provide a credible theoretical foundation, and likewise made it impossible for skeptical physicists to give any clear and general proof that cold fusion could not work; all they could do was rebut one daft idea after another, and even that required a good deal of patient explanation.

But it has to be said that one of the reasons that Pons and Fleischmann prospered early on was that few people were willing to stand up and say why they thought cold fusion was nonsense. (One or two physicists suggested in private that it was up to this journal to take on the task in its editorial pages). After the 23 March press announcement, the response of most experts consulted by science reporters was academically correct but journalistically weak. It's a very interesting idea, was the gist of the scientific community's opinion, and we really can't say what we think of the experiments until we've seen more details, or tried it for ourselves, or consulted our colleagues in the chemistry department. This measured skepticism, contrasted with the unhesitant declarations coming from Pons and Fleischmann, sounded like academic nose-holding, as if physicists knew they had been beaten but could not bring themselves to admit it.

Perhaps science has become too polite. Lord Kelvin dismissed the whole of geology because his calculations proved that the Sun could be no more than a few million years old; Ernest Rutherford is still remembered for his declaration that talk of practical atomic energy was "moonshine"—but the stature of neither man has been noticeably diminished by their errors which were as magnificent as their achievements. Kelvin and Rutherford had a commonsense confidence in the robustness of their judgments which the critics of cold fusion conspicuously lacked. Would a measure of unrestrained mockery, even a little unqualified vituperation, have speeded cold fusion's demise?

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44) Before Going to Salt Lake city

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Let me begin with a message which I posted on the Phys-L discussion list several days ago. I wrote: “Two more items were added to my cold fusion site at:

http://csam.montclair.edu/~kowalski/cf/

You will recognize some voices from our recent debate. I also had an item written in January but decided not to post it. The item started like this: “What did I see today (1/19/03) in Salt Lake City?

a) An example of conspiracy to deceive by presenting fraudulent data? To what purpose?

b) A manifestation of a large scale self-deception involving hundreds of Ph.D. scientists in several countries? If this is possible then how do we know that it does not exist in other scientific areas?

c) Experimental support for a claim that nuclear processes can be initiated at nearly room temperatures? How can this be explained?"

Then I described what was shown to me and what I was told about it. I am not posting this message because I was invited to participate in the next experiment. I will be in a better position to write about this in March, after the experiment. They will show me how radioactivity of thorium nitrate (dissolved in water) can be practically eliminated in 30 minutes through a process involving an electric current of 5 A at 60 V. I do not believe that this is possible; my null hypothesis will be that radioactivity which was lost (if it was lost) went somewhere else. But what if attempts to find radioactivity in suspected places fail?

Then I will be able to say that a nuclear process (in this case turning radioactive isotopes into stable isotopes) can indeed be induced in a very unusual way. It is fun speculating about such possibilities. Such speculations are nothing more than wishful thinking; they are in conflict with what we have learned about nuclear phenomena. But rejecting experimental facts because they disagree with what we think should happen is not appropriate.

By the way, E. Storms just posted a LONG review of the entire CF field. He decided to call it a "student's guide." I would not offer it to my undergraduate students; the guide is for those who are prepared for study the field at an advanced level. But that only my personal opinion. In any case, this review is at:

http://www.lenr-canr.org/StudentsGuide.htm

Do not expect to digest it in ten or twenty minutes.” That was posted on 2/10/03. Let me add to this a summary of the 1997 and 1998 articles describing the Salt Lake City work. I composed it for myself but decided to share it. The essay begins on the next page.

A new kind of alchemy? Will it be confirmed?

Ludwik Kowalski, 2/13/03

Introduction

As most people know, chemistry evolved from alchemy, just as astronomy evolved from star gazing and from astrology. Alchemists tried to produce gold from less expensive elements. They failed and this led to the realization that chemical elements are immutable. The idea of immutability of elements, confirmed by all chemical experiments, is no longer as absolute as it used to be; we know that nuclear reactions often result in transmutation of elements. But this happens only when kinetic energies of subatomic particles (neutrons, protons and their aggregates) exceed those encountered in ordinary chemical experiments. An observed transmutation of elements at low temperature, if confirmed, would indicate that subatomic particles are somehow accelerated to unusually high energies.

Suppose that the potential difference between two electrodes is 100 volts. It could be used to accelerate protons to an energy of 10 eV. This is not sufficient to overcome an electrical repulsion of atomic nuclei. But suppose that a proton is attached to a cloud of a million electrons. (Speculations about the possible stability of such a cloud are described in Appendix 2.) A proton attached to the cloud, in traveling toward the positive electrode, could gain kinetic energy of ten million eV. This would be sufficient to penetrate a nucleus and cause a nuclear reaction. The purpose of this essay is to summarize two articles about experimental evidence of nuclear transmutations occurring in simple tabletop experiments. The authors of the articles, Hal Fox and Shangxian Jin, invited me to work with them and I will summarize what I know about their work. I plan use this essay as a basis for writing about the outcome of my ten-day-long visit to their private laboratory. This summary is limited to experimental facts; comments and tentative speculations are to be found in appendices. The articles were published in the Journal of New Energy (1,2); Hal Fox, from Salt Lake City, is the editor of that journal.

Experimental Setup

The setup, called LENT-1 reactor, was essentially a vessel with two electrodes made of oxidized zirconium. It was partially filled with 25 cc of distilled water in which various amounts of thorium nitrate (between 0.1 and 0.5 grams) were dissolved. The vessel was hermetically sealed to withstand high pressures (tens of atmospheres) at high temperatures (up to 500 F). The electrodes were connected to secondary winding of a power transformer (up to 2 kVA). The typical current flowing through the electrolyte was 5A while the effective a.c. voltage was typically less than 100V. The potential differences across the electrolyte were very small in comparison with potential differences along the very thin layers of zirconium oxide covering the electrodes. This fact seems to be significant, as elaborated in Appendix 2.

Thorium nitrate was chosen because it is highly soluble compound containing several radioactive nuclides (^{232}Th in equilibrium with its daughters). The main purpose of the experiments was to show that the initial radioactivity can be reduced very significantly during short time intervals, most often 30 minutes. Let me mention that numerous attempts to reduce radioactivity, shortly after radium was discovered by Marie Curie, were made but they were not successful. According to (3): “High and low temperatures were tried, strong electric and magnetic fields were applied, tremendous pressures, and the strongest possible chemical reagents were put to use, but not a single one of the most powerful weapons of the physical laboratories affected the radiation of energy by radium.” In that context the findings of Fox and Jin are very puzzling. At present I accept their data but during the experiments my position will be different; I will do everything possible to question their findings. But I will be open-minded; I will change my skeptical attitude if we fail to contradict the data.

Summary of experimental results

The authors write that it took them three years to develop a procedure for the effective transmutation in the LENT-1 reactor. Over a dozen successful experiments were performed. The method used consisted of comparing radioactivity “before” the current was turned on with radioactivity “after” the current was turned off, 30 minutes later.

1) A Geiger counter measuring radioactivity of the electrolyte showed practically nothing but the background after the

processing. The level of a constant radioactivity before the experiment was “considerably above the background.”

2) The same counter was used to measure the radioactivity from the surface of electrodes. The electrodes were not radioactive initially but became radioactive after 30 minutes of processing. The most remarkable was that the surface radioactivity was not constant, it decreased, more or less exponentially to a constant level (about 90 counts per minute) after 200 hours. This was interpreted as an indication that relatively short-living radioactive nuclides were formed through nuclear reactions.

3) A mass spectroscopic analysis of electrolyte, performed in a specialized laboratory, showed that 95% of thorium was lost. This was consistent with what has been inferred from the Geiger counter data (see point 1 above).

4) The second article of Fox and Jin focuses on the use of two gamma ray spectrometers, one using a pure Ge detector. The mass spectrometric analysis showed that the amount of Th in the electrolyte was reduced from 4399 ppm to 9.5 ppm. This confirmed findings reported in the first article.

5) The below shows the cutaway view of the vessel and the location of the Ge detector. The gamma ray spectra (actually shown in the publication) were obtained during one hour interval immediately after the end of processing. The vessel was not open and gamma rays had to penetrate the 1 cm wall of zirconium.

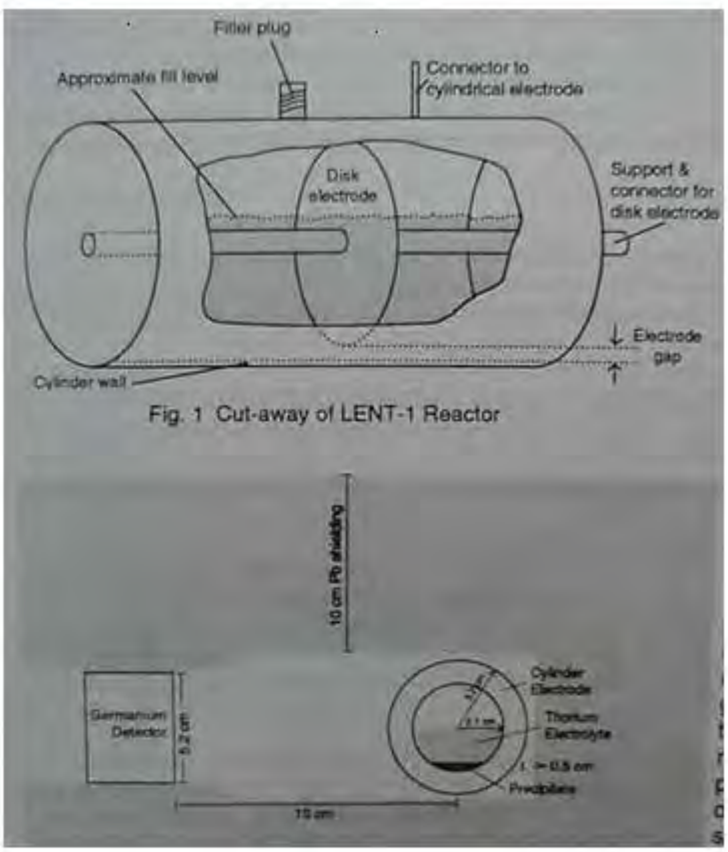

Figure 1.
The setup shown to me was slightly different. It was essentially a zirconium cup whose central electrode (a zirconium rod) was supported by a covering teflon plate.
Intensive underwater arcing and sparking accompany the flow of current.
= =

As illustrated below, the radioactivity from the entire setup (based on four well known gamma ray peaks due to daughters of thorium) was reduced by approximately 50%. This is five times more that could possibly be attributed to a change in the distribution of radioactivity (concentration in the precipitate).

	peak (keV)	net cnts/hr before	net cnts/hr after
^{228}Ac	911	127 +/- 11	66 +/- 8
^{212}Pb	239	421 +/- 22	204 +/- 16
^{212}Bi	727	40 +/- 7	26 +/- 6
^{208}Tl	583	217 +/- 15	105 +/- 15

Is there a contradiction between these results (showing that the loss of radioactivity, due to the prominent peaks was about 50%) and the 95% reduction reported in points 3 and 4? It depends on what fraction of the total radioactivity is represented by the selected gamma ray peaks.

6) In another experiment the setup was left closed for several days before opening it. Then the solution (containing the precipitant) was decanted and its radioactivity was measured using a different geometry than in the first experiment. The mixture was found to contain a significant amount of radioactivity due daughters of thorium, as in the first experiment. The radioactivity of the mixture was significantly smaller than from the initial solution, as illustrated below:

		Initial solution	Final mixture
	peak (keV)	net counts/hr	net counts/hr
^{228}Ac	911	195 +/- 14	75 +/- 9
^{212}Pb	239	1849 +/- 44	612 +/- 25
^{212}Bi	727	76 +/- 9	31 +/- 6
^{208}Tl	583	459 +/- 22	147 +/- 13

7) An additional experiment was conducted to demonstrate that only a small fraction of thorium, and its daughters, was deposited on the electrodes during the processing. This allowed authors to conclude that a significant part of the initial radioactivity was destroyed in 30 minutes of processing.

Appendix 1, comments

Appendix 2, speculations

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45) Experiment in which I participated

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1) Imagine the following experiment. The lead nitrate salt, which is not radioactive, is dissolved in distilled water. Two zirconium electrodes, inserted into the solution, are connected to the output of a transformer whose effective difference of potential is 300 volts. A current of several amperes is allowed to flow through the solution. Intensive arcing and sparking takes place inside the liquid. After about thirty minutes of processing the voltage is turned off and the radioactivity of (a) solution, (b) electrodes, and (c) precipitate are measured with a NaI gamma ray detector. That was the first experiment in which I participated last week in Salt Lake City. No radioactivity exceeding the natural background was found.

Why would anyone bother to conduct such an experiment? Because in earlier experiments (1,2) the thorium nitrate was used under identical condition and a sizable reduction of radioactivity was observed. Was this an indication that nuclear reactions were induced by the current or was it simply an indication that some thorium (or its daughters) escaped into the atmosphere? The easiest way to answer this question was to use lead nitrate, which is not radioactive, and to process it in the same way as thorium nitrate. If thorium and its daughters are transformed via electrically-induced nuclear reactions then the same is likely to occur in lead. Finding radioactivity in lead, after processing, would confirm the reality of highly unusual nuclear reactions.

Hal Fox, who invited me to his lab, still thinks that nuclear transmutation might occur producing stable (not radioactive) elements. That would be highly desirable; think about turning radioactive waste from nuclear reactors into stable products. That is much better than keeping them in Yucca mountain for tens of thousands of years. To test for this possibility the non- radioactive products (several grams of powder) were collected. They will be sent (as soon as funds become available) to a chemical lab to analyze the content. Was the initial lead (or at least a big fraction of it) transformed into elements that were not initially present? That is the question to be answered. Personally I do not expect to hear about new elements. But I am sufficiently open-minded to accept experimental evidence of nuclear transmutations, if such evidence becomes convincing.

2) An experiment to verify the destruction of radioactivity of thorium nitrate, as described in item 44, was also performed by using a gamma ray detector (sodium iodide) connected to a multichannel analyser. The results were negative; the radioactivity after processing (for dominant gamma ray peaks) was not essentially different from that before processing. The activity before processing was measured by placing the thorium nitrate powder on top of the detector, the activity after processing was measured by using essentially the same geometry. Gamma rays after processing were emitted from the remains of electrodes and from the residual powder removed from the vessel after all water was allowed to evaporate.

3) Let me add that the laboratory in which I worked is small. But it has interesting results on separation of precious metals from rocks. The current financial situation of the company is expected to improve.

Addendum (3/11/03)

I think it is appropriate to described what happened after I left the lab. The best way to do this would be to show my replies to messages I received. The first message described an experiment involving thorium nitrate; the gamma activity “after sparking” turned out to exactly the same as “before sparking.” I wrote:

1) I agree with Jin's conclusion (no significant loss of activity resulted from sparking in this trial). Therefore I see no need for using my new program [based on Bateman's formulas]. It was a challenge to create it. Let me know if changes in activity are observed again. Then we can see how this can (or can not) be explained. Keep in mind that a single experiment can never be trusted.

2) I read your two papers (1997 and 1998) again and this reminded me that your processing before was done in a totally different reactor (high pressure and high temperature). Therefore the new observation, even if it is confirmed in the future, is not necessarily in conflict with what was reported before. May I suggest that you use the old reactor again and analyze spectra under the constant geometry condition.

3) The only reason we used open beakers was to create an experiment that teachers could perform. In that sense we did not succeed.

Next came a message from Hal which I quote entirely in my reply below.

On Tuesday, Mar 11, 2003, Hal Fox wrote:

> I am sorry that I didn't find the papers from the Cincinnati group. They spent
> much time and much money in working with the reactor under higher pressure
> and temperatures. The major results in their work showed a considerable
> difference in the before and after chemical analysis.

You can not be responsible for what they published.

> However, one thing that we know is that we can obtain precipitates when we work
> with radioactive (and non-radioactive liquids). We KNOW that we can reduce the
> degree of radioactivity in the liquid (obviously, there is radioactivity in the precipitates).
> However, the before and after measurements of just the solution was not made during
> the recent experiments. It will be interesting to bombard some of the lead precipitates
> and see if there is also new elements involved after the use of the new type of proton
accelerator.

Somebody reading your two papers (1997 and 1998) is likely to think that the claim was about nuclear reactions taking place, not about the removal of radioactivity from the electrolyte. If I were the coauthor I would insist on publishing (at least in JNE) a short statement that tentative conclusions, described in these papers, were not confirmed in more recent experiments. But I would wait for the result of what you are planning to do before publishing the correction.

> The use of the new type of particle accelerator (which we hope to have running within
> a very few weeks) will enable us to demonstrate that the radioactive precipitates can be
> properly bombarded with the HDCC combined with protons (low-pressure, hydrogen
> atmosphere) and transmuted to stable elements.

That would be an extraordinary accomplishment. I would be very happy to participate in this project. The success here depends on the ability to generate HDCC carrying protons. Instead of trying to transform heavy nuclides, such as Th , etc. I would begin with very light nuclides because here the coulomb barrier is much lower. What about the reaction Cockroft and Walton used (in 1930) to confirm E=m*c^2 experimentally, for the first time? They bombarded Li-7 with protons (whose energies were up to 0.7 MeV) and observed two alpha particles. Another possibility would be to produce N-13 by bombarding C-12 with protons. The half-life of N-13 is 10 minutes and it emits positrons of up to 1.2 MeV. Positrons annihilate with electrons producing easy to detect 0.51 MeV gamma rays. Keep in mind that I have no data on the corresponding cross sections; one would have to do some literature research to take my specific suggestions seriously. But the general idea of starting with light elements is probably valid.

> I do not understand why there was not some degree of radioactive lowering in the
> recent experiments. While Dr. Jin is working on the new particle accelerator, I'll work
> with another of our group (with better equipment) and see if we can't boost the HDCC
> activity even in a covered beaker. We will also do some more experiments using a

> closed, high pressure, and high temperature environment. We will soon have an ICP
> so that we can make immediate measurements of the before and after chemistry as
> rapidly as we perform experiments.

4) Let me also add that according to what I found today (7/2/2003) at

http://www.earthtech.org/experiments/sparkly/report.html

no evidnce for excess heat was found from a well designed "undewater sparking" experiment. The author was inspired by the results of Hal Fox.

Appended on 5/25/08
1) Today I posted the following message on a private Internet discussion forum for CMNS researchers: "The essence of our CMNS controversy is the 1989 claim made by Fleischmann et al. -- a nuclear process is triggered by a chemical process. Please help me to compose a list of experimental effects that were used to validate the claim. Here is the beginning of my list:
1) Excess heat is orders of magnitude larger than what can be attributed to known chemical reactions.
2) Excess heat (produced in a cell whose electrolyte is made from heavy water) is correlated with production of 4He. The amount of excess heat is close to 23 MeV per atom of produced helium. In other words, helium is the nuclear ash.
3) The isotopic composition of an element, after a chemical (atomic or molecular) process is significantly different from the composition before this process. Diffusion of a gas through a solid, by the way, is an atomic process.
4) Energetic nuclear particles, not attributable to cosmic rays or contamination, are detected during a chemical process.
5) The amount of a radioactive substance is REALLY changed during a chemical process (by much more than expected on the basis of the half-life).

The "really" is worth emphasizing. I once participated in a demonstration in which the destroyed-radioactivity effect turned out to be apparent, and not real. Radioactivity was redistributed during high-voltage electrolysis and the counting geometry changed. What was originally in the electrolyte was deposited at the bottom of the cell. A more careful experiment, using the same call, showed that the amount of radioactivity, after the experiment, was essentially the same as before the experiment. What else should be added to my list?

Shortly after that, X wrote to me in private:
Ludwik, you stated/** on the CMNS List that you had personally participated in a demo which sounds to me very much like the Cincy Group LENT-1 Radwaste Remediation Reactor of my late friends Stan Gleeson and Don Holloman. Could you please be more specific about the time/place/participants and who did the before-and-after testing?

My reply:
1) Yes, it was something similar to Gleeson's cell. But our experiment was at normal atmospheric pressure (in an open glass beaker, rather than in a hermetically closed metallic vessel). Both electrodes were Zr and we used the AC power supply. I was a guest of Hal Fox; it was my first CF experiment (in 2003 ?). The experiment was performed by me and by one of Hal's coworkers. Hal was certainly involved but his participation was not 100%. He was doing other things at the same time.

2) A salt containing thorium was dissolved in water, the NaI detector of gamma rays was placed on the side of the beaker and gamma radioactivity coming from the beaker was measured. Seven well defined peaks, spread over a large range of energies, were recorded in a multichannel analyzer. Then electrolysis started; it lasted for several hours; I do not recall exactly how long. Water was added periodically to the cell to keep the volume of the electrolyte constant. After that the radioactivity was measured again, for the same time as before the experiment. The seven peaks were again seen in the spectrum but their heights were significantly reduced. So much for the apparent effect.

3) After that we performed the experiment in a different way.
a) We used the same amount of salt as before. But we placed it at bottom of the beaker, distributed evenly.
b) We place the beaker on top of the NaI detector and measured the radioactivity (recording the spectrum of seven gamma peaks)

c) We added water to the beaker and waited till the salt was dissolved.
d) We started electrolysis, and kept it going for the same time as before (also adding water periodically)
e) At the end of electrolysis we placed the beaker on top of the hot plate and allowed all the water to evaporate.
f) A solid deposit was formed at the bottom of the beaker. It consisted of the initial salt, pieces of Zr, and who knows what else.
g) The beaker was then placed on top of the same NaI detector, in exactly the same position as before electrolysis.
h) Radioactivity was measured, for the same duration as before electrolysis. Each of the seven peaks, after electrolysis, was nearly the same as before electrolysis.

The experiment was designed to produce a clear yes-or-no answer to a well formulated question--is gamma radioactivity reduced or not reduced during electrolysis. And the answer was no. The apparent reduction of radioactivity (in the first experiment) resulted from the fact that its distribution after the electrolysis was not the same as before electrolysis. The "centrum of radioactivity" after electrolysis was further away from the detector than before electrolysis. The second experiment eliminated this effect. I wish other CF experiments, in which I participated, also produced unambiguous answers.
P.S.
Please do not share this description with others. It was Hal's experiment, not mine. My only contribution was to perform the experiment as described in 3 above.

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46) Charlatans versus scientists

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An interesting paperback book was published in 2003 by Keith Tutt in England. The title is "The Scientist, the Madman, the Thief and the Lightbulb." The book begins with the biography of Nikola Tesla, goes over the alleged discovery of Henry Morey from Salt Lake City (1930's) and then focuses on recent episodes, including that of cold fusion. In chapter 11, entitled "Of Charlatans, Conspiracies and Skeptics" the author gives a description of schemes by which con artists take advantage of naive expectations of many investors and convince them to finance unreasonable, often non-existing projects. One recent episode of that kind involved an Australian manipulator, Brian Collins.

He was claiming to invent a miraculous energy-making machine "measuring just 12 inches by 3 inches and weighing only 10 pounds. During the tests the machine was said to produce enough electrical energy to completely power an average size home..... With the availability of unlimited amounts of affordable electric energy, individuals can at last pursue their creative aspirations in a new age of society. In a longer description of the device, published at the same time, the claims are even more generous: "The prototype .....produced electrical energy in excess of 1000 kW, enough power to satisfy the energy needs of 100 domestic dwellings at average load demand."

Exploiting naive desire to get rich from free energy Collins wrote: "The special few who sent funds ..... for every dollar that they sent, they'll see more money than they ever believed possible,.....[I]n the next few weeks there could be some amazing things happening that could see many, many times the funds returned to everybody." But several months later, when money was collected, he apologized that he had been misled by his scientists about the status of the generator. The only persons who benefited from the fraud were Collins and his associates.

In reading numerous books critical of cold fusion I never encountered an accusation of fraud directed to Fleischmann and Ponds or to those who carried out additional investigations in thirteen years after the initial announcement. I saw accusations of misinterpretation, lack of expertise, self-delusion and inappropriate methodology but no accusations of deliberate deception or fraud. The only exceptions were words attributed to an MIT professor Ronald Parker. According to a journalist, Nick Tate (Boston Globe, May 1, 1989), cold fusion was denounced by Parker as "scientific schlock." But in a news conference next day the professor denied using these words.

On the other hand, I encountered one accusation of fraudulent manipulation of data on the part of critics of Fleischmann and Pons. According to E. Mallove, the chief science writer in the MIT's press office (who had access to nearly all the information that was put out by the Plasma Fusion Center) there was a deliberate campaign to discredit cold fusion. Those who orchestrated the campaign (Parker among them) were motivated by the desire to protect their own projects supported by the government ($ 200,000,000 per year). Mallove wrote that "MIT as a whole did, indeed, acquire the deserved reputation as a 'bastion of skepticism' on cold fusion. . . . They suspected that..... if the public were to have a too open-minded attitude toward..... the cold fusion, funding for their [hot fusion] program would be endangered. Assuming this to be true one is tempted to criticize the motivation based on financial self-interest. Scientists are expected to be objective in the analysis of experimental facts and theories. But how can one be impartial when asked to evaluate a competitor?

On page 139 Tuff shows two sets of plots summarizing a calorimeter experiment conducted at MIT. The draft plot, dated July 10, 1989, shows evidence of excess heat, as reported by Fleischmann and Pons. That evidence, however, was removed from the final plot, dated July 13, 1989. I (who discovered the contradiction) asked for the original data but his request was ignored. Mallove believes that this was a conspiracy designed to influence the US Department of Energy.

By the way, a colleague sent me a copy of an interesting article of R. Park. It was published in The Chronicle of Higher Education (vol. 49, issue 21, page b20, 2003). The title is “The Seven Warning Signals of Bogus Science.” You can still access this article at : http://chronicle.com/free/v49/i21/21b001.htm

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47) Catalytic Fusion

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Selected articles on cold fusion, from the "Infinite Energy" magazine, can be downloaded from:

http://www.mit.edu/afs/athena/mit/users/r/e/rei/www/CFdir/CFhome.htm

You can find a 1998 interview with Martin Fleischmann and the description of the catalytic fusion, the so-called "Case effect." It is named after Dr. Les Case from New Hampshire. Let me summarize briefly this interesting effect. Case is a chemical engineer who searched and found a catalyst to sustain cold fusion. His approach is not based on electrolysis and it promises to be very efficient. The catalyst is commercially available "activated carbon doped with precious metals such as palladium."

About 50 grams of that material were placed in a closed vessel into which hydrogen gas was introduced. The material was heated at a constant rate to keep the constant temperature of 180 degrees C. Then, without changing the rate of heating, the hydrogen gas was replaced by deuterium. The temperature started to rise till the level of 215 degrees was reached, after about three days. That temperature remained constant for many weeks. The author interprets this rise of temperature as an indication of fusion of deuterium nuclei into helium nuclei. What else can be responsible for the fact that the temperature of deuterium is 35 degrees higher than in ordinary hydrogen? Neither the amount of gas nor the geometry were changed? The rate of heating the catalyst was also the same in two cases.

At the end of the experiment the deuterium gas was removed from the vessel and subjected to an analysis. It was found to contain helium. No helium was present in the gas before it was processed. The concentration of helium, after about one month of processing, turned out to be 11 ppm. This is significantly higher than the concentration of helium in air (5.2 ppm). If the initially pure gas were contaminated with 10% of air, which is highly unlikely, then the amount of helium would be 0.52 ppm. This is is about 20 less that what was actually measured. The author, a chemical engineer working in his basement laboratory in New Hampshire, wrote: "My objective always has been not to play around scientifically, because I am not a physicist, but to head toward commercialization." Then he elaborated on what would it take to make practical generators, first at the domestic 5 kW level and than at the level of 100 MW power plants.

A scientist from the SRI (Stanford Research International) laboratory, Michael McKubre, worked with Case equipment and used his own high resolution mass spectrometer (able to distinguish ^4He from $^4D_2$). After confirming accumulation of helium he makes a classical "scientific method" attempts to justify it presence by trivial factors, such as leakage or contamination. Failing to do this he arrives to a conclusion that helium is really PRODUCED in the chamber: McKubre wrote: “If we observe helium in our experiments it’s either because it leaked in from the atmosphere—we can rule that out by the blanks that we do and the fact that the helium signal that we have seen is larger than the helium in the ambient. It’s possible that the helium preexisted in the sample and was simply released to the gas phase with long term exposure. We can rule that out largely because we’ve analyzed the catalyst that we’re using and found that it contains no measurable levels of helium. The only possibility that remains, and remains to be checked, is that the helium is produced by a nuclear process. “ Is this analysis an example of pathological science? I do not think so; everything that I read in the article seems to be 100% scientific. On what basis could this serious and careful investigation of McKubre be accused to be pseudoscientific?

The scientist also makes an attempt to correlate the amount of helium produced with the amount of the excess heat generated. Expectations are not hard to formulate. It is well known the energy released from $^2D+^2D-->^4He$ fusion is 23.8 MeV. In hot fusion this energy appears in the form of gamma rays, in cold fusion it seems to appear in the form of heat. That is one of the mysteries of cold fusion. Assuming the excess heat is generated at the level one watt, and using the above 23.8 MeV value, one can easily show that $2.62*10^{14}$ atoms of helium must be produced in each second. McKubre recognizes that his apparatus was not actually built to do very accurate calorimetry. But it is good enough to say that the rate of helium production is roughly consistent with what is expected. He plans to build a better apparatus and to return to this subject in the future. Is this pathological science? I do not think so. It is interesting to speculate how would cold fusion be received by the scientific establishment if Fleischmann and Pons did not announce the discovery of excess heat till the evidence for the commensurable amount of helium could be demonstrated.

The McKubre does not go away from recognizing weaknesses that must be overcome in order to convince skeptics that the effect is real. He wrote: ”One of the difficulties in the cold fusion field is the apparent lack of reproducibility of experiments: many people performing the same experiment get apparently different results; different experiments performed in the same laboratory give apparently different results. So it’s obvious that if you do the same thing you must always get the same result. What this is telling us is that there are some important parameters of our experiments that are not under our control. Some of them I know and understand, and still [we] can’t control some of these parameters we don't know about yet. We just don’t know what the process is that we are studying, so we don’t know what parameters we need to control in order to yield a consistent result. An experiment which always gives the same result --can be performed in several different laboratories to yield the same result -- would be very valuable to us, in part in helping to convince the remaining skeptical scientists in the world that there is a phenomenon to observe. But, in fact, in order to use the scientific method to observe scientific results, we have to be able to reproduce the results of our own experiments so that we can see what the effects of small changes are on these experiments.”

How does this differ from an investigation in so-called “mainstream science?” Preliminary results are always tentative and additional studies are conducted to reach the desirable level of control. After describing his investigations McKubre refers to another very convincing illustration of cold fusion. He describes the work of two Japanese scientists, Arata and Zhang, who also observed accumulation of helium in a very different experiment. McKubre does not hide the fact that this very sophisticated experiment was performed only in one laboratory in Japan.

He writes: “what we are attempting to do here is to produce their same results with their apparatus and with their help. This is a collaborative effort between Arata and Zhang and the SRI group, to produce in *our* laboratory the same results as they have obtained repeatedly over the years, which would indicate that we have some degree of mastery over the experiment. The experiment that we have running here, in fact, is relatively young; it hasn’t been operating for very long. One of the difficulties with Arata’s experiment is that it requires many, many months to produce a result, and quite literally we’re not very experienced with Arata’s methods, so we’ve had some difficulty getting his experiment set up and operational. Certainly, it’s caused me to have an increased level of respect for Arata and Zhang’s technical competence. They are very, very good scientists. Within a month or two, we hope to have reproduced their experiment faithfully and reproduced their result. And the benefit will be in part sociological. We will demonstrate that an experiment can be transported from laboratory to laboratory and yield the same result. It will also give us something that we can do again ourselves and define somewhat the parameter space in which these experiments yield excess heat and, apparently, helium-3 and helium-4.”

Why are experiments of that caliber not evaluated, or criticized, by those who decided, in 1989, as rapidly as possible, that cold fusion is “pathological science?” I view the Japanese experiment as a hybrid between electrochemistry and catalytic fusion. The palladium cathode in their experiment is not a rod, as in many electrochemical setups, but a thin walled vessel whose walls are penetrated by D atoms. The space inside the palladium vessel contains tiny grains of palladium power. Production of helium presumably occurs on the surface of palladium grains. A high resolution mass spectrometer was used to analyze the gas accumulating inside the vessel. The system is designed to establish a correlation between the excess heat and the amount of helium produced. Results are said to be highly reproducible. But the results must be reproducible by all qualified scientists, at least at the level of 20% accuracy, in order to become a firm proof of the reality of cold fusion.

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48) High Density Charge Clusters

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One paragraph was removed from the initial draft at the request Hal Fox. The content of that paragraph, he wrote, might interfere with a current patent application.

During my research trip to Salt Lake City I learned about the idea of high density charge clusters. Kenneth Shoulders has several US patents devoted to technology of charged clusters. He discovered them experimentally (in early 1980's) by observing tiny holes produced by electric sparks on surfaces of several materials. The observations were made by using a scanning electron microscope. Shoulders claims (1,2) that under certain conditions roughly 10^{11} electrons can cluster in a region whose size is very small (one micron or so) and move as a single particle-like unit. That unit, called charge cluster (CC), has the same q/m ratio as a single electron. Accelerated across a potential difference of only 10 volts, for example, the CC would acquire the kinetic energy of 10 eV per electron, or 1,000,000 MeV per cluster. The speed acquired would be $1.87*10^6$ m/s. A small number of protons, for example, one per million electrons, can be trapped in a cluster. These protons would have the same velocity as CCs. In other words, the kinetic energy of each proton would be 18,351 eV. This is 1835 times more than a single proton can possibly acquire from the same difference of potential.

But electrons separated by very small distances repel each others. How can a CC remain stable? This is a theoretical question; I do not know how to answer it. But I did read a paper (3) devoted to this difficult question. The authors claim that CCs are toroidal and that electrons move rapidly with respect to the center of mass. The magnetic field generates forces which overcome the repulsion. It is hard for me to accept this explanation; I know that it would not work for two beams of electrons traveling along parallel paths in the same direction with the same speed. Why not? Because the attractive magnetic force (per unit length) is always smaller than the repulsive force dues to electrostatic forces.

According to a recently published book (4) Shoulders described the idea of CC to Richard Feynman. The great scientist rejected the idea at first. But then he wrote (5): "when you were in my office I could not see how 10^{10} or 10^{11} electrons could be kept as a ball in a vacuum without ions. So I was skeptical and didn't let you tell me about them. I must apologize for it has come to my attention that it is indeed possible . . . Now that I understand how it might work, I should be glad to discuss it again anytime you wish." Jin and Fox (1) refer to several theoretical papers devoted to CCs (6,7,8).

References:

1) See, for example, U.S. Patent Patent 5,054,046, issued October 1, 1991.
2) Kenneth Shoulders, "EV, A tale of Discovery", 1987, published and available from the author, P.O. Box 243, Bogeta, CA, 94922-0243
3) Shang-Xian Jin and Hal Fox, "Characteristic of High-Density Charge Clusters: A Theoretical Model. Journal of New Energy, vol 1, #4, Winter 1996, pages 5 to 20.
4) Keith Tuff "The Scientist, the Madman, the Thief and their Light Bulbs." Pocket Books, London, 2003 (ISBM-07434-4976-2)

5) R. Feynman's personal letter to Shoulders (quoted on page 223 by K. Tuff).
6) G.A. Mesyats, "Ecton Processes at the Cathode in a vacuum Discharge," Proceedings of the XVIIth International Symposium on Discharges and Electrical Insulation in Vacuum, Berkeley, CA, pp 721-731, July 21-26, 1996
7) R. W. Ziolkowski and M. K. Tippet, "Collective Effects in an Electron Plasma Systems Catalyzed by a Localized Electromagnetic Wave," Phys. Rev. A, vol 43, #6, pp 3066-3072, 15 March, 1991.
8) P. Beckmann, "Electron Clusters, Galilean Electrodynamics, Sept./Oct. vol 1, #5, pp 55-58, 1990.

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49) AFTER MY COLD FUSION NOTE WAS REJECTED BY THE PHYSICS TEACHER

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My cold fusion note, submitted to The Physics Teacher, was evaluated by two referees. Their recommendation was not to publish it. The editor wrote to me: "Our editorial staff has completed its review of your manuscript 'On Reproducibility of Data in Cold Fusion Experiments.' The process included consultation with two of our referees. I regret to inform you of our decision not to publish the paper. The first referee offered only brief comments: 'I don't think The Physics Teacher is the right journal for this paper. Most readers of this journal are in no position to judge these rather esoteric matters. Indeed, most teachers wouldn't know what the fuss is all about.'

The second referee's comments are attached [see below]. In the light of the referees' comments and of our own careful reexamination of the manuscript, we believe that TPT readers would not be able to make sufficient use of the paper to warrant its publication. While we are not able to use the manuscript you submitted, we appreciate having had the opportunity to read it and we are grateful for your continued interest in The Physics Teacher." The second referee wrote:

"I must recommend against publication of this paper in *The Physics Teacher*. Despite the claim of 'helping us teachers,' the paper seems actually to be an attempt to have a serious journal endorse cold fusion as a research field to be taken seriously. The pages of *The Physics Teacher* are not the appropriate place to make such arguments. The author says that 'its main claim, the discovery of unexplained heat, was not challenged.' The quoted assertion is totally incorrect; the claim was indeed seriously challenged. And the author dismisses the observation that those neutrons were not observed (and never have been observed, to my knowledge) as somehow being merely a 'tactical error' on the part of Fleischmann and Pons. I don't understand how that statement is supposed to get around the lack of neutrons. Whatever the tactical failings of F&P might have been, they do not hide the absence of those neutrons. Even if there were some validity to the now 14 years' worth of attempts at cold fusion (I haven't heard of any), *TPT* is about the last place in the world for this to be published."

The purpose of this note is to share my paper over the Internet and to react to the comments made by the second referee. The last sentence shows that this referee is not aware of recent cold fusion publications. why was s/he selected? Can a nuclear process of some kind be triggered by a chemical process? The answer, based on what I know about nuclear physics, is negative. On the other hand I read descriptions of many experiments which seems to indicate the opposite. These experiments were performed many years after the first evaluation of "cold fusion" was made by DOE. They were performed by reputable scientists in several countries. As a physics teacher I would very much appreciate a second evaluation of the field by a panel of competent investigators. I am no longer comfortable saying that "cold fusion is pseudo-science."

ON REPRODUCIBILITY OF DATA IN COLD FUSION EXPERIMENTS

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Those who followed the discovery made by Fleischmann and Pons (1) are likely to remember that the number of teams confirming cold fusion was about the same as the number of teams not able to confirm it. The claim was that palladium loaded with deuterium ions generates thermal energy which can not possibly be attributed to chemical processes. According to (2,3,4) the reproducibility of the effect is much better today than it used to be. But that does not mean that the experiment is easy.

Here is how the situation was described by experts (3). "Most scientists hold the view that anomalous effects in deuterated metals can be explained by experimental errors. Some scientists go so far as attributing positive results to self-deception and even fraud and consign this phenomenon to the realms of Langmuir's 'Pathological Science.' Due to the lack of experimental reproducibility, this field remains practically defenseless against such attacks. To our knowledge, no laboratory can provide detailed experimental instructions to another laboratory and guarantee the reproducibility of the excess heat effect. Nevertheless, considerable knowledge has been gained concerning experimental conditions that favor the excess heat effect. . . Our experiments indicate that the lack of reproducibility is due largely to unknown and uncontrolled variables contained within the palladium stock.

. . .[Our results] have been used to support both sides of the scientific controversy regarding anomalous effects in deuterated metals. Our first set of experiments conducted over a 6-month period (25 March - 7 September 1989) produced no significant evidence for any excess enthalpy produced. . . [Other groups] also reported no evidence for excess heat, thus greatly impacting the general scientific opinion regarding this field. All three [of these] groups discontinued their experiments after only a few months of investigation. We continued to investigate other palladium samples and eventually observed significant evidence for excess enthalpy from the use of Johnson-Matthey palladium rods. In retrospect, it would be impossible for any research group to adequately investigate the multitude of variables involved with this field in only a few months. These variables range from the palladium metallurgy to the D_2O purity, the type of electrolyte and its concentration, the electrochemical cell, the electrode arrangement, the type of calorimeter, proper scaling of the experiments, the handling of metals, the current densities used, the duration of the experiments, the loading of deuterium into the palladium, the use of additives, and so on."

This was written in 1996, seven years after the field of cold fusion was declared a scientific fiasco (5,6). I strongly recommend (2,3,4) to those who would like to build an electrolytic setup demonstrating the reality of excess heat. It is clear to me that naming the unknown phenomenon "cold fusion" was most unfortunate. The new (awkward) name is "LENR-CANR," where LENR stands for "low energy nuclear reactions" and CANR stands for "chemically assisted nuclear reactions." Yes, I know that LENR has already been chosen for something else and that CANR contradicts everything we know about atoms and nuclei. On the other hand I believe that those who now routinely observe excess heat are experienced and honest scientists. The term AE (anomalous energy) is also used, instead of cold fusion, when the emphasis is on excess energy. Personally, I prefer to retain the original CF abbreviation interpreted as "cold furor" (7)." In any case the discovery of CF was not a passing episode; several hundred people have been working in the area since the big excitement of 1989.

Up to now the field, far from being a fiasco (6), still can not be called scientific because highly trained scientists have often been unable to reproduce each other's experiments, even qualitatively. But due to persistent efforts of many dedicated researchers the CF field is becoming scientific. According to (4), "excess power levels below 20% are routine [but considerable skill is required] while values above 100 % are rare. The highest reported excess power is 1500% using palladium deutride made in a fused-salt

electrolytic cell at 450 ^{o}C." Theoretical models will probably be developed as soon as basic facts are finally recognized. Practical applications are possible but it is probably too early to speculate about them. On the other hand, I think that discussing CF with students is highly appropriate, especially in the contexts of the methodology, history and sociology of science.

The early history of CF is outlined in (6), and in other books (7,8,9,10,11,12). According to (6), the year during which the discovery of CF was announced was like a soap opera. "Confirmations, retractions, new positive claims and null results were the order of the day…" partly due to the very unconventional form of the announcement -- via a press release, rather than publication in a peer-reviewed journal. Furthermore, many important details were not initially available to those who wanted to conduct similar experiments, most likely on the advice of lawyers handling patent applications.

The essence of the discovery, as described in (1), can be summarized as follows. Heavy water was decomposed in an electrolytic cell whose cathode (negative electrode) was palladium and whose anode (positive electrode) was platinum. The amount of energy entering the system was found to be smaller than the amount of energy exiting the system. The law of conservation of energy requires that a process of some kind must be responsible for the anomalous energy. The hypothesis put forth by Fleischmann and Pons was that the AE was generated via fusion of atomic nuclei. The only reason to advance that hypothesis was the realization that the generated heat was too large to be attributed to a chemical process. This, however, antagonized scientists familiar with nuclear fusion; how could they accept an ad hoc hypothesis contradicting accepted concepts?

But there was an additional, equally good, reason for rejecting the hypothesis. Fusion of deuterons (atomic nuclei of deuterium) is known to produce neutrons, protons, tritons and, very rarely, ^{4}He. Assuming that the fusion hypothesis was correct, scientists quickly calculated the amount of byproducts from the reported magnitude of AE. The actual amount found was much lower than what was expected; the discrepancy was at least ten orders of magnitude. This did not prevent Fleischmann and Pons from saying: "it is evident that … nuclear processes must be involved." I think that linking an experimental discovery with a hard-to-accept
conclusion (and calling it evidence) was a tactical error. Another tactical error was saying that the discovery was ready for practical exploitation.

The panel of scientists appointed by the US Department of Energy correctly concluded, in their Executive Summary (5), that "the experimental results on excess heat from calorimetric cells reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. In addition, the Panel concludes that experiments reported to date do not present convincing evidence to associate the reported anomalous heat with a nuclear process. The Panel also concludes that some observations attributed to cold fusion are not yet invalidated."

There would have been no need for the first two Panel conclusions if Fleischmann and Pons had not made those tactical errors. And the third conclusion was not at all negative; how can a discovery of a totally new, and difficult to observe, phenomenon be validated in less than one year? In other words, the CF field was criticized for its two marginal claims; its main claim -- the discovery of unexplained heat -- was not challenged. The field was nearly discredited by two premature claims. The last sentence of the Executive Summary stated that the Panel was "sympathetic toward modest support for carefully focused and

cooperative experiments within the present funding system." As a teacher who, until very recently, was not paying attention to developments taking place in the area of CF I would welcome its revaluation. Dedicated scientists working in that area would probably also welcome the initiative. The reevaluation might lead to the end of the isolation in which they found themselves after the Panel Report to the United States Department of Energy was published. And it will help us teachers.

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Comments on what the second referee wrote

I was wrong to write “the discovery of unexplained heat, was not challenged.” Many scientists were not able to reproduce the experiment described by Fleischmann and Pond. Given a chance I would correct the unfortunate assertion. The reviewer also wrote: “the author dismisses the observation that those neutrons were not observed as somehow being merely a ‘tactical error’ on the part of Fleischmann and Pons.” Yes, neutrons were never observed in the amount commensurable with the amount of heat. This supports the view that the reaction (claimed to take place) was not thermonuclear fusion, as we know it. Instead of saying that “something unusual is taking place” Fleischmann and Pons said “it is nuclear fusion.” That is what I meant by tactical error. The biggest mistake, however, was to announce the discovery prematurely.

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50) Why Cold Fusion Has Been So Hard to Explain and Duplicate

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INTRODUCTION

Cold fusion (LENR) has been difficult to duplicate and many proposed explanations have not helped to solve this problem. I suggest the difficulty is caused by an erroneous assumption. LENR reactions obviously do not occur everywhere in nature. Painful experience has shown that they occur only in special and difficult to create materials, the so-called NUCLEAR ACTIVE ENVIRONMENT. In the past, people have assumed that the NAE is in the bulk of a Pons-Fleischmann (P-F) palladium cathode. The surface of the cathode is assumed to be inert and only serves to modify the composition of the underlying beta-PdD. In contrast, I'm proposing that the surface layer itself is the NAE.

The concept of NAE is difficult for people trained in physics to accept because phenomenon studied by physics, particularly nuclear physics, are independent of chemical form. For example, when sufficient plutonium is brought together in one place, the resulting nuclear reaction does not depend on whether the material is the carbide, nitride or oxide. However, in the case of LENR the environment is very important. When the proper environment is achieved, the nuclear reaction occurs spontaneously, with no additional effort required. A person does not have to understand the mechanism of the process. Unfortunately, most theories have focused on the mechanism rather than on the environment. To make matters worse, the mechanisms are applied to ideal, imagined materials, not to those existing in the real world. As a result, little useful progress has been achieved.

DISCUSSION

To discover the properties of the NAE, it's location on the cathode must be determined. The following observations suggest that it is located in the surface region of the cathode.

1. Helium generated by LENR is found in the surrounding gas rather than in the palladium deuteride.

When helium is placed in palladium using decay of dissolved tritium, the resulting ^{3}He can be removed only by heating near the melting point [1]. Only helium located near the surface is able to leave the material without this treatment. Therefore, the observed anomalous ^{4}He must have been generated very near the surface of a P-F cathode to be observed in the surrounding gas.

2. Anomalous tritium is detected in the electrolyte rather than in evolving gas.

Tritium that is known to be present within the cathode can be displaced by deuterium during electrolysis. This tritium is found to leave with the evolving deuterium gas [2]. However, tritium produced during a cold fusion reaction is found in the electrolyte, provided a recombining catalyst is not present in the cell. Tritium is able to enter the electrolyte as T+ because it is able to exchange with D+ at the cathode surface before it has a chance to form DT molecules, which would leave as gas.

3. Anomalous heat is observed to be generated on the cathode surface.

Szpak et al. [3, 4], using an IR camera, observed hot spots that turned on and off in rapid secession on an active cathode . The generated energy was claimed to come from just below the surface.

4. Transmutation products are located only in the surface region.

A number of studies have detected transmutation products located within the surface region of P-F cathodes. These elements do not exist within the bulk material[5-9].

5. Melted regions are observed on the cathode surface.

Microscopic examination of the surface shows many melted regions after anomalous energy has been generated[10].

6. Large amounts of anomalous energy can be generated using thin films on an inert substrate.

Significant anomalous energy has been generated using very thin films of material[11-13]. If a large amount of palladium were required, as would be present in bulk material, these thin films would have produced very little anomalous energy.

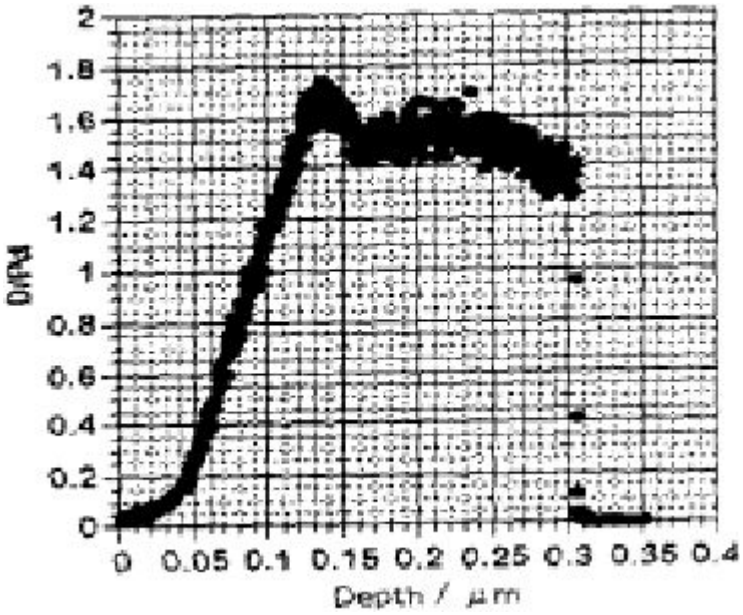

FIGURE 1. Deuterium concentration in Pd after electrolysis based on ERDA. [14] Some deloading from the surface is evident.

Next, the deuterium content of the NAE needs to be discovered. Unfortunately, this property is difficult to measure. Reported values are always based on the average composition, not the composition of the surface. Two efforts to measure the composition near the surface are shown in Figs. 1 and 2. The D/Pd ratio of the NAE will be greater than these values.

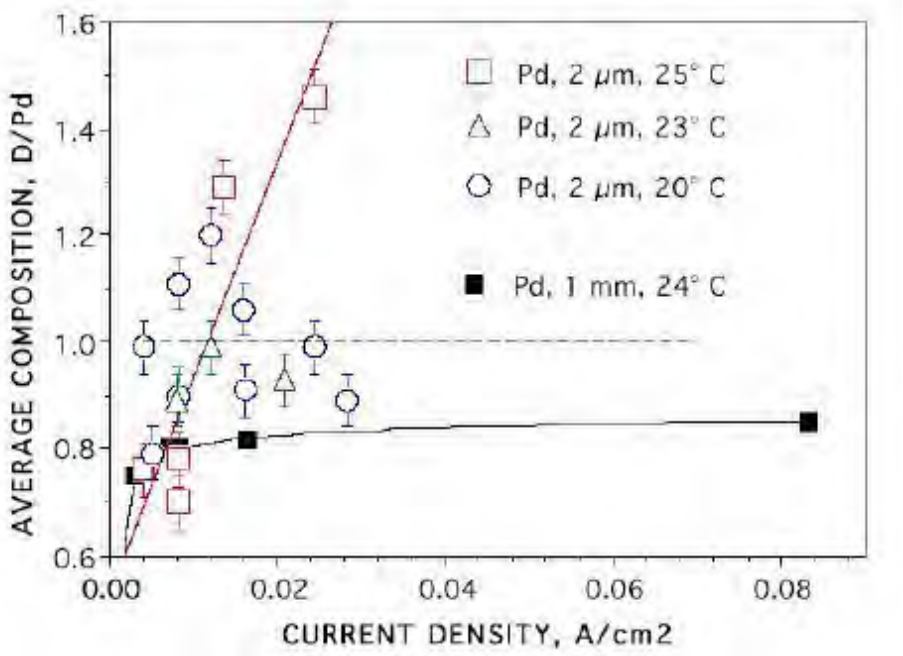

FIGURE 2. Deuterium content in Pd films plated on Pt after electrolysis based on orphaned oxygen. [15] Film thickness and temperature are indicated.

These measurements indicate that the NAE has a composition in excess of PdD1.7. The beta phase has a limiting composition of PdD1.0. Therefore, the NAE can not be beta-PdD. Of course, some people will argue that the additional D+ goes into tetrahedral sites. However, no evidence exists for occupancy of this site. In fact, if this site were occupied, a change in crystal structure would be expected as experienced by hydrides formed by other metals. For example, other metals form hydrides having a limiting composition of MH2, in addition to MH. An example is shown in Fig. 3 for the Zr-H system. The ZrH_2 structure is fcc tetragonal, as would be expected if D+ occupied both tetrahedral and octahedral sites and had an ordered arrangement. If tetrahedral sites were occupied, it is safe to conclude that they would be present in PdD_2 not in beta-PdD. Also, such structures have the potential for the presence of deuterium dimers. Besides having a very high D/Pd ratio, the surface also contains a high concentration of Li, Pt and Si, along with many other elements[8, 16, 17]. Consequently, the surface is a complex mixture of many elements with a very high D/M ratio and an unknown structure.

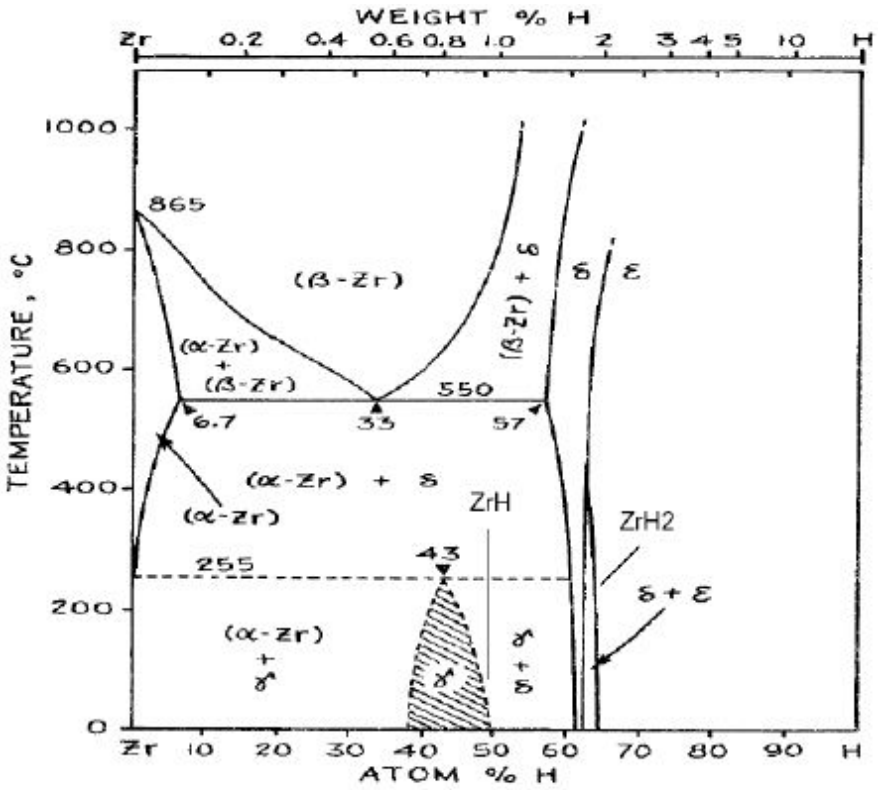

FIGURE 3. Zr-H system at 1 atm H_2 pressure. (GE Handbook of Phase Diagrams).

What other characteristics might the surface layer have? LENR has been produced from finely divided palladium using palladium-black [18, 19] or fine powder attached to charcoal[20]. Even microparticles of other substances imbedded in palladium are found to be active[21]. Examinations of cathode surfaces show the presence of similar microstructures as a result of the electrodeposition process. In addition, successful thin films, as mentioned previously, also contain this type of microstructure. Apparently, structures in the nanometer range of size are frequently associated with anomalous energy production.

CONCLUSION

1. Pure beta-PdD, regardless of its deuterium content, is not the environment in which LENR occurs during the Pons-Fleischmann effect. In addition, the complex alloy in which the effect occurs in this case apparently requires a very high deuterium content.

2. Nearly pure beta-PdD, when it is used as small particles, appears to be active at a relatively low deuterium content.

3. LENR requires nanosized particles and can involve various complex materials, not just palladium.

4. Most theories, mechanisms, and explanations are inadequate because they have not taken these factors into account.

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51) Ten Years of US Navy CF Program

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I would like to thank William Beaty for telling me about a recent article (New Scientist, March 29, 2003, pp 36-43) on cold fusion research in US navy laboratories. The author is Bennet Daviss and the title is “Reasonable Doubt.” The article is based on research conducted at the US Office of Naval Research laboratories and, presumably, on interviews with some scientists. The reports on the navy research is now available over the Internet.

www.spawar.navy.mil/sti/publications/pubs/tr/1862/tr1862-vol1.pdf

The name of the report, edited by P.A. Mosier-Boss and S. Szpak, is "Thermal and nuclear aspects of the Pd/D_2O system: a decade of research at navy laboratories." It downloads as a pdf file whose size is 3.6 Mb.

At the beginning of his article B. Daviss write: “After more than 200 experiments, conducted over 10 years at various navy laboratories, several of its researchers are willing to declare that these laboratories have played host to events that not only indicate that cold fusion is real, but that can't be explained in any other way.” The author reminds us that “cold fusion has been as respectable in [mainstream] science as pornography in church.” The big issue was how to use public money to support research in that area. The laboratory had a large number of highly qualified electrochemists who were eager to study cold fusion after it was officially declared it to be pathological science. The work was done on “time not claimed by assigned projects . . . using discretionary funds controlled by their department chiefs.”

The article describes work of Pamela Mosier-Boss and Stanislaw Szpak in one navy California laboratory (in San Diego) and that of Melvin Miles in another (China Lake). Seeing preliminary results of this work the “navy research officials decided to treat their scientists' cold fusion research a little more seriously. Up to this point the cold fusion work at the navy labs had been informal - experiments were carried out in researcher' "spare time", funded by their department chiefs' discretionary budgets. But after Miles, Szpak and Boss had been at their benches for three years, they had collected enough evidence to convince those higher up the ladder to formalize their efforts.” A limited budget was allocated to continue cold fusion research.

“From the beginning, the idea was to keep things modest. ‘We put less than $1 million a year into the program,’ Nowak says. ‘Above that level, the red flags go up.’ Saalfeld and Nowak never gave the program its own line in the ONR's budget, but allotted money to it from miscellaneous funds. ‘We were to keep working and we were allowed to publish our results, but we weren't supposed to say a lot about it,’ Miles recalls. ‘Some people were worried that word would get out and it would jeopardize the navy labs' funding from Congress for other research. We didn't even call it 'cold fusion'. We called it 'anomalous effects in deuterated systems'.’ “

But research work was not easy and results were not always reproducible, as illustrated by the following description: “In July 1992, Miles received Imam's first attempt at making a suitable electrode, a palladium-silver alloy. ‘It produced nothing,’ Miles recalls. ‘Energy in was equal to energy out.’ For almost two years, while Boss and Szpak logged success after success, Imam sent Miles a steady stream of palladium alloys, and even various forms of unalloyed palladium. None produced any excess heat at all.” But things changed (in the summer of 1994) when alloyed samples of pure palladium with boron were tested. Using these new materials Miles was able to observe excess heat from eight out of nine cells. But why didn't the ninth one work? The answer came from Imam who engineered new materials. He found that negative electrode used the ninth cell was very different from those used in the eight heat producing cells. Instead of being microscopically smooth it had numerous cracks on its surface.

“A correlation between cracks and null results has been noted by many researchers, before and since. So the researchers had evidence of excess heat. They had also seen telltale evidence of nuclear reactions in the form of tritium and otherwise inexplicably large amounts of helium.” But this work had to stop in the next year. “By 1995, after watching Miles trying and failing to wring excess energy from Imam's electrodes, Saalfeld and Nowak decided to stop giving the project any more money. .....With the money gone, Szpak and Boss moved on to other projects. Miles wasn't so lucky. In 1996, Nowak left the ONR, robbing the navy's cold fusioneers of their front-line champion. Around the same time, Miles's boss left, and his replacement discontinued the discretionary funds that had been supporting the work. To make things worse, Miles couldn't find other work. ‘I couldn't get ONR funding for anything,’ he says. After failing to find new projects to take on, in 1997 Miles - with an international reputation and more than 100 publications to his credit by that time - was reassigned to work as a clerk in the stock room.”

But all this did not destroy confidence of scientists. They are still willing to resume research if it is funded. One of them wrote: “Something is going on, though, and the navy may eventually see fit to investigate it further.” In 1997 Mill was invited to participate in the Japan's New Hydrogen Energy Program. “During that time, he ran 11 experiments and three control tests. Of these 11 experiments, 10 yielded anomalous energy. These included tests that used Imam's palladium-boron blend, and three new tests of the co-deposition method. When he returned, Miles wrote papers detailing some of his results, which were published in 1999 in Fusion Technology and a year later in the Journal of Electroanalytical Chemistry and Interfacial Electrochemistry. Boss is also ready get back on board if the work is funded. And though Szpak has now retired, he still comes in to the San Diego lab to work at refining the co-deposition technique, supported on a shoestring budget that Gordon, his department chief, supplies.”

Anticipating future development scientists believe that the revival should be modest. One of them said: "If you put a bunch of money into this, you'd probably have the same result you had in 1989 - a lot of unqualified people would start working on it and we'd begin to convince ourselves again that this can't work." Clarifying this another scientist said "The worst thing that could happen to cold fusion is to make a big blip on the scientific radar screen again. [The area] needs a modest amount of funding - a few million a year with a firm, multiyear commitment - run by people who aren't political and are more interested in the science than they are in building their resumes. The energies being reported are vastly too big to be chemical in origin. But that still leaves a huge question. Where the hell is all that energy coming from?"

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52) Another Summary

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After rereading the 1997 paper of T. Muzino et al., entitled “Changes in Isotopic Distribution of the Elements on Palladium Cathode Electrlolyzed in D_2O Solution,” I decided to summarize it, and add some comments. The paper describes a standard cold fusion experiment (palladium cathode, platinum anode and the heavy water electrolyte) lasting seven days. Chemical and isotopic composition of the cathode surface revealed dramatic changes in chemical and isotopic composition. The authors, affiliated with prestigious universities and research centers, took extraordinary precautions to prevent contamination. Four analytical methods were used to analyze samples: 1) energy dispersive X-ray spectroscopy (EDS), Auger electron spectroscopy (AES), secondary ion mass spectroscopy (SIMS) and electron probe microanalysis (EPMA).

Figure 1 shows distributions of peaks detected via the X-ray spectroscopy. Except for palladium, all peaks belong to elements which were originally absent. Note that the vertical axis scale is logarithmic; the numbers represent counting rates from a high resolution (GeLi) detector of X-rays. The labels "before" and "after" refer to results obtained before and after the electrolysis. The strongest argument against contamination, as far as I am concerned, are isotopic compositions of reaction products. Like Karabut (see item # 13 at < http://blake.montclair.edu/~kowalskil/cf/ >, the authors found that isotopic distributions are significantly different from those found in our environment.

An alchemist would say that palladium, on the surface of the electrode, was transmuted into Pb, Hg, Pt, Sn, Cd, Zn, Cu, Ni, Co, Fe, Mn, Cr, Ti, Ca, S and C. But unlike medeval alchemists the authors know that transmutation of elements involes changes in atomic nuclei. They are aware that nuclear processes responsible for the above products are highly unusual; their existence conflicts with everything we know about nuclear reactions. Why are all newly produced isotopes stable? How can nuclear processes take place despite repulsive electric forces acting between the atomic nuclei? Nobody can answer these questions today. Fully aware of this S.Szpak and P. Mosier-Boss wrote: “ We should like to suggest that ‘theory guides -- experiment decides’ is a preferable approach in this area of research. . . .

Immediately after the Fleischmann and Pons announcement in 1989, the scientific community became divided into believers and skeptics. The first group reported the results of their research with enthusiasm. At times their enthusiasm overstated the significance of their results. The skeptics, on the other hand, rejected these results as a matter of conviction. One often heard argument ‘this is contrary to our understanding of nuclear physics,’ thus stating that what we know is all that has to be known.

Irrespective of the position taken by the skeptics, one cannot dismiss experimental facts. As long as data are outside the experimental errors, they must be accepted regardless of our understanding and whether or not they can be easily reproduced. The facts stated in this communication were checked and rechecked for accuracy with details given in their respective references.”

Note 1: The above quote was found in a 1997 paper, entitled "Thermal and Nuclear Events Associated with Pd + D codeposition."

Note 2:
Both papers were fetched from the IEEE XPLORE data base. They were downloaded as reports from the “32nd Intersociety, vol 2.” The report was also referred to as “Energy Conversions Engineering Conference, 1997.”

Note 3: Tadahico Mizuno is the author of a book "Nuclear Transmutations, the Reality of Cold Fusion." It can be ordered at www.amazon.com ($30 new or $16 used).

Note 4:
Suppose that four probes were deposited on our planet by creatures living somewhere else in the universe. Conditions in their place are totally different from ours. The probes landed in the same place but several days apart. The first showed a wind of 50 mi/hr blowing north, the second and the third showed no significant wind while the fourth showed a wind of 30 mi/hr blowing to the west. Should the investigators conclude that the idea of wind, formulated after the first reading, is erroneous? Certainly not. They should continue to investigate till the variable nature of winds is understood. Likewise, the irreproducibility of cold fusion experiments should be studied till the phenomenon is understood. Declaring it to be pathological science makes no sense to me.

Note 5:
I have no idea why the scientific contributions of Mizuno et al. (and many others) are usually rejected as pseudo-science. Something is not right.

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53) Cold fusion and microscopic cracks

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Montclair State University, Upper Montclair, N.J. 07043, (4/14/03)

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In reading cold fusion papers i often saw references to fusion inside tiny cracks (size 0.1 to 1 microns) existing on metallic surfaces. The claim is that strong electric fields are often generated inside the cracks (which define boundaries between grains of different materials). The field can supply deuterium ions with a kinetic energy of 10 to 100 keV. The nuclear fusion cross sections, in this energy range, are known to be between about 0.03 and 30 mb. Bursts of neutrons and tritons, occasionally detected in cold fusion experiments, are said to be produced via traditional hot fusion in tiny cracks. According to a recent paper of M.Di Giulio et al. (from the University of Leecy, Italy) production of new elements in cold fusion is strongly enhance when cracks are present. What follows is my short description of their work. The original paper was published in the International Journal of Hydrogen Energy (27, 2002 pp 527-531); its tittle was "Analysis of nuclear transmutations observed in D and H loaded Pd films."

Thin palladium films were deposited on silicon wavers. Some surfaces were rough while others were smooth. The films were then loaded with H_2 and D_2 (by keeping them for one week in chambers filled with these gasses at the pressure of six bars). The films were then exposed to the UV light from an pulsed laser (308 nm, 1 Hz), presumably to trigger plasma oscillations inside cracks. "The laser fluence was always lower than 25 mJ/cm^2 to avoid palladium ablation." Surfaces were examined by SEM (scanning electron microscope), chemical compositions were determined by EDS (energy disparsive X-ray spectroscopy). The elements found in laser-treated films were Ca, Fe, S, Zn, Ti, Cu and Cr; their distribution over surfaces were closely correlated with locations of cracks. No quantitative information about the rates of transmutation were given in the article; the emphasis is on how to create surface irregularities rather than on production of new elements.

I am primarily interested in transmutation. That is I want to end this short summary with a quote from a paper of David .J. Nagel (*Accountability in Research,* 2000. 8: p. 137) about production of new elements in cold fusion experiments. The author wrote: "Transmutations of heavy elements were reported first in 1995 when the Pd electrodes in a heavy water electrolyte became radioactive and exhibited gamma ray spectral lines appropriate to neighboring elements of Pd. Later, the coated-sphere experiments and solid-electrolyte experiments lead to reports of the generation of numerous elements across the periodic table, with many of the same elements increasing in concentration within both of the very different experiments. All of these problems, the diverse experiments and the complex results continue to make study of the field of cold fusion challenging."

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54) What do critics say about cold fusion?

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In an attempt to be "objective" I decided to read and review a recent book written by a strong opponent of cold fusion, Robert Park. The title of the book is "Voodoo Science; the Road from Foolishness to Fraud" (Oxford University Press, New York, 2000). In one way this book is similar to a Russian book, "The Highwaymen of Science" written by Edward Kruglijakov (Nauka, Moscow, 2001). In both cases criticism of cold fusion is mixed with criticism of fraudulent "science," such as astrology, homeopathy shamanism and antigravity machines. Referring to these areas Park writes that the claims "are totally, indisputably, extravagantly, wrong, but nevertheless attract a large following of passionate, and sometimes powerful, proponents." Kruglijakov's book is full of similar pronouncements. In one place he writes: "How can all this be explained? Why is it that poorly educated con artists can influence the thinking of so many? There are many causes. General social upheaval and the ideological vacuum created by the abolition of old ideas led to a situation in which tired, disillusioned individuals started to believe in miracles."

It is remarkable that both authors belong to scientific establishments. Park directs the Office of Public Affairs of American Physical Society. Kruglijakov heads the Russian Academy of Sciences Committee created "to "fight pseudo-science and fraud in scientific research." The authors are highly educated scientists; I would expect them to be familiar with serious cold fusion publications, such as those summarized on my list <http://csam.montclair.edu/~kowalski/cf/>. But their criticism totally ignores recent findings and focuses on "easy targets," such as Joseph Newman or Dennis Lee. Both books expose fraud and scientific ignorance very well but their characterization of cold fusion is not correct. Many practitioners of cold fusion research are serious scientists and the field should be described in terms of recent findings, not in terms of mistakes made more than ten years ago. The cold fusion scientists who I met recently are very critical of unjustified claims and deplorable public relation practices. All of them agree that the initial announcement of the discovery was premature.

In one place Kruglijakov does refer to the findings of Karabut (see "Russian Connection" on my list). But instead of summarizing the results, and arguing against them, he simply disqualifies the data and calls them ridiculous "miracles." The author probably thinks that experimental findings inconsistent with current paradigms cannot possibly be accepted. Likewise, according to Park, cold fusion is a discredited claim that "may have little or no scientific support." I strongly disagree with Park's claim that "cold fusion is no closer to being proven than it was the day it was announced." Let me focus on this pronouncement. The situation in 1989 was summarized, for example, in the highly critical ERAB report. From a purely scientific point of view the main objections were:
1) Absence of reaction byproducts "commensurable" with the amount of excess heat.
2) Irreproducibility of "excess heat" experiments.
3) Absence of theoretical explanations

How can the work of Arrata and Zhang (published in 1997) be ignored when addressing the issue of byproducts? These authors demonstrated (in six reproducible experiments) that the amount of helium produced is commensurable with the expected energy of 24 MeV per event. Do the critics think that data reported by the Japanese are in error? Do they think the data are fraudulent? The same question can be asked about findings of Karabut et. al., about work of Mizuno et al. etc. etc. All these researchers are recognized experts (with the highest possible degrees); I don't believe they are charlatans or con artists.

It is true that experiments are still not 100% reproducible, but the situation is no longer as bad as it was in 1989 or 1990. According to Storms, much progress has been made in recognizing causes of irreproducibility. It should be clear that the lack of reproducibility can mean at least two things. Those who perform experiments may not be sufficiently qualified (not be familiar with significant nuances known to other scientists). Some influencing, but still unknown, factors are not under control of experimentalists. To illustrate this second point let me refer to electrostatic experiments. Every physics teacher knows that they are often "irreproducible." Today we say that the outcome of a demonstration depends on humidity but early investigators were not always aware of this. They probably believed that frictional electricity experiments were irreproducible. Why should irreproducibility be associated with "not being real"? It should be associated with "we are not aware of some important factors."

Let me illustrate this with something I personally experienced. Several weeks ago. I was heating milk in a microwave oven for two minutes. Then I removed the cup and inserted a cold spoon into it. At that moment I observed sudden "explosive boiling;" a clear indication that superheated milk was created. I tried to reproduce this event next day, and several days later. I used the same oven, the same cup, the same amount of milk, etc. but without success. Does it mean that my observation was not real? I do not think so. It only means that some important factors, perhaps the air temperature or pressure were different in subsequent experiments. Or perhaps the rate and angle at which the spoon was inserted into the cup were not exactly the same. After reading about hundreds of successful experiments in the area of cold fusion I am convinced that, unless all authors are liars, the reported phenomena were real. Cold fusion phenomena seem to depend on factors which are hard to identify, as indicated by Edmund Storms (see items 50 on my list).

I am not a theorist but I have read about several attempts to understand cold fusion, including the work of Nobel laureate J. Schwinger. Some researchers have emphasized the role of collective behavior in solid materials, as in the Mosbauer effect; others think that neutrons trapped in solids play an important role. It is true that these attempts have not produced a generally accepted theory, and that such a theory is needed to guide experimentalists. But its absence is not a valid argument against experimental findings. The entire field of cold fusion is still at the fact-gathering stage, a stage at which electromagnetism was before Faraday and Maxwell. Let me refer to a wise observation by Stanislaw Szpak and Pamela Mosier-Boss: "theory guides -- experiment decides." This is a preferable approach in the area of cold fusion. Rejecting scientific data that do not agree with the current paradigm is not scientific.

P.S.

What follows is an email message I received recently:

Dear Mr. Kowalski,
Help! My name is XXX XXXXX and I am a sophomore at XXXXX High School. In my chemistry class, I am doing a project on Cold Fusion. I was looking on the Internet for websites on Cold Fusion, and I came across your links to your Cold Fusion items. I was wondering if you could give me some advice or information? I would like to know what Cold Fusion is, [and] how Cold Fusion was started. . . . .

I am no longer comfortable saying that "cold fusion is voodoo-science." I am a physics teacher; how should I answer questions about cold fusion?

Can a nuclear process be triggered by a chemical process? The answer, based on what we know about nuclear phenomena, is negative. On the other hand many experiments seem to indicate the opposite. These experiments were

performed many years after the first evaluation of “cold fusion” was made by our Department of Energy. As a teacher I would very much appreciate a second evaluation of the field by a panel of competent investigators. What can one do to make this happen?

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55) The Smoking Gun?

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The discovery of the catalytic approach to cold fusion, by Case, has already been described in item 47 on my list. The description also referred to the work of McKubre et. al. who confirmed the observation of helium in spent cold fusion fuel. One of the major criticisms of Fleischmann and Pons was their inability to demonstrate presence of that byproduct. After rereading McKubre's article I want to focus on its main points. But first let me observe that McKubre and two of his coauthors are from the Stanford Research Institute. The fourth coauthor is a professor from MIT. Would Robert Park call them misguided scientists? Would he qualify their work as voodoo science? These highly qualified people have been studying cold fusion from the year in which it was discovered. Why did Park ignore scientists of that caliber and used Rendall Mills as an example of a cold fusion researcher? As far as I can tell Mills' proposals are only marginally connected with cold fusion; they are based The Grand Unified Theory book described at the Black Light Power company website < http://www.blacklightpower.com >

The article which I am summarizing here is entitled "*The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd System: Evidence for ^{4}He and ^{3}He Production*." It was a paper presented at the 8th International Conference on Cold Fusion, Italy, 2000. Like many other publications of leading cold fusion researchers the article can be downloaded from the library at: < http://www.lenr-canr.org >. The authors begin by listing the "apparent anomalies in carefully performed experimental studies of D/Pd and H/Pd systems. Such anomalies include:

1) prompt emission of electrons and charged particles
2) unexplained heat in excess of known input sources
3) the residual presence of light elements (notably ^{3}H, ^{3}He and ^{4}He)
4) the possible occurrence of isotope anomalies in higher mass elements
5) unusual electrical conductance effects both stable and transient."

Then they describe four experiments verifying what has already been reported by other scientists -- the correlation between excess heat and accumulation of helium. Here is their descriptions of two experiments:

"2) Loading of D_2 and H_2 into Pd on carbon supported catalyst using modest gas pressures (1-3 Atm.) and temperatures (170º–250ºC). These experiments were designed to test the claim by Case to observe excess temperature and increasing 4He levels under similar conditions. Experiments at SRI were performed in sealed Nupro® 50 cc stainless steel vessels connected to a steel manifold. Periodic measurements of ^{4}He were made by direct connection to an Extrel® mass spectrometer capable of resolving the mass-4 peaks of D_2 and ^{4}He. Using information recorded from temperature sensors placed inside and outside active and reference gas cells it was possible to obtain heat-flow calorimetric information at times when the catalyst bed temperature rise was significant. . . .

4) Closed cell electrolytic loading of D (and H) into hollow Pd cathodes sealed to contain small dimension Pd-black powders. These experiments were performed to replicate published results by Arata and Zhang in which excess heat, ^{4}He and ^{3}He, were found to be associated with the electrolysis of such "double structured" cathodes in D_2O, but not in H_2O. In experiments performed at SRI accurate mass flow calorimetry was used to evaluate and compare the heat production of double structured cathodes electrolyzed in D_2O and H_2O in otherwise identical cells. ..... "

The results, illustrated on four figures, are remarkable. The authors were able to confirm the existence of the previously

observed correlation. Their quantitative results are not exactly identical with those reported by other researchers but discrepancies are not excessive. This work, based on findings of Case, Arata and Zhang, is very convincing evidence that the accumulation of helium in palladium is commensurable with the amount of excess heat. Let me end this short note with a quote from the ERAB report < http://www.ncas.org/erab/ > which discredited cold fusion in 1989.

"The initial announcement by Pons and Fleischmann exhibited the discrepancy between heat and fusion products in sharp terms..... The persistence of this major discrepancy [absence of reaction byproducts in the amount commensurable with the reported excess heat] in all subsequent experiments has led to various explanations as to how the observation of fusion products might be obscured. For example, some have suggested that in a solid the fusion energy is released directly as vibrations of the metal lattice, so that no hard radiations would be observed. In any case, this would not explain the absence of helium or tritium. Helium should be produced in about 50% of all reactions (see Table 1.1). Several electrodes from cold fusion experiments have been examined for helium, but at this writing, none has been reported. The amount of tritium directly observed in some experiments is much too small to account for observable heat. ...."

Why does the energy released in cold fusion, when helium is produced, go directly into the "vibration of metal lattice," rather than to gamma ray photons, as in D+D--> He? This question remains to be answered; the mechanism of helium production is still a mystery.

P.S. A recent publication reporting the results of a several-years-long study (downloadable from < http://www.lenr-canr.org >), by Antonella De Ninno et al., also confirms generation of helium byproducts and focuses on several new effects. Contribution of Carlo Rubia (Nobel Laureate) to this study is recognized by the authors. The title of the report is "Experimental evidence of ^{4}He production in a cold fusion experiment." How many independent confirmations are needed to convince our scientific establishment that cold fusion research is not voodoo science? I think the CF field should reevaluated in light of new findings.

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56) Looks Like Technological Con Artistry

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An example of real pseudo-science was shown in item 16 on my list. It was posted several months ago. To my great surprise I found that the company is still in business. How many naive investors have they found? How much money have they collect from them? Why do people invest in devices without seeing them in operation?
The company website < http://www.genesisworldenergy.com/team.htm > is worth visiting.

In their Q&A section (December 2002) one reads that "commercially viable versions of the Edison Device currently exist, as does the manufacturing capacity to produce and assemble devices." In a more recent statement (March 2003) one reads that "Genesis World Energy has currently received proposals from governments, private organizations and individuals within 59 countries that meet the specified requirements listed in the Licensing Section." Hmm, how can this be verified by a potential investor? Important information about the company, such as names of its researchers, location of laboratories and factories, etc. is not available; it is a company secret.

One reader asked: "Is the information being written and posted on the Internet independently by critics of the Genesis effort accurate?" The answer to this question was: "No. Genesis World Energy anticipated from the beginning that certain individuals and industries would be adamantly opposed to the proliferation of the Genesis Technology and would attempt to stop its market introduction. Thus, we fully expected the dissemination of false information [for example, by scientists]. That is why the only correct source of valid information on the Genesis Project and Team is on this website." Here is what Robert Park, with whom I totally agree this time, comments on their activities.

"Genesis World Energy proposes to free the world from fossil fuel dependence by 'harnessing energy from the molecular structure of water'. The idea is deceptively simple: use electrolysis to separate water into hydrogen and oxygen and then use a hydrogen fuel cell to generate electricity. I know, you're going to say it will take more electricity to split the water than the fuel cell can generate..... The Genesis people say they assembled a team of 400 top scientists. You think these guys never heard of the conservation of energy? 'For security purposes,' Genesis explains, 'the Genesis Team has elected to disclose little about the science behind the technology. In the absence of detailed information, it is not possible to understand how Genesis' stated results were achieved. Therefore, the scientific community at large will analyze the Genesis Project based on conventional thinking.' OK, I'm busted! I've been relying on the First Law of Thermodynamics, which is about as conventional as you can get."
Quoted from < http://www.aps.org/WN/WN03/wn011703.html#2 >

Another critic of the device, John Lichnetstein, wrote: **"***What is Genesis World Energy?* People calling themselves Genesis World Energy have announced the Edison Device, which they say converts water into energy. They claim to be a private general partnership based in Idaho. You can verify for yourself that as of 2003-January-15, they are not registered in Idaho. They might be performance artists. They might be a scam. *What is the Edison Device?* It's been described inconsistently by GWE. When it was first released, it was a miracle that would liberate the world from fossil fuel dependence, a device that converts water to hydrogen and oxygen and back, extracting more energy from the conversion to water than is required for the conversion from water. This is what's traditionally called a perpetual motion machine; it's a violation of the first law of thermodynamics..... *Have there been independent tests of the Edison device?* No such claim is made. GWE claims that they have shown the device to various smart folk, but none of these folks are talking. An anonymous endorsement is uninformative..... *Who is GWE?* The Edison device design team all wish to remain anonymous..... I'm John Lichtenstein, a statistician from California. I hate to see people ripped off." Quoted from < http://slashdot.org/~chipotle_pickle/journal/21169 >

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57) Hydrinos: are they really predicted by Mills' theory?

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What should one think about BLP (Black Light Power Inc.), a company headed by Randell Mills? Unlike cold fusion, which is still a purely empirical field of investigation, his claims seem to be based on theories. And unlike the secretive company, Genesis World Energy (see item 56 on my cold fusion list), Mills does provide information about his scientists, the location of his laboratory, etc. I would very much like to know what a theoretical physicist would say about Mills' theory. With this in mind the following message was posted on Phys-L, an Internet discussion list of physics teachers:

On 4/22/2003 Ludwik Kowalski wrote:
> Randell Mills claims that the ground state of hydrogen
> (ionization energy 13.6 eV) is not the lowest possible
> energy state of the atom? How can it be that such a thing
> was never discovered by spectroscopists?
>
> Hydrogen atoms below the ground state were named
> hydrinos. Mills justifies their existence on theoretical
> grounds. A short summary of his work can be found at:
>
> http://www.blacklightpower.com/theory.shtml
>
> The underlined title is actually a link to his March 3, 2003
> conference presentation. I clicked on it and an impressive
> 1.2 Mb file (157 slides) appeared on my desktop. Unfortunately,
> I am not very familiar with most of what he refers to. Are Mills'
> theoretical claims valid or are they used to impress those of
> us whose familiarity with modern theories is very limited? I
> saw a mixture of familiar concepts and concepts too advanced
> for me. Do Mills' equations justify existence of hydrinos? Are
> his arguments real or are they only a camouflage for nonsense?

On 4/23/2003 XXX replied:
> As best as I can tell there is absolutely nothing to his
> claims. The people at the www.csicop.org have a short blurb
> about it written 6 years ago. Mills (who is an MD not a
> physicist) has not built any such device and is still trying
> to raise capital for his idea. Let's just look at the obvious.
> There is no solution to the basic Schroedinger equations that
> have "fractional" quantum numbers. If he is right Quantum
> Mechanics is sunk and transistors do not work.

Unfortunately, this has been the only reply. In my mind neither the Edison engine nor technologies based on hydrinos have anything to do with cold fusion. These technological claims are based on theoretical predictions unconfirmed by experiments. The cold fusion claims, on the other hand, are just the opposite; strange phenomena have been observed

but they have not been explained by an accepted theory. Referring to hydrinos, Steven Weinberg, a 1979 Nobel laureate in physics, wrote that the idea of the ground state "is a fabulously well-tested mathematical theorem. I would bet my life on it." Then he added: "of course a theory can be wrong; we don't turn a blind eye to anomalies...you don't throw away 75 years because of an anomaly you don't understand. As far as we know, quantum theory is rigorously valid. I have no idea what would replace it." These quotations were found at

http://www.space.com/businesstechnology/blacklight_power_000522.html

together with interesting comments about Mills' theory. Addressing the same subject Robert Park, (the author of "Voodoo Science; the Road from Foolishness to Fraud," Oxford University Press, New York, 2000), wrote: "His 'theory' reminded me of my thesis advisor's comment when I referred to my first scientific paper as 'a theory'. 'It's a theory,' he said gently, 'to the extent that it was done with a pencil.' Nor had Mills offered any experimental evidence for his claim." Park's book was written for general public. As a physics teacher I would like to know about specific errors, unjustified assumptions, etc. in theoretical papers of Mills and his coworkers. More specifically, I would like some help in finding out what makes the theory ridiculous.

One more aspect of this subject is worth mentioning. It has to do with the role of authority in science. Many think that scientists do not need authorities to validate experimental or theoretical proclamations. This is not always true. A scientist may not have equipment, and training, to perform a sophisticated experiment. Or s/he may have only a limited theoretical background to distinguish valid theoretical arguments from those that are not valid. It is practically impossible to be an expert in everything and, except in his or her own specialty, a scientist is essentially in the same situation as a layman. Many claims are accepted by us when several experts agree that statements are valid. Teachers of introductory courses know how to verify claims made in textbooks. But claims made outside familiar areas are often accepted on the basis of authoritative statements. Recognizing this fact professional journals often publish review articles for their readers. A serious review devoted to the last ten years of research in the area of cold fusion is needed.

P.S. Here are two pieces about R. Mills found on the Internet:

http://csf.colorado.edu/archive/sustain-l/msg00367.html
1) " . . . Dr. Randell Mills, the founder and President of BlackLight Power Company, has a prestigious academic and professional record as a scientist and inventor. He graduated summa cum laude and Phi Beta Kappa in 1982 with a degree in chemistry from Franklin and Marshall College in Pennsylvania, and went on to Harvard Medical School, where he earned the MD. degree in 1986, conducted applied research, and filed patents on several revolutionary medical technologies. Dr. Mills' graduate education also included classes in electrical engineering at MIT, which influenced him to examine certain problems with current atomic theory. .....Since 1989, however, he has devoted nearly all his energies to the development of what he has dubbed the HydroCatalysis process of liberating energy from hydrogen atoms. . . . (5/29/97)"

http://www.njo.com/business/ledger/d8b23a.html
2) Kevin Coughlin (Star Ledger (10/10/99), wrote that Mills is "misguided at best, a sci-fi con man at worst. P.T. Barnum with an F in physics, a perpetual motion machine of ego. If Mills is right, they say, then a century of quantum mechanics -- the bedrock of modern physics that has produced transistors, lasers, telecommunications -- is wrong. 'His theory is totally groundless,' says Paul Grant, a former IBM scientist now with the Electric Power Research Institute in Palo Alto, Calif. But the brickbats haven't kept private investors – including two power companies and a former federal energy official – from pumping $20 million (the company's figure) into Mills' BlackLight Power Co. Inc. They're betting he's the next Einstein, even though some admit they can't understand a word of "The Grand Unified Theory of Classical Quantum Mechanics," Mills' self- published tome. (A thousand pages for $100, at Amazon.com.) If Mills is right, they say, he will transform the world and make them all very, very rich. . . .

Mills says he's found the answer and it's chemical, not nuclear..... And no room-temperature fusion in a jar. The concept of "cold fusion" caused a sensation 10 years ago, but its promise of limitless energy hasn't panned out." Is the author aware that many scientists continue working on cold fusion phenomena? Is he aware that accumulation of helium, missing ten years ago, has been observed by several teams of researchers? Cold fusion, as I wrote above, is a set of experimental claims without an accepted theory, it is not a theory without experimental confirmations.

Scientific theories are justified by their mathematical correctness, and validated by experimental data. And what validates experimental data? Some say that laboratory data must be reproducible, and I agree with this. Others say that reproducibility alone is not sufficient, experimental data must be consistent with other data via theoretical models. I agree that consistency with other data is a desired end product of a research project. But I do not think that consistency is necessary at initial stages of investigations. Comments on reproducibility, in the area of cold fusion, can be found in items 54 and 52 on my list.

< http://blake.montclair.edu/~kowalskil/cf/ >

Experimental facts, not consistent with existing theories, should be suspected to be wrong but they should not be rejected on that basis.

Another P.S. On August2, 2003 I received the e-mail from Norm Winningstad

Dear Dr. Kowalski,
You should know that the self-proclaimed physics gurus [such as the APS spokesman, Dr. R. Park, who claims that no spectral lines of hydrogen atoms exist below 80-nm] are now in serious trouble..... If you simply substitute 1/p for "n", [in the Rydberg equations] the positive integer number of the hydrogen orbit, and let "p" be any positive integer, you get the location of the spectral lines of the hydrino. The reason the spectra of the hydrino was not see before is that it is not commonly found on earth (before Mills.) BUT it WAS seen before, but not recognized. .....The below 80-nm spectra is now peer-reviewed and published, it was done at another lab from Dr. Mills' lab (it was done in the Netherlands.) Dr. Park now needs to shut up, and other skeptics need to have the courtesy of examining the experimental results; they will all learn something new.

* * * * * * *

I replied: Dear Norm:
1) Who are you and how did you find out that I am interested in hydrinos?
2) I agree with you that experimental results are essential. And that R. Park's should examine them before making strong negative pronouncements.
3) Please supply references for the peer-reviewed publications of experimental results.
4) What would be more convincing than experimental results properly interpreted by experts? (a) Confirmation of such results by experts from other laboratories and (b) personal participation in the validation process.
5) Are you one of those who had a chance to participate? I am a physics teacher who was not able to penetrate the depth of theoretical arguments of Mills.

* * * * * *

“Dear Ludwik:
. . . I am C. Norman Winningstad, BSEE, MBA, LL.D, and I worked for the Lawerance Berkeley Laboratories early in my career, . . . The references are now on the BLP home page. The probem is (I have asked them to correct it) the papers are not marked as to what journal they are published in, which complicates the search. ..... I am a techweenie by education, and I understood what Mills was talking about. I went back to his lab, and watched it happen (^_^) But Park and Zimmerman were invited, but it was beneath their dignity, I guess, so they didn't go. But just lately, Pirelli in Italy duplicated the plasma generation tests, and they are hot-to-trot. .....I did a little digging, and I would recommend you go to the BLP web site, and click on the left on Technical Papers. Then at 2nd from the top, click on Abstracts. Then go to #1, about the NASA report. Go to # 45, which has a Journal of Applied Physics peer-reviewed article (2002) Vol. 92, No. 12, pgs. 7008-7022. #49, Journal of Molecular Structure. #62, IEEE Transactions on Plasma Science, Vol. 30, No. 2 (2002) Pgs. 639-653.

I will be the first to admit that it is frustrating to have most of the new articles listed as "Submitted" or "In Press." I assume the first means it is not peer-reviewed yet, and the second, that it is peer-reviewed, but not yet printed (*_*) You will note the list is long, largely because of the crescendo of writing lately at BLP as a result of their experimental work being fruitful, due to years of optimizing. I suspect the reason that they do not name the journals where the papers were Submitted, or where they are In Press, is because BLP experienced having papers through the peer-review process, only

to have the editor over-rule his own peer-review panel, just before printing, because it was leaked to Park and/or Zimmerman, who blew into the editor's ear about how foolish he will look, if he publishes some thing contrary to Schroedinger's Equations (^_~)”

If I had time I would ask Randell Mills, whose labs are in New Jersey, to invite me for a demo. But my priorities are about to shift toward teaching. I will keep this possibility in mind. This fellow, Norm, seems to be overexited, especially in long parts of his messages that were skipped above. He was mostly a businessman. Now he is a writer and a consultant (at $500 per hour). Did he invest his own money in Mills’es company?

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58) An ongoing public debate about cold fusion.

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A teacher from the Phys-L discussion list sent me pieces from an ongoing cold fusion debate at < http://prorev.com/ > . I am posting these pieces as an illustration. The list "owner" is Sam Smith, the author of Piece 3. He is a journalist and a politician. The pieces illustrate interest in cold fusion.

Piece 1
It's amazing to me that none of the people proposing that government spend money on CF have spent any of their own on it, nor found any venture capital -- there are billions of dollars running around out there looking for a good investment opportunity. If CF had 1/1000th of the potential that its backers claim you'd think they'd be fighting to get their money into the pot first. . . oddly, they only want other people to invest in it.

Piece 2
To the physics community, cold fusion appeared unlikely, but if true, exciting, so a rush of labs tried to replicate the P&F result. While most labs had no luck, a few had occasional and marginal success, though their results can not be verified by other labs. This is pretty much what you expect if cold fusion doesn't work. While cold fusion requires only a modestly equipped lab, judging the results of an experiment is difficult. Rare neutrino emissions, a measure of success, are difficult. Small 'excess' heat is the other prime measure of success, but as this is the difference between large energy inputs and outputs it is easy to make a mistake. A well known similar case where measurement is difficult is calculating the power carried by alternating current electricity. Problems with this have mislead good engineers, and over the years has been the basis of a bunch of perpetual motion machines. If cold fusion really worked, I would expect a reliable setup very similar to the P&F one to have been found in the initial rush of experiments. If something pretty simple sort of works and a bunch a people give it a try, some of them will hit the way of making it work. A reliable method of achieving cold fusion would be new physics, and tremendously interesting even if the energy generated was miniscule.

Piece 3:
I was a physics major at U of I, Urbana-Champaign when cold fusion was first reported, and I followed it closely for a while. Over time it became clear the P&F had done shoddy work, that it was easy to get mislead, and that the other reported successes were marginal and unrepeatable. I think the continuing interest is due to the great interest in energy generation, the relative ease of setting up a cold fusion experiment, and the capacity of people to believe what they really want to be true. [Most of what I know about science I learned in high school, but I have followed the confluence of science and politics, a problem that dates back at least as far as Galileo and is as recent as the numerous duplicative Star Wars reports done for the Reagan administration by defense contractors and universities (and their scientists) in support of the former's political agenda and the latter's budgets. As whistleblower Alric Saucier put it, Star wars was "largely a paper program producing research and development studies. The reports are a shameless waste. Multiple contractors are assigned to do the same work and then to do it again and again. As a rule, the studies are not read. They get stored at different locations outside the Pentagon until room is needed for new ones. . ." That was not just bad science, it was fraud. I was similarly attracted to the cold fusion issue because of political, rather than scientific, factors. After the initial Pons-Fleischmann experiments had proven faulty, a number of anomalies developed. Some of the media seemed to go out of its way to beat a presumed dead horse and a couple of anti-cold fusion books even appeared. The

Department of Energy made it publicly clear it wanted nothing to do with the matter.
The Patent Office refused to consider it.

Meanwhile, in other countries research continued, sometimes - as in Japan - with public monies, and some hardy American scientists kept plugging away, all gathering at international conferences notable for media absence. Even Toyota put money into the research, although the Japanese have since slashed their funding. Also in foreign lands was little suggestion that those interested in the subject belonged at Waco rather than in the lab. As one investigator put it, "In the U.S. there is a degree of envy among cold fusion researchers for their Japanese colleagues. In Japan, the debate over cold fusion is polite and scientific. Researchers are not rashly judged or branded incompetent for suggesting cold fusion could be real. Their American counterparts would like to conduct research in a similar atmosphere, without accusations and emotionalism."

The potential import of cold fusion, should it prove valid, along with the economic interests involved - including those involved in conventional energy or getting government money for other alternatives raised the suspicion that some of the opposition might not be scientific at all. The hostility seemed to go beyond skepticism and veered towards political or public relations campaigning. So the Review - in its role as a way station for the new, the imaginative, and the abused - has remained hospitable to the cold fusionists without offering the slightest guarantee that they are right. They simply deserve to have been treated a lot better than they have been. But don't trust me. You can check it out for yourself by attending the Tenth International Conference On Cold Fusion at the Sonesta Hotel in Cambridge, MA in August. Some professor at MIT is the chair. He can probably explain it better than I can.

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59) To be published in a local newsletter

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(May 19,2003)

In March 1989, two reputable chemists, Fleischmann and Pons, announced the discovery of an unexpected heat-generating phenomenon at the University of Utah. This effect, labeled “cold fusion,” was immediately investigated by many researchers, not all of whom were able to confirm it. A controversy ensued; some scientists became skeptical about the reality of cold fusion while others continued to study it. A highly unusual formal investigation of cold fusion, initiated by the US Department of Energy, took place several months later. The report resulting from the investigation was mostly negative and, like most people, I accepted its conclusions. But today, thirteen years later, I am no longer comfortable saying that “cold fusion is voodoo-science.”

What caused me to become more open-minded? It was the 2002 International Conference on Emerging Nuclear Energy Systems (New Mexico). Several papers presented there were devoted to cold fusion research. After the conference I gathered a large number of recent cold fusion papers (available at www.lenr-canr.org) and several books devoted to the phenomenon. Critical evaluation of that material was facilitated by my nuclear physics background. Using the Internet I was able to communicate with scientists who have been studying cold fusion since its premature announcement by Fleischmann and Pons. I was highly impressed by what I read, and by the credentials of many authors. A Nobel prize laureate, J. Schwinger, for example, was trying to construct a theoretical model of cold fusion in the last year of his life.

Conducting research in cold fusion became very difficult in the US after the scientific establishment (NSF, DOE, leading journals, etc.) declared the field pseudoscientific. Financial support for research was cut and many journals no longer offered peer review of submissions in the discredited area. Despite these difficulties some Americans continued to explore the field. Very significant discoveries were made by them, and by scientists from other countries, such as Japan, Italy, Russia and France. It turns out that cold fusion is very complex; no generally acceptable theoretical model has been advanced, as far as I know, to explain the experimental data. The absence of an accepted theory is a great weakness. The term cold fusion usually refers to two kinds of observed phenomena: generation of unexpected heat, and occurrence of highly unusual nuclear reactions. Practical applications of these effects may be possible in the future; speculating about them seems to be premature.

The major criticism of cold fusion studies is that results are not always reproducible. Initially only about 50% of researchers were able to confirm generation of excess heat. The situation is much better today but one still cannot be sure that all experimental results obtained in one laboratory will be confirmed in others. The outcomes of experiments seem to depend on difficult to identify conditions, such as presence or absence of some impurities (at the ppb level) or surface irregularities of metals. It is remarkable, however, that perfect reproducibility in a single laboratory has often been reported when essential materials, such as palladium, originate from the same manufacturer and from the same production batch.

In my opinion, the absence of qualitative reproducibility is a clear indication that the cold fusion field is still a mixture of art and science. But this is not sufficient reason to declare it pseudoscientific. Cold fusion scientists trying to make their field scientific should be supported by government agencies (such as NSF, DOE, etc.); why should they be treated differently than scientists working in other fields? Their papers should not automatically be rejected by our leading journals; they should be given the same chance to be published as papers in other areas of science and technology. How can this be accomplished? If it were up to me I would suggest another formal evaluation of the entire cold fusion field

by a group of appointed experts. That group would visit existing cold fusion research centers, study the reports and participate in experiments. Then, after a year or two, it would issue a report based on data which were not available to the first investigating team.

It is unprecedented that an area of research, conducted by recognized experts, has been officially declared an outcast of mainstream science. Are experimental cold fusion data fraudulent? Are we dealing with a mass-scale self-deception involving hundreds of scientists in several countries? It is difficult to answer such questions; they would be totally irrelevant if the cold fusion field were rehabilitated. An authoritative report by a group of experts would help teachers deal with this unusual situation in the history of modern science.

In order to reach my own conclusion I plan to go to the 2003 International Cold Fusion Conference (Massachusetts). Hopefully, this will result in an invitation to participate in an essential experiment conducted by a recognized expert. As a nuclear physicist I would seek an invitation to confirm the reality of a nuclear transmutation process due to a chemical process. Witnessing one such phenomenon would be sufficient for me to be able to claim that cold fusion is not a self-deception or fraud. For the time being I will continue studying papers available over the Internet and sharing with others what I learn. Feel free to examine my items at http://csam.montclair.edu/~kowalski/cf/

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60) Alchemy; comments on two papers

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The 1996 Japanese paper of T. Mizuno, T. Ohmori and M. Enyo, downloaded from the www.lenr-canr.org, is worth reading; it shows evidence of nuclear transformations taking place in a Pd-Pt-heavy water electrolytic cell. Several analytical methods were used to demonstrate that isotopes of many elements (mass numbers from 6 to 220) were deposited in the Pd cathode during its one-month-long operation. This is highly unusual nuclear alchemy. In the introduction to the paper the authors claim that the accumulation of elements in the cathode can not possibly be due to contamination because, in many cases, the isotopic composition of reaction products is highly unnatural. For example, natural chromium is known to be 4.3% ^{50}Cr, 84% ^{52}Cr, 9.5% ^{53}Cr and 2.4% ^{54}Cr. But the chromium found in the cathode was 14% ^{50}Cr, 51% ^{52}Cr, 2.4 % ^{53}Cr and 11 % ^{54}Cr. The percentage differences are extremely significant; natural variations of abundance of chromium isotopes is known to be less than 0.003%.

Similar observations were made, at about the same time, by two American scientists:
G. H. Miley and J. A. Patterson. Their paper can also be downloaded from the above mentioned www.lenr-canr.org web site. The authors analyzed composition of elements accumulated in a very thin Ni foil used in an electrolytic cell. They wrote: “Following a two-week electrolytic run, the Ni film was found to contain Fe, Ag, Cu, Mg, and Cr, in concentrations exceeding 2 atom % each, plus a number of additional trace elements. These elements were at the most, only present in the initial film and the electrolyte plus other accessible cell components *in much smaller amounts*. That fact, combined with other data, such as deviations from natural isotope abundance, seemingly eliminates the alternate explanation of impurities concentrating in the film.”

Analytical methods used to detect and analyze the accumulated elements in these studies were: secondary ion mass spectrometry (SIMS), Auger electron spectrometry (AES), energy dispersive x-ray (EDX) analysis, electron probe micro analyzing (EPMA), and neutron activation analysis (NAA). Let me describe these techniques briefly.

SIMS: A sample under investigation is placed in a vacuum chamber and high-energy ions are fired at its surface. Known as ‘primary’ ions, they penetrate the near-surface atomic layers and set up chains of collisions between surface atoms. This results in releases of ionized (secondary) atoms from the exposed material. These atoms are analyzed in a mass spectrometer. Surface imaging is possible by focusing the primary ion beam on selected spots of a sample. The depth profile of trace elements concentrations can be obtained by using the ion beam sputtering, that by removing thin layers of material.

EDX: A sample to be examined is used as an anode of an X-ray tube; it is bombarded by electrons whose energies are between 10 and 20 KeV. The characteristic X-rays are analyzed with a high resolution Li drifted silicon detector. The method is used to study chemical composition of materials at the depths of up to microns. The sample can be scanned to perform surface imaging.

AES: A sample under investigation is placed in a vacuum chamber and bombarded by a beam of electrons whose energies are sufficient to ionize atoms. This results in the emission of characteristic X-rays, as in the EDX, and in the radiationless deexcitation process producing Auger electrons. The energy spectra of these electrons are analyzed to identify individual chemical elements and to determine their concentrations near the surface. Note that Auger electrons generated in deeper layers do not escape from samples. Chemical surface imaging is possible by scanning the surface with the focused beam of electrons.

EPMA: It is essentially a scanning electron microscope designed and optimized for X-ray analysis of elements from small areas. An X-ray spectrometer is used to identify elements, scanning is used to perform chemical surface imaging.

NAA: A sample under investigation is first exposed to a beam of neutrons, usually in a nuclear reactor. It is then analyzes with a detector of nuclear radiation. The nature of radiation is indicative of the average chemical composition of the irradiated sample.

The instruments used are sophisticated and only highly qualified scientists are able to use them. Pseudoscientists would not know what to do with such instruments, or how to describe observations correctly. And, yet, the entire cold fusion field is often said to be pseudoscientific. By the way, the authors of the above two articles are affiliated with highly reputable research centers: Department of Nuclear Engineering of Hokkaido University, Catalysis Research Center of Hakkaido University and Fusion Studies Laboratory at the University of Illinois. They are veteran scientists and recognized authorities in several disciplines. On what basis can their research be disqualified as pseudoscientific?

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61) How will cold fusion be remembered?

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I have no doubt that cold fusion will be remembered as an unusual event in the history of science. Hundreds of qualified researches (Ph.D.s, publications, recognitions, etc.) are practically blacklisted. In most cases discoveries are debated among scientists without formal investigations initiated by government agencies. Without interventions debates often continue for as long as necessary. In some cases discoveries are eventually validated by other scientists; in others they are shown to be wrong. Occasionally debates linger without leading to definite conclusions; then they die due to lack of interest. But that is not what happened in the case of cold fusion. Less than one month after the discovery was announced the US Department of Energy prompted a formal investigation. In the request to investigate, dated April 24, 1989, one reads:

“In recent weeks, there has been a great deal of interest in the prospects for ‘cold fusion’ based on experiments at universities in Utah and subsequent experiments performed elsewhere. At present, the apparent observations of cold fusion and significant quantities of energy from this phenomenon are being investigated extensively. Because of the potential benefits from practical fusion energy, I request that the Energy Research Advisory Board (ERAB) assess this new area of research. Specifically, I would like the Board to:

1. Review the experiments and theory of the recent work on cold fusion.
2. Identify research that should be undertaken to determine, if possible, what physical, chemical, or other processes may be involved.
3. Finally, identify what R&D direction the DOE should pursue to fully understand these phenomena and develop the information that could lead to their practical application. I request that the Board provide an interim report on the first item by July 31 and a final report on all items by November 15, 1989".

I think that addressing the issue of “practical applications” was premature and that the special investigation of cold fusion by an appointed board was a mistake. Nine months after the initial announcement at the University of Utah the board issued its final report. In the executive summary of that report one reads:

"The Panel concludes that the experimental results on excess heat from calorimetric cells reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. In addition, the Panel concludes that experiments reported to date do not present convincing evidence to associate the reported anomalous heat with a nuclear process".

This was a correct summary of the situation in 1989. But why did the announcement of Fleischmann and Pons trigger a special investigation? The answer has to do with highly unusual circumstances at the University of Utah described in many books. The president of the university wanted to establish an extraordinary research center to harvest benfits from an unconfirmed discovery. In the same executive summary one reads:

"The Panel recommends against the establishment of special programs or research centers to develop cold fusion. However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system."

In other words, the final recommendation was to treat the announced discovery in the same way as any other scientific claim, to continue studying the phenomenon in different research centers. But that is not how the report was interpreted by our scientific establishment, that is by directors of research granting agencies and by editors of major scientific publications. For them the report was a red light to stop supporting cold fusion research and to stop publishing results of scientific investigations in that area. They saw the report as an authoritative pronouncement that cold fusion was

“voodoo science.” This accusation is still used today.

Three reasons for skepticism about cold fusion were enumerated in the report. They were: a) irreproducible data on excess heat, b) absence of expected nuclear reaction products, and c) conflict with the existing theory of the so-called “nuclear tunneling” effect. The report shows that nuclear generation of heat, at the level of 1 watt, must be associated with between 10^{11} and 10^{12} reactions per second. It means that an electrochemical cell generating 1 watt of excess heat should produce approximately 10^{16} reaction products, such as ^{4}He. No evidence for the accumulation of helium was available when the report was written. But the situation today is very different; several researchers have reported accumulation of helium at the expected rate.

Excommunication of researchers on the basis of what they are investigating is unprecedented in the history of science. Two different scenarios can be anticipated. In the first scenario cold fusion is not validated (even after considerable efforts to demonstrate its reality). In the second scenario the reality of cold fusion is finally accepted by the entire scientific community. In one case one would have to explain a strange phenomenon of long-lasting self-deception involving hundreds of scientists in many countries. How do we know that the phenomenon of self-deception is not more common among scientists than we are willing to admit? In the second case a long-lasting conflict between scientists and administrators must be explained. What are the causes of this conflict? How does it differ, for example, from the conflict between geneticists in the Soviet Union and communist ideologists of that country?

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62) Scientific American and cold fusion

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A letter to the editor of Scientific American was sent Edmund Storms in November of 2002. That letter has not been published. Today I downloaded the unpublished reply, (available at http://www.lenr-canr.org) and the correspondence it triggered. The extracted pieces can be used by those of us who discuss the issues of "science and society" while teaching physics. What follows are: Storms' letter to the editor, the unpublished reply of the editor, quotes from other correspondence, and my short comments.

Storms' Letter:
Dear Sir: Your analysis in the December issue about why science is neither respected nor understood by the general public I found to be very much to the point. Those occasions when science accepts claims that are later found to be false clearly give science a black mark. Unfortunately, in an effort to avoid such embarrassment, science also rejects claims that are later found to be true. I ask you, which is the greater threat to science and mankind, accepting a claim that can have no possible benefit or rejecting a claim that can have great benefit?

I could offer many examples of how good ideas have been rejected in the past, but I would like you to consider one very important claim that now has almost universal rejection, yet is supported by a growing body of data. As a scientist, I was trained to judge the reality of nature from good data based on replicated experiments. Yet, I find that the scientific community increasingly bases what is real on the opinions of a few respected journals and academics using theoretical arguments, regardless what is being discovered by other scientists operating in the real world. How is the general public expected to respect science when it does not follow its own stated rules of evidence?

The discovery I would like to use as an example of this double standard is what is called LENR or low energy nuclear reactions. This has also been given the very inaccurate name of cold fusion, a name that now causes rejection and ridicule. This ridicule comes from people who have no understanding about what is now known, yet their opinions are accepted as fact. Is this the way science is supposed to operate?

If you wish to be true to your stated wish to make science more respected, I suggest you educate yourself about this important phenomenon by reading information available at www.lenr-canr.org. There you will find over a thousand publications that support the reality of such anomalous nuclear reactions, as well as several reviews in full text that answer important questions raised by skeptics in the past. Serious scientists rejected "cold fusion" in the past for good reason. These reasons no longer apply. If science cannot correct a past rejection, then what good is the scientific method? Can anyone respect a scientist who cannot change his/her mind after being presented with better data? Respectfully, Edmund Storms, Ph.D.

A Response from the Editor of Scientific American, John Rennie:
Dear Dr. Storms: Thank you for your email proposing that Scientific American reevaluate the status of LENR-CANR research and consider publishing an article on the subject. As you suggested, I did look over a number of the offerings at www.lenr-canr.org. Unfortunately, I still don't see evidence in those papers, or in the mainstream physics literature, that LENR-CANR has achieved any significantly new level of credibility in the eyes of the general physics community. The site does point to a large number of publications that ostensibly offer evidence of the phenomenon, but sheer numbers of papers is not sufficiently compelling--as I'm sure you know, even the creationists can point to thousands of

"publications" and "scientists" seemingly supporting their position.

I noticed that the LENR-CANR site, on its page appealing to readers to "help spread the word" about the phenomenon, described Scientific American as having "gone far out on a limb opposing cold fusion. They, along with the leaders of the American Physical Society and the Department of Energy, have made their institutions into bastions of opposition to cold fusion." We at SciAm don't feel that we've gone out on a limb in criticizing cold fusion harshly in the past. As for our being a bastion of opposition to it, I don't think we have an intractably committed position. If LENR-CANR can be demonstrated satisfactorily for acceptance by the physics mainstream, we would be more than happy to publish more favorable articles about it. Your problem starts with establishing more credibility in their eyes, not ours.
Sincerely, John Rennie

Comments:
Referring to the above, Jed Rothwell (the manager of the lenr-canr web site) wrote: “Rennie has dismissed 2,000 papers, including research from laboratories such Los Alamos, the Naval Research Laboratory and Mitsubishi Heavy Industries, mainly by equating these papers with creationism. Waving your hand and saying that "X is like creationism" does not make X resemble creationism. Cold fusion is an experimental finding, which is completely unlike creationism, or actual evolutionary theory, for that matter. It is not based on theory or an interpretation of the fossil record, but rather on long-established laboratory instruments and procedures in calorimetry, mass spectroscopy and so on. The only way to disprove cold fusion is to find technical errors in the techniques that are applied by specific cold fusion researchers.”

Only highly trained scientists using expensive equipment are in a position to validate cold fusion claims. We read their papers and form our own opinion. This fact should be recognized; the situation was very different one hundred years ago. Neither editors of Scientific American, nor we, physics teachers, can perform experiments with sophisticated mass spectrometers in ultra-clean labs. In this new situation the role of a journal like Scientific American becomes even more important than in the past. I have no idea why Storms’ letter was not published. The author is a recognized authority in nuclear chemistry, a research veteran from Los Alamos, one of the best nuclear technology laboratories in the world. Why was he silenced by John Rennie?

Responding to Jed Rothwell the editor of Scientific American wrote “the people you need to convince about the scientific credibility of cold fusion aren't journalists. They're professional physicists who review submissions for respectable technical journals. If you can convince mainstream scientists that LENR-CANR is real and significant, magazines like Scientific American will drop into line.” In other words, he is saying that Scientific American is not a technical journal; its decision to silence a high caliber scientists was bureaucratic rather than scientific. I can understand this. But I do not think that Storms’ letter would do any harm to the general public. On the contrary, it would promote interest in science.

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63) Jed Rothwell's publication

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1) Jed Rothwell is one of those who manage the lenr-canr.org list. Last night I sent him a message making the following suggestion: ". . . My sabbatical project devoted to cold fusion will culminate just before the beginning of the academic year. It will be my participation at the 10th ICCF. During that conference I would like to have a chance to meet and speak with major CF researchers. I also want to write an article about that conference. Perhaps it will be published somewhere; if not it will be shared as another item on my web site. With this in mind I would like to suggest that conference organizers collect statistical information about participants. How many of them (percentage) hold Ph.D. degrees in science and technology? How many . . . I think that information of this kind is worth collecting and sharing. It will show how cold fusion proponents differ from pseudo-scientists. The best way to collect such information, I think, is to prepare an anonymous form and to send it to each person who registers. . . .

Responding to the above Jed wrote: "There will be few people at the conference. Most researchers have retired and are too old or too poor to attend. How many of them (percentage) hold Ph.D. degrees in science and technology? 100%, I expect. I do not know any who do not have a Ph.D.. How many of them have published at least ten papers in their specialties? All of them, I expect. All the ones I know have. People like Bockris and Fleischmann literally wrote the book on the subject. I mean the authoritative textbooks. They are all senior people, because young people would never be allowed to do CF. How many were affiliated with national labs? Not so many. Storms, Miles, Srinivasan and a few others. Most are from universities. How many were university professors? Nearly all, including Miles, who also did a stint at China Lake. How many were research project directors? etc. etc. I wouldn't know. Fleischmann and Bockris were.

2) Jed also referred to a paper he presented at the 1999 conference, and modified this year. It can be seen at http://www.lenr-canr.org/acrobat/RothwellJcomparison.pdf. Let me comment on some points of that paper.

a) Problem of age: Jed wrote: [in the past] "young scientists have made most new discoveries.....They have the instinct to explore, and strike out for new territory. This is considered the natural order of things, and perhaps it is, but it is also a cultural construct. Unfortunately, the culture of science has changed. We punish failure. Research is too formal. Fun in the laboratory is discouraged. Young people have grown up with this system and know no other. Nowadays, old scientists are in the vanguard. They have tenure. They came of age in a more dynamic, liberal era when mistakes and adventures were encouraged, and progress was in full flood. Young scientists are afraid to step out of line. Controversial research will jeopardize their careers. Too much scientific funding is controlled by policymakers in Washington and science journal editors who demand allegiance to established theory."

It is true that exploring cold fusion is a risky business for a young scientists. Who wants to be branded a pseudo-scientist after investing several years in serious investigation of an abnormal phenomenon? Who wants to enter a field in which research grants are not available? Only people with tenure, or retired, can afford to be active in an officially condemned field. That is why another official evaluation of cold fusion is urgently needed.

b) Problem of demonstration device: Jed wrote: "[Eugene Mallove] and I tell scientists to put aside their theories, stop trying to improve their calorimeters, stop trying to improve reproducibility for the time being, and concentrate on a public demonstration instead.....Many scientists respond to our suggestions with hostility, because they do not understand our strategy. They think we are denigrating theory, and belittling the need for rigorous experiments and better calorimeters. We say first things first: get the funding, and then go back to your research. Many scientists dismiss

this as grandstanding or a publicity stunt."

I agree that it would be extremely useful if, instead of exploring many different aspects of cold fusion, the effort of many could be focused on one aspect, such as accumulation of tritium, or emission of totally unexpected alpha particles. A reliable working device demonstrating a single abnormality would convince skeptics that at least one claim of cold fusion researchers is real. Why is it that no such device has been produced in the thirteen years since the original announcement? Limited financial support in the US is only a small part of the answer, I think. Somebody will answer this question in the future.

My trip to Salt Lake City (see items 44 and 45 on this list) was triggered by a desire to produce such a demonstration, at least for physics teachers and students. Unfortunately, the attempt was not successful. At least one claim made under the cold fusion banner was shown (by the initial investigators themselves, with my participation) to be premature. This claim, as far as I know, did not undergo the scrutiny of the peer-review process. Would I be able to construct a device based on a claim made in a peer reviewed article? Which claim should have been chosen if chices were available? How many "failures to qualitatively confirm a claim" would be sufficient to discredit the entire field? Is it too late to ask such questions at the next cold fusion conference? It depends on how many active cold fusion researchers will participate. Not too many, according to Jed.

c) On cycles in support for science: Jed wrote: "generational role reversal is one reason we got into this mess. Another is that science happens to be at a low ebb. It is going through a conservative, uncreative phase in which theory overrules experiments, talented young people ignore science, and experts go around saying this is the twilight of the scientific age. I think science is cyclical, like the stock market. An upsurge begins when a dramatic improvement comes along. Success follows success; excitement causes more excitement. Society rewards the enterprise with bigger budgets, more money. Eventually the process gets out of hand. Too much success carries the seeds of future failure, breeding smug satisfaction, hubris, bloated budgets in science, and irrational exuberance in the stock market. Giant projects like the hot fusion program and the supercollider are botched; the public loses faith, budgets are cut, morale plunges. We reach a low ebb, a crisis. The crisis leads to self-examination, house cleaning, renewal and revitalization, and the cycle begins again."

That is an interesting observation. But is it correct? In the last two hundred years scientific progress was gradual, not cyclical. It was nothing like stock market fluctuations which tend to repeat themselves every ten years or so. I am not aware of similar cycles in physics. But perhaps it has to do with the scale of operations in science; in the past experiments were much less expensive than they are now. How long will scientific progress cycles be? Jed ends on an optimistic note; he writes: "I hope we succeed in my lifetime. Regardless, I have faith that science will recover, and mankind's adventures will begin anew." What is the basis for such optimism? It seems that a generation of cold fusion pioneers is leaving the stage without producing natural successors. What could have been done in ten years may take a century to accomplish in the future. One good thing is that a large fraction of reports describing what has been done is gathered in one place -- the library at the http://lenr-canr.org site.

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64) Two kinds of excommunication

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William Beatty, a subscriber to Phys-L (a discussion list of physics teachers) sent me personal comments on recently published < http://lenr-canr.org/AppealandSciAm.pdf > correspondence with editors of Scientific American. William added: “feel free to use my message for anything you wish.” I decided to show it below as an appendix. In my opinion the issue of cold fusion can be solved only by competent scientists who perform experiments and discuss their meaning. Political statements, like William’s comments, and like some of my own items, are significant in the context of the issue of “science and society” rather than in the context of “validity of claims.”

But science and society topics are profound and worth addressing. One of them attracted my attention recently; it was the “excommunication of genetics” by Stalinists. At first I simply wanted to compare two kinds of excommunication, that of cold fusion in the US and that of genetics in the Soviet Union. But then I started reading about mechanisms of control of science by ideologically motivated “state apparatus.” I think I will continue writing about this in the future. (Even cybernetics was at one time condemed in the Soviet Union as pseudo-science: “psevdo-nauka na uslugakch americanskovo imperialisma”). Here is a very interesting Internet reference (authored by a Russian researcher, Kirill Rossiyanov):

http://eserver.org/cyber/stalin.txt

I did not know that Stalin was personally involved in editing Lysenko’s pseudo- scientific publications. By the way, those accusing geneticists of being pseudo- scientists (and class enemies) are now universally condemned as charlatans. Here is an intersting quote about genetics from the Soviet Encyclopedia (1950).:"...gene is mythical part of living structure which in reactionary theories like Mendelism-Veysmanism-Morganism determines heredity. Soviet scientists under leadership of Academician Lysenko proved scientifically that genes don't exist in nature."

Appendix (message from William Beatty):
[Correspondence with Scientific American is] VERY interesting. He [the current editor of the journal] seems to base his opinion of LENR only upon what others think; others who very probably have never read the papers. He should be basing his opinion on his own examination of *both* sides of the controversy... and yet he won't go and read the papers himself? Bizarre. Perhaps this a case of "my mind is made up, so don't bother me with evidence found later!" Or perhaps he's waiting for OTHER magazine editors to publish articles about the positive evidence before even questioning his own position. If so, it's bizarre for a journalist to have such an attitude, and triply bizarre for anyone associated with science.

Well, New Scientist had their short article out in March. Heh. Perhaps this just proves that New Scientist staff is incompetent? If so, then that's a classic example of the "experimenter's regress" problem. If we become certain that LENR doesn't exist, then when Schwinger and Storms and McCubre later present positive evidence for it, this is not evidence, instead it is "evidence." In other words, it just proves their own incompetence. Since we know that LENR is bogus, all the papers supporting the phenomenon are seen as powerful evidence of experimenter incompetence. In that case the reliability of the labs reporting a replication mean nothing. Since we know that LENR is bogus, then positive replications are demonstrations that those labs are incompetent. Note that no amount of positive evidence can *ever* be convincing. A pre-existing conviction that LENR is bogus acts as a mental filter. All positive evidence is seen as corrupt, and then is rejected unread. This causes the negative evidence to build up and reinforce the initial judgement, but the positive evidence does not.

But where did such a pre-existing conviction come from? In science it's wrong to first adopt a viewpoint and then to use the selection of evidence in order to support that viewpoint. Politicians do it. The legal system is based on it. Science is totally different: a bend-over-backwards search for the truth rather than a defense of a an existing position during a debate. If one claims to be scientific, yet also adopts a position not based on evidence, then that is pseudoscience. It is a particular form of pseudoscience known widely as "Cargo Cult Science." One essential element is missing: the no-holds-barred search for truth.

This illustrates that it's enormously important for science workers to remain tenative and unprejudiced. If they fail to do this, then they'll start using self-persuasion tactics. To read only the material which supports their position, while at the same time refusing to read material written by opponents, that's a blatent example of self-persuasion in action. Mr. Rennie has apparently fallen into this trap. I could be mistaken though. Has he read the several books which support LENR? Or does he only read material which supports the anti-LENR position? If the latter, then he's involved with Cargo Cult Science.

Why would anyone ever adopt a firm position and start selecting evidence? One reason is ridicule. It's a known phenomenon in sociology. Once a person has ridiculed a particular concept, that person is trapped. They've surrendered their objectivity and their self-image is on the line. A huge conflict of interest arises because, if their ridicule was wrong, it's an embarrassing error of major proportion, and they've opened *themselves* up as a target for ridicule. Once such a conflict of interest exists, only an overwhelming amount of contrary evidence can ever sway them into reexamining their position. The practice of ridicule very often leads to a firmly closed mind, so scientists should avoid such behavior at all costs.

One major mistake he made: saying that he needs no justification for his position. Totally wrong. His position is not proper science; he's not reviewing both sides of the evidence while calmly revising his opinion when new evidence comes in. Instead he takes a stance of *active disbelief*, even hostility, then refuses to examine evidence which might damage this position. Strong levels of belief and disbelief BOTH require detailed justification. The only position which doesn't require justification is the scientific one: refusing to make judgements when evidence is lacking. He certainly hasn't refused to make judgements. Yes, it is up to the LENR researchers to present evidence, it is not up to the scientific community to disprove unsupported claims. But his position is nothing like that of a scientist. Instead he mysteriously "knows" that LENR is bogus, and when LENR researchers present evidence, he bad-mouths it and refuses to inspect it. I wish he had simply cleared up the mystery in his letters to Jed. On what is his judgement based? Perhaps he wants to hide his reasoning because it will fall apart in the light of day. Perhaps it's from the following. If we're to actively disbelieve the LENR papers, on what should we base our disbelief?

1. We all know that only Pons and Fleichman presented positive results, and none of the attempted replications produced any similar effects. (Wrong, as shown by the briefest inspection of the long list of research papers.)

2. The rest of the scientific community knows that LENR doesn't exist. That many smart people can't possibly be wrong. This includes the editors of other major journals. (Bad move. This is like reviewing a book by talking to lots of other people who never read it either. T. Gold at Cornell calls this "the sharpening of opinion in a field, leading to the apperance that the problem is solved." Where is the large group of experts who have actually read any of the positive results? Do they even know that the papers exist? R. Feynman had harsh words to say about this sort of pseudoscience, re. the parable of the Emperor's Nose.)

3. Nobody offered a mechanism whereby fusion can occur at such low energy. (Bad reasoning. Galileo's whispered response shows the great flaw in the above thinking: "and yet it moves." Galileo didn't propose a mechanism. Science doesn't insist on knowing mechanisms before accepting evidence! What if this wasn't the case? This would mean that the scientific community of the time should rightly have turned their backs on Galileo, only to embrace the heliocentric viewpoint after Newton proposed a mechanism fifty years later. No! Observations don't magically become real only after a theory is later developed to explain them.)

4. Pons and Fleichman spent all that money, yet where's the fusion- powered home furnace? (Bad reasoning. Here's an analogy to illustrate. What if the scientific community had ridicule Becquerel's crazy claim that mysterious rays were

emitted by a common mineral called pitchblende? Ridicule was a danger, since he's proposing an instance of a perpetual motion machine. Fortunately his claim was trivial to reproduce. But imagine what might have happened if it wasn't. In that case, should his colleagues reject evidence on the grounds that nobody is unable to build a uranium- powered furnace in 1910 or 1920? Obviously not. We shouldn't reject LENR claims while while requiring that all new phenomena be easy to harness and engineer. And also note that Bequerel committed a high crime: he proposed no airtight theory nor identified a sensible mechanism when reporting his evidence. His evidence triggered a decades-long search for a mechanism. The LENR papers did not. Ask yourself why, then go out and find a solid answer.)

6. Only crackpots come out in support of LENR. (Wrong. Before his death, Julian Schwinger was a supporter on the theory side. SRI and NRL reproduced the phenomenon and recommended that studies continue. So did one of the country's top electrochemists J. O. Bockris. And on the public information side, Sir Arthur C. Clarke has come out as a major supporter. Ah. But because of "experimenter regress", if we decide that LENR is bogus, this just means that these people are all fools. #6 above is circular reasoning: only crackpots support LENR because anyone who supports LENR is defined as being a crackpot.)

7. If LENR is real, then Alchemy really exists! We'd have to rewrite everything we know about chemistry in the everyday world! (Alchemy is blasphemy, and LENR supporters are heretics? In science, ever it was thus: most revolutionary discoveries were blasphemy. Continental drift is the poster child for this. The above isn't an argument against LENR, instead it's a complaint that LENR, if real, will trigger the next major scientific revolution. Yes, we all knew this.)

8. Pons and Fleichman saw neutrons. Then they didn't. And the neutron output was vastly too low to explain the excess heat as having nuclear origin. Then they saw gammas at one energy. When someone showed that the energy was wrong for that sort of reaction, their next paper showed the curves moved to the right place. Doesn't this prove their incompetence? (Maybe, but maybe also it demonstrates the pressures and rushed publication of the times, and demonstrates a trial and error process of discovery which always exists but is usually hidden behind a polished research paper. Only their initial results, the thermal results, stood the test of time, and it was only later that other 'nuclear ash' such as He4 and Tritium were measured and found.)

9. Major labs showed that LENR doesn't exist. That's what led to the ERAB report which condemned the whole affair. (Yes, but this falls into the "experimenter's regress" problem: if we know that LENR is real, then when major labs cannot replicate yet a few individuals succeed, then perhaps it means that the major labs didn't call up Pons/Fleichman and ask about the various techniques needed for success. Yet if LENR *is* bogus, then it means that the major labs did perform the experiment correctly after all, and anyone announcing a successful replication has made a major error. Which position is right? We cannot know, since we would first have to know in advance if LENR is real! In other words, in order to properly judge evidence, in order to know if LENR is real, we have to first know whether LENR is real. That's the experimenters' regress.)

10. The Japanese poured huge amounts of money into LENR research. Then they gave up. If LENR is so real, why didn't a well-funded program demonstrate this? (Good question. It had better not be rhetorical. Why did the Japanese effort fail? I'm not familiar with the issue myself. There could be many answers, so we'd better investigate and not simply take it as proof that LENR is bogus. Perhaps the project was bogged down by politics? Strapped with the sort of active hostile disbelief so prevalent in the USA? Bad luck in using palladium from sources later proved to contain unknown contaminants? Refusal to contact Pons/Fleichman or other successful experimenters to find out the needed techniques? Or perhaps it was a competent demonstration that LENR doesn't exist. Further info is needed.)

11. If LENR is real, where is all the evidence in support of this position? (It's at http://lenr-canr.org Ah, but anyone who finds positive evicence for LENR has simply revealed themselves to be a crackpot? By definition, that website contains papers written by crackpots with scientific credentials? So it would seem, but only if we adopt the initial bias that LENR is bogus.)

OK, so far I see no reason for adopting a stance of active and hostile disbelief. Perhaps Mr. Rennie will wish to submit some info which will cause me to take his position. Jed's article is now in a public place. In my opinion it looks pretty bad that Mr. Rennie can't instantly give clear and convincing reasons why he has made such a firm judgement that

LENR is nothing but a mistake. If he reads this, perhaps he can replace my above list with something sound.

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Voodoo science in Russia today

Ludwik Kowalski (May 26, 2003)
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Kruglyakov's book about pseudo-science in Russia today has been mentioned in one of my earlier items. I read it in Russian, after the book was sent to me by Karabut. Today I found a good English summary on the subject at:

http://www.csicop.org/si/2002-07/dangerous.html

It is a paper published by Edward Kruglyakov in the July/August 2002 issue of Skeptical Inquirer. I think that many people are aware of pseudo-scientists; several illustrations are presented by Robert Park in his recent "Voodoo Science; the Road from Foolishness to Fraud" book. I would not be surprised to learn that pseudo-scientists exists in the cold fusion community. But I have no evidence that major contributors to that field are pseudo-scientists. Here is a quote from Kruglyakov's article:

"In Russia, even research institutes with pseudoscientific tendencies have appeared. I'll give only two examples: the International Institute of Space Anthropecology and the International Institute of Theoretical and Applied Physics. The first has even managed to attain state accreditation with the help of the Russian Ministry of Science. The second has received financial support both from the Ministry of Science and the Ministry of Defense for the well-known swindle of torsion fields. Peaceful coexistence between science and pseudoscience is impossible. From time to time, science attempts to unmask pseudoscience. The latter fights back with fierce hatred. Pseudoscientists are anxious to settle accounts with the Academy of Science, because the Academy is a great obstacle to these newly half-baked 'scientists'."

To what extent is the growth of pseudo-science facilitated by what happens in real science? To what extent is it influenced by what happens in education?

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66) Cybernetics as Voodoo science

Ludwik Kowalski (May 26, 2003)
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An example of excommunication of the entire field of science (genetics) in the Soviet Union was mentioned in item 64. This reminded me of another example; cybernetics was also initially branded pseudo-science in the Soviet Union. To refresh my memory I went to the Internet and found some interesting quotations. According to

http://web.mit.edu/slava/homepage/disint.htm

"No other scientific or engineering discipline underwent such frequent and profound changes of attitude in the Soviet Union as cybernetics. In 1954, the Short Philosophical Dictionary defined cybernetics as a 'reactionary pseudo-science, an ideological weapon of imperialist reaction.' In the late 1950s, cybernetics was portrayed as an innocent victim of political oppression and 'rehabilitated,' along with political prisoners of the Stalinist regime. The Soviet lag in computing was blamed on the earlier rejection of cybernetics. In the 1960s, cybernetics was canonized in a new Party Program and hailed as a 'science of communism'."

And according to:

http://www.pact.sscc.ru/history/early.html

“Norbert Wiener’s book ‘Cybernetics or Control and Communication in the Animal and the Machine’ published in 1948 was actually banned in the USSR, because some ideas expressed by Wiener did not agree to the official Soviet doctrines. In 1953, the leading ideological journal ‘Problems of Philosophy’ published a notorious article ‘In whose service is Cybernetics?’ [3]. The author who hid himself under a pseudonym ‘Materialist’ wrote, in particular: ‘The theory of Cybernetics, trying to extend the principles of modern computing machines to a broad variety of natural and social phenomena without due regard for their qualitative peculiarities, is mechanicism turning into idealism. It is a sterile flower of the tree of knowledge resulting from a one-sided and exaggerated blowing of a particular trait of epistemology’. And further: ‘The imperialists are unable to resolve the contradictions splitting the capitalists’ world. They cannot prevent the approaching inexorable economical crisis. They try to find salvation not only in the frenzied arms drive but in the ideological weapon as well. In the depth of their despair they resort to the help of pseudo-sciences giving them some shadow of expectation to lengthen their survival.’

In the article ‘Cybernetics,’ in the 4th edition (1954) of ‘Concise Dictionary of Philosophy’ [4] this science was defined as a ‘reactionary pseudo-science which appeared in the USA after the World War II and became also wide spread in other capitalist countries; a kind of modern mechanicism’. One can easily imagine what it meant to defend and disseminate a ‘reactionary pseudo-science’ at those times in the Soviet Union!” Nevertheless, some scientists (described on the same web site) were brave enough to defend the field excommunicated by ideologically motivated officials. These scientists are now honored as pioneers of new technology. The two quoted references are:

3. "Voprosy Filisofii", 1953, No.5, pp.210-219.
4. “Kratkiy Filosofskiy Slovar”. Moscow, 1954, pp.236-237.

P.S.
Naturally, Stalin’s persecution of genetics was very different from the withdrawal of support for cold fusion in the US; nobody was imprisoned or killed here for promoting heretical ideas of cold fusion. But the phenomenon of pseudoscience is very real and society should be protected from those who exploit ignorance in order to benefit from

unscientific claims and manipulations. Making money on therapeutic magnets, for example, is a scam; the healing effects of such gadgets have not been validated, as far as I know. The same applies to devices delivering electric energy from a so-called “vacuum.” How can society be protected from con artists without confusing charlatans with honest scientists addressing non-conventional topics?

In my opinion **cold fusion researchers should be as active in exposing pseudoscience as those who do so under the banner of mainstream science.** How actively have they done this? How often do cold fusion researchers criticize each other? I suspect that this does not happen too often. I noticed, for example, that journals publishing cold fusion papers also publish papers devoted to topics of more questionable validity, such as perpetual motion devices, antigravity or hydrinos. Many cold fusion researchers probably disagree with such articles. But how often do they express this openly? I do not know.

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67) Experiments in Texas

Ludwik Kowalski (May 28, 2003)
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What follows is another testimony about cold fusion experiments performed in Texas A&M University by Professor Bockris and his associates. Like Fleischmann, Bockris, (who retired in 1997) was a recognized expert in electrochemistry. He is the author of a 1993 textbook entitled "*Surface Electrochemistry: A Molecular Level Approach*." Was he lying about what has been observed? In my opinion, funds should have been made available to continue investigations of unexplained data. The quote below is an extract from his 2000 paper (*Accountability in Research,* 2000, vol. 8: p. 103). The entire paper, with references, can be downloaded from the library of the www.lent-canr.org web site.

"A quick start was made by my own group (in the Chemistry Department) partly due to my personal knowledge of Martin Fleischmann, who readily told me on the telephone some aspects of the technique he and his collaborators had used. The intense period of work at Texas A&M lasted about one year [ending in 1990]. Work in my own group continued until 1994. The following results from it have been published in refereed journals.

1. Multiple observations of the formation of tritium from deuterium; the tritium production turns on for several hours, then ceases. It can be started up again by means of an increase of the cathodic electrode potential. K. Wolf in the Cyclotron Institute also reported tritium in high concentrations in his own independent experiments. Later, he claimed that this must have been due to tritium present as an impurity in the palladium.

2. We observed significant amounts of excess heat in a few runs. The excess heat was observed in experiments of Appleby and Srinivasan in the TEES laboratories at Texas A&M.

3. In one run we observed heat and tritium together (we had not sought this relation in our other runs). The amount of tritium produced was about 0.1% of that necessary to explain the heat.

4. We found ^{4}He in the Pd lattice after prolonged electrolysis, the difficult analysis being done at North American Aircraft. The helium was about 100 times above background. (Melvin Miles subsequently found ^{4}He in the gas phase equal to around 1/4 that necessary to explain the heat.

5. We carried out about 20 experiments on the detonation of a mixture of solids. We found between 10 and 300 ppm of noble metals, in particular gold, in several experiments. However, the results were not reproducible.

6. In work on damage inside Pd electrodes we found that the impurities deposited on the electrode surface matched those in solution but that new nuclei (of species not present in the solution) developed inside the electrode after it had been saturated with deuterium or hydrogen. We also found Fe produced from spectroscopically pure carbon rods, arced under water of O_2 were present.

All these results have been subsequently verified in many independent labs (the detonation experiments by only two other labs)."

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68) Two meanings of “impossible”

Ludwik Kowalski (May 30, 2003)
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In an essay devoted to the Second Law of thermodynamics, downloadable from http://philsci-archive.pitt.edu/archive/00000313/00/engtot.pdf, one finds a section dealing with the concept of “impossibility.” The author, J. Uffink, wrote: “. . . As we have seen, the basis of the second law is a claim that certain processes are impossible. But there are various senses in which one can understand the term

(i) ‘Possible’ may mean: ‘allowed by some given theory’. That is, the criterium for calling a process possible is whether one can specify a model of the theory in which it occurs. This is the sense which is favored by modern philosophers of science, and it also seems to be the most fruitful way of analyzing this notion. However, thermodynamics has a history of more than 150 years in which it did not always have the insights of modern philosophy of science at hand to guide it. So, one should be prepared to meet other construals of this term in the work of our main protagonists.

(ii) The term ‘possible’ may be taken to mean: ‘available in the actual world’ (or in ‘Nature’). This is the view that Planck and many other nineteenth century physicists adopted. For them, e.g. the statement that it is possible to build a system which exhibits a particular kind of perpetual motion means that we can actually build one. An important aspect of reading ‘possibility’ in this way is that the question of whether a process is possible or not, is not decided by the theory, but by ‘the furniture of the world’, i.e. the kinds of systems and interactions there actually are. This includes the systems and forms of interactions which we have not even discovered and for which we lack an appropriate theory. So, the claim that such a process is impossible, becomes a statement that transcends theoretical boundaries. It is not a claim to be judged by a theory, but a constraint on all physical theories, even those to be developed in the future. Clearly, the idea that the second law is such a claim helps
explaining why it inspired such feelings of awe..... ”

Those who say that cold fusion is impossible because experimental data have not been explained by an accepted theory should wait for the theory to be developed. Skeptics who think that cold fusion is not possible because it has never been observed in nature are probably not familiar with papers describing experimental data collected in the last ten years. I am referring to data on excess heat, and on unusual nuclear phenomena associated with it, as described in papers downloadable from the library at the www.lenr-canr.com web site. Some of them are summarized on my web site, for example, in items 55 and 60. Skeptics familiar with recent experimental data probably suspect that cold fusion claims are fraudulent. What evidence do they have to support accusations of fraud? The issue of poor reproducibility has already been addressed in the item 54 on my list. Here how it was described by B. Bush and J. Lagowski at *the Seventh International Conference on Cold Fusion* in 1998.

“Many laboratories have reported generating excess heat during deuterium oxide electrolysis at palladium cathodes, many of these reports being extremely convincing. The major difficulty with the topic of excess heat generation is not reproducibility, but rather control. The ability to control the circumstances of the electrolysis so that the excess heat can be ‘turned on, and turned up or down’ has eluded us as yet. Generally, generating excess heat remains unpredictable.”

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69) International conspiracy?

Ludwik Kowalski (May 30, 2003)
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The item 68 ended with a quote from two electrochemists from Texas A&M, Bush and Lagowski. Their 1998 paper can be downloaded from the library at www.lenr-canr.org web site. Let me also quote from a paper of M. McKubre, F. Tanzella, P. Tripodi and P. Hagelstein. Their paper can also be downloaded from the above web site; its title is "The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd System: Evidence for ^{4}He and ^{3}He Production." The paper was presented at the 8th International Conference on Cold Fusion (Italy, 2000); the first three authors are from Stanford Research Institute, the last is from MIT.

After describing the apparatus used, and after showing several convincing graphs, the authors concluded: "There exists a strong time correlation between the rates of heat and helium production measured using on-line high-resolution mass spectrometry. In experiments performed using three different metal sealed cells, three different calorimetric methods, by electrochemical and gas loading means and with ^{4}He analyses made at three different institutional laboratories, there is observed a quantitative or near-quantitative correlation between heat and ^{4}He production consistent with the reaction:

d + d - -> ^{4}He + 24 MeV *(lattice)*

Evidence for near-surface retention of ^{4}He in the lattice can be used to accommodate the discrepancy between measured and expected yields of ^{4}He. Evidence was obtained for excess heat production in the electrolysis of D_2O using "double structured" cathodes, supporting original claims by Arata and Zhang (6). Associated with this result, although not quantitatively correlated to it, is the production of a significant amount of ^{3}He within the cathode void volume, suggesting the presence of a second nuclear reaction. The origins of this ^{3}He and its theoretical implications are being explored."

Why are these authors accused of being voodoo scientists? Are they trying to deceive us by presenting fraudulent data? I do not think so. Their papers are not different from those published by mainstream scientists. The instruments they use are highly specialized and not available to typical teachers. That is why a panel of experts should be appointed to evaluate major cold fusion claims.

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70) To petition AAPT?

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Cold fusion was mentioned in a message I received two days ago. The author of the extracted quote (see below) is a physics teacher; he often posts messages on our Internet discussion list, Phys-L.

> We have several list members who are genuinely and probably
> appropriately interested in issues considered by some as "fringe
> science"-- cold fusion comes immediately to my mind. I think we
> can and should entertain some of this inquiry, while recognizing
> that it isn't mainstream and likely is a blind alley. We all have room
> for growth by examining blind alleys. Certainly our students will
> encounter these fields and we need to know something of them
> as physics educators ourselves.

I think that this is a typical attitude toward cold fusion among physics teachers. The “room for growth” aspect is worth emphasizing; one can learn a lot via debates about variuous claims But let us face it, most of us are not equipped with sophisticated instruments, and with the expertise needed to use them properly. We know that only experimental data can show whether or not cold fusion claims are valid but we cannot collect such data in our laboratories. That is why a second official evaluation of cold fusion claims is needed at this time. To help students we must be helped by the scientific establishment.

The report from the first evaluation, initiated by the Department of Energy, and conducted by a panel of appointed experts, was released in November 1989, only nine months after the discovery of cold fusion was announced. It was based on what was known at that time. What should teachers think about new experimental data, such as accumulation of helium in cold fusion cells, or highly unusual isotopic ratios among heavier byproducts of cold fusion reactions? Several groups of researchers (from Japan, Russia, Italy and US) published articles about these phenomena. Are the authors of these publications incompetent? Are experimental data published by them fraudulent? Why are they often compared to voodoo scientists? Unable to answer such questions by ourselves we teachers expect to be guided by experts.

Many of us are familiar with convincing arguments that cold fusion could not possibly be dominated by the same nuclear reactions those occuring in hot plasma fusion. That conclusion, as far as I know, has been accepted by all scientists, including those from the cold fusion community. After criticizing the circumstances under which the discovery of excess heat was prematurely announced, and after emphasizing the need for controllable and reproducible data, the old report ended with the following: “However, there remain unresolved issues which may have interesting implications. [We are], therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system. “ I find the last sentence very significant; it indicates a desire to examine the outcome of better experiments, if they become available in the future. This was fourteen years ago; new experimental data are now available and they should be evaluated. (I already wrote about this in item 21 at html://blake.montclair.edu/~kowalskil/cf)

Would it be appropriate to formulate a petition asking the American Association of Physics Teachers (to which most of us belong) to initiate a second evaluation of cold fusion? Should I draft a petition and start circulating it on Phys-L? I have no experience with something like this. My note to The Physics Teacher, asking for a reevaluation of cold fusion,

was rejected. My letter to the editor of Physics Today, asking for the same thing, was ignored. Will my letter to the editor of The Physics Teacher, submitted several weeks ago, also be ignored? I do not know what else I can do. Who would suffer from another formal evaluation of cold fusion claims? Why is the scientific establishment unresponsive?

Here is what I wrote in a recent Phys-L thread: "We must accept data coming from real reproducible experiments, no matter how many theories they disagree with. But so-called 'gedanken experiments' are very different in that respect; we accept their conclusions only when these conclusions do not conflict with accepted theories." Cold fusion experiments are real, their results should not be rejected on the basis of desagreements with existing theories. Confirming that ^{4}He is produced in a hydrogen cell, for example, would be highly significant, even if the reproducibility were as low as 10%. The same would be true for highly unnatural isotopic ratios. If it were up to me I would focus on these two claims; they were made by several groups of recognized scientists and, if confirmed, would open new avenues of officially supported research. Lack of 100% reproducibility is always an indication that some important parameters are not yet under control and that additional work is needed.

To see what Schwinger (who shared Nobel Prize with Feynman) wrote on the subject of irreproducibility of cold fusion experiments see item 33 on my list. Very good descriptions of the current status of cold fusion, by three top scientists, can be found in item 24. If what you are reading now is printed on paper then you might not know that my cold fusion items are available over the Internet. The address is:

http://csam.montclair.edu/~kowalski/cf

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71) Josephson about voodoo science

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(June 1,2003)

R. Park's "Voodoo Science" book has been referred to several times. A review of that book by Brian Josephson, a Nobel Laureate and a professor of physics at the University of Cambridge, can be seen at:

http://www.tcm.phy.cam.ac.uk/~bdj10/articles/park.html

Josephson wrote: "We find in Park's book the official story regarding a number of "mistaken beliefs". What one will not find -- and is hard to find anywhere if one does not know where to look to bypass censorship -- is the additional information that might lead one to conclude that the official view does not tell the whole story. Regarding the paranormal, Park follows others in quoting a lecture on "pathological science", given by noted chemist Irving Langmuir, concerned with claimed phenomena that are difficult to reproduce. In a number of cases this was because the observed effects were clearly shown to be caused by a flaw and went away when a properly designed experiment was done. But Langmuir then went on to make the dangerous generalisation that if any effect is weak or difficult to reproduce then the effect is not a real one. This does not logically follow; an effect may be weak or difficult to reproduce simply because it is weak or difficult to reproduce. It is not easy, for example, to detect neutrinos from the Sun, and different laboratories tend to get different results in this research. . . .

It is interesting to look both at Park's account of the history of cold fusion and at that of the protagonists, presented in a video documentary *Cold Fusion: fire from water* (available from www.infinite-energy.com). Park impresses on the reader the fact that if the process that generates the heat is really fusion then one would expect to see fusion products. He fails to mention here, as the video does, that the small amount of such products anticipated, given the amount of energy generated, was eventually observed, and in just the right quantity. All mention of positive results, such as the experiment where, by what appears to be a sound method, it was found that the energy generated was considerably in excess of anything that could be explained conventionally, is collapsed into a paragraph where Park notes that many claims are soon withdrawn because of errors being found (as also happens in ordinary science).

This device legitimises the dismissal of all positive results, and so also the corollary 'cold fusion is no closer to being proven than it was the day when it was announced'. This is a seriously misleading statement. There *are* scientific arguments against cold fusion, but equally there were arguments against continental drift. ..... Despite its faults, *Voodoo Science* is an interesting book, with many stories about the kinds of mistakes made by people who believe they have made an important discovery. But it should carry a disclaimer that is the converse of the one with which Park ends his 'What's New' column on the American Institute of Physics website: 'the opinions in this book are unquestioningly shared by many scientists, but they should not be'."

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72) Anecdotal Evidence ?

Ludwik Kowalski (June 27, 2003)
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The fragments below were extracted from the Introduction to Tadahiko Muzino's book "Nuclear Transmutation: The Reality of Cold Fusion." The book, published in 2000, can be ordered from Amazon.com. The introduction was written by Jed Rothwell; it can be downloaded as "MizunoTnucleartra.pdf" file from the library at html://www.lenr-canr.org

* * *

. . . Mizuno wrote this short book about his work and personal experiences. It is the best informal account yet written about the daily life of a cold fusion researcher. It gives you a sense of what the job feels like. It is not intended to be technical. For technical details, the reader is invited to examine Mizuno's numerous scientific papers, some of which are listed in the references. One event described here which is not described in the technical literature is an extraordinary 10-day long heat-after-death incident that occurred in 1991. News of this appeared in the popular press, but a formal description was never published in a scientific paper.

Mizuno says this is because he does not have carefully established calorimetric data to prove the event occurred, but I think he does not need it. The cell went out of control. Mizuno cooled it over 10 days by placing it in a large bucket of water. During this period, more than 37 liters of water evaporated from the bucket, which means the cell produced more than 84 megajoules of energy during this period alone, and 114 megajoules during the entire experiment. The only active material in the cell was 100 grams of palladium. It produced 27 times more energy than an equivalent mass of the best chemical fuel, gasoline, can produce. I think the 36 liters of evaporated water constitute better scientific evidence than the most carefully calibrated high precision instrument could produce. This is first-principle proof of heat. A bucket left by itself for 10 days in a university laboratory will not lose any measurable level of water to evaporation. . . .

It is a terrible shame that Mizuno did not call in a dozen other scientists to see and feel the hot cell. I would have set up a 24-hour vigil with graduate students and video cameras to observe the cell and measure the evaporated water carefully. This is one of history's heartbreaking lost opportunities. News of this event, properly documented and attested to by many people, might have convinced thousands of scientists worldwide that cold fusion is real. This might have been one of the most effective scientific demonstrations in history. Unfortunately, it occurred during an extended national holiday, and Mizuno decided to disconnect the cell from the recording equipment and hide it in his laboratory. He placed it behind a steel sheet because he was afraid it might explode. He told me he was not anxious to have the cell certified by many other people because he thought that he would soon replicate the effect in another experiment. Alas, in the seven years since, neither he nor any other scientist has ever seen such dramatic, inarguable proof of massive excess energy.
Here is a chronology of the heat-after-death event:

March 1991. A new experiment with the closed cell begins.
April 1991. Cell shows small but significant excess heat.
April 22, 1991. Electrolysis stopped.
April 25. Mizuno and Akimoto note that temperature is elevated. It has produced 1.2 H 107 joules since April 22, in heat-after-death. F. Nakano, "Mohaya hitei dekinai jyouon kakuyuugou [The reality of cold fusion can no longer be

denied],” Bungei Shunju, September 1991
The cell is removed from the underground lab and transferred to Mizuno’s lab. Cell temperature is >100 deg C.
April 26. Cell temperature has not declined. Cell transferred to a 15-liter bucket, where it is partially submerged in water.
April 27. Most of the water in the bucket, ~10 liters, has evaporated.
The cell is transferred to a larger, 20 liter bucket. It is fully submerged in 15 liters of water.
April 30. Most of the water has evaporated; ~10 liters.
More water is added to the bucket, bringing the total to 15 liters again.
May 1. 5 liters of water are added to the bucket.
May 2. 5 more liters are added to the bucket.
May 7. The cell is finally cool. 7.5 liters of water remain in the bucket.
Total evaporation equals:
April 27 10 liters evaporated. Water level set at 15 liters in a new bucket.
April 30 10 liters evaporated. Water replenished to 15 liters
May 1 5 liters replenished.
May 2 5 liters replenished
May 7 7.5 liters remaining.

Thus, evaporation since April 30 is: 15+5+5-7.5=17.5 liters. Total evaporation is 37.5 liters. The heat of vaporization of water is 540 calories per gram (2,268 joules per gram), so vaporization alone accounts for 85 megajoules. One aspect of the heat-after-death event seems particularly strange. It is as if the cathode is trying to maintain stasis inside the cell. After the external 60 watt heater was turned off, the heat-after-death reaction increased just about enough to compensate for the loss of external heat. This sounds like an instrument error. It prompted Mizuno to double check all instrument readings with meters attached directly to the sensors. As unbelievable as this sounds, it is a real phenomenon which others have observed. Stanley Pons noted that the cold fusion effect has a kind of “memory.” After a perturbation, temperature tends to return to a fixed level. Perhaps this is not so strange. The physical configuration of deuterons in the metal controls the power level.

. . . Mizuno has often talked about the prehistory of cold fusion. Most great discoveries are visited and revisited many times before someone stakes a permanent claim. People sometimes stumble over a new discovery without even realizing what they see. Mizuno did his graduate and post graduate work on corrosion using highly loaded metal hydrides. His experiments were almost exactly like those of cold fusion, but they were performed for a different purpose. In retrospect, he realized that he saw anomalous events that may have been cold fusion. At the time he could not determine the cause, he did not imagine it might be fusion, and he had to leave the mystery unsolved. No scientist has time to track down every anomaly.

. . . Recently, Mizuno, Bockris and others have increasingly focused on so-called “host metal transmutations,” that is, nuclear reactions of the cathode metal itself. The cathode metal was inexplicably neglected for many years. The term “host metal” is misleading. It was an unfortunate choice of words. It implies that the metal acts as a passive structure, holding the hydrogen in place, cramming the deuterons or protons together. The metal is a host, not a participant. The hydrogen does the work. Now, it appears the metal itself is as active as the hydrogen. The metal apparently fissions and fusions in complex reactions. Now the task is to think about the metal, and not just the hydrogen. Theory must explain how palladium can turn part of itself into copper and other elements with peculiar isotopes. One of the few “Eureka!” events in this book is the moment when Mizuno and Ohmori saw the scanning electron microscope images of the beautiful lily-shaped eruptions on the surface of Ohmori’s gold cathodes. This was visual proof that a violent reaction takes place under the surface of the metal, vaporizing the metal and spewing it out. Later, these vaporized spots were found to be the locus of transmutation. Around them are gathered elements with an isotopic distribution that does not exist in nature. The only likely explanation is that these isotopes are the product of a nuclear transmutation.

* * *

What might skeptics say about recent cold fusion data?

1) Muzino and others in Japan are liars, like Karabut and others in Russia, like Bressani and others in Italy, like Lonchampt and others in France, like Bockris and others in the US. The data are fraudulent.

2) These people only pretend to be scientists. Their Ph.D.diplomas were counterfeit; their professorships at famous universities were bought; the books and hundreds of articles they published were produced by somebody else. They are members of an international "mutual support society."

3) We already know everything about nuclear phenomena; facts which disagree with existing theories are not acceptable. Absence of commensurate amounts of neutrons and protons is a sufficient reason to ignore claims about unusual nuclear processes.

4) Cold fusion researchers were often wrong in 1989. Therefore what they are finding now must also be wrong. They should never be forgiven for announcing a discovery via a press release, or for claiming that excess heat experiments are very simple.

5) Claims made under the banner of cold fusion were not described in articles published in leading journals. Therefore they cannot be accepted. The editors of these journals refuse to publish cold fusion articles; they know better what is right and what is wrong.

6) Neither the Department of Energy nor the National Science Fundation support research in the area of cold fusion. Therefore such research is not worth taking seriously. Those who perform experiments cannot be objective about their own research.

7) Practical applications of cold fusion have not been demonstrated; therefore the underlying phenomena cannot be real.

8) We know nothing about recent cold fusion findings; therefore they must not be correct. The entire field was declared pseudoscientific in 1989 by a panel of experts. The opinion of experts must be respected; it can not be challenged by new findings.

9) Experiments should be 100% reproducible before they can be accepted.

I agree with the last reservation. Lack of reproducibility is a clear indication that some important parameters are still not under control by experimentallists, and that conclusions are tentative. I elaborated on this in item # 54. Electrostatic experiments used to be called "irreproducible" before the effect of humidity was recognized. I do not think that it is appropriate to identify "irreproducibility" with "not being real;" Irreproducibility is typical in all areas of emerging science. I see nothing unusual when a competent cold fusion scientist is successful only eight or nine times out of ten to demonstrate a new phenomenon. As far as i know, this is typical in only some types of cold fusion experiments. The situation is no longer as bad as it was in 1989.

That is why I think that the time is right for a new investigation of the entire cold fusion field by a panel of experts. They should focus on experiments which are nearly always
reproducible, not on observations which were reported only once or twice. If you, a reader of this item, do not accept my suggestion, then please explain your position on the Phys-L list. Perhaps your message will influence our thinking. Or write to me at : <kowalskil@mail.montclair.edu>, if you prefer. Thanks in advance.

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73) Reproducible Excess Heat

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What a coincidence. Yesterday I posted an item about Mizuno's work in Japan. Today I found a very recent French report confirming Mizuno's results. I have no idea who the author (Jean-Louis Naudin from JLN-Labs) is. I will assume that he is a student because in one place he writes: "I am very grateful to Professor Pierre Clauzon for his contribution to the CFR project and his help during some CFR tests runs and also to Professor Jean-François Fauvarque and Gérard Lallevé from the " Laboratoire d'Electrochimie Industrielle" of the CNAM (Conservatoire National des Arts et Métiers) from Paris, for their scientific support and their contribution to this project." At his website at:

http://jlnlabs.online.fr/cfr/html/cfrdatas.htm

Jean-Louis summarizes results of 33 runs of excess heat experiments completed only four days ago (on June 25, 2003). He writes: "Today, I can say that the Mizuno-Ohomori's Cold Fusion Reactor is fully replaceable and that it works very well as described in their papers. You will find below the latest measurements results that I have performed since May 7th, 2003"

I was not able to follow details but conclusions are very impressive. In run 28, for example, when 170 grams of water were evaporated, the electric energy was supplied to the setup at the rate of 1200W while heat was generated at the rate of 3093 W. The blue line in the last figure, showing what would happen if there were no excess heat, is significantly different from the red line representing the results of real calorimetric measurements. See http://jlnlabs.imars.com/cfr/html/cfrpd.htm for additional details.

In my opinion the results are reasonably reproducible, as far as reported data from 33 runs are concerned. But will they be reproduced by other researchers? The blank run seems to indicate the absence of a systematic error. This reminded me Karabut's data (item 13 on my list). Naturally, skeptical accusations raised about Mizuno data (see item 72 on my list) can also be raised about Naudin's data.

P.S.
After posting the above I sent an email message to Jean-Louis. The reply was: "Thanks for putting a link to my Cold Fusion replication experiment... For your information, I am not a student as you have supposed (I am 45 years old...), I work in my own and private lab and some French physicists work closely with me in this field of research..."

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74) Reproducible Excess Heat, part 2.

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Here is how the experiment of Mizuno, replicated by Naudin (see item #73 on this list), was described by Mallowe. Dr. Eugene F. Mallove is the Editor-in-Chief of the "Infinite Energy Magazine Cold Fusion Technology." He is also the author of "Fire from Ice," a book about cold fusion published by John Wiley & Sons, Inc. (in 1991). I am surprised that hydrinos are mentioned at the end Mallowe's description of topics to be investigated. Linking hydrinos with cold fusion is not desirable, I think. My item #57 is about hydrinos. I would like to hear arguments showing that excess heat can not possibly result from chemical reaction taking place in experiments quoted by Mallowe. I like situations in which different teams work on similar experiments. The experiments which I was invited to participate in Salt Lake City was similar but it was focused on transmutations rather than on excess heat. No evidence of transmutations was found.

****
A word of WARNING right up front: This is potentially VERY DANGEROUS work and must not be done without maximum, safety precautions and supervision by those thoroughly versed in laboratory safety! It involves high-temperatures, high-voltages, explosive mi xtures of oxygen and hydrogen, caustic solutions, and steam generation that if improperly contained could cause a deadly explosion. These are just the beginning hazards. This is unknown territory. Drs. Ohmori and Mizuno measured the transmutation of elem ents in this experiment. Thus, until further notice, and despite your possible skepticism about their claim of transmutation, this work must be considered, by definition, nuclear experimentation. With that said, we encourage every thoughtful group who can do this experiment safely to attempt it. Infinite Energy and Cold Fusion Technology, Inc. take no responsibility for the known or unknown radiation or other hazards that are associated this experiment.
****

Dear Colleagues:

After much work we now have what appears to be a marvelous do-it-yourself "cold fusion" experiment. Any reasonably equipped chem/physics lab can see the effect within days to a week -- if they get their act together. The experiment has the following qualities:

1. It is visually and audibly spectacular -- brilliant glowing, pink, purple, lavender with white flashes on an underwater tungsten (W) electrode (e.g. 2 mm x 5 mm W foil or 1 cm x 1.6 mm diameter tungsten welding rod). A plasma-like underwater discharge on the electrode that often manages to disintegrate or melt tungsten underwater with only about 50 to 80 watts of power over a short period. (Tungsten's melting point is 3680 K or thereabouts.) The sound of the underwater "explosions" on the cathode -- brilliant white flashes on the purple background plasma -- is very impressive.

2. It is *totally reproducible* -- at will -- with no loading time as in the Pd/heavy water experiments

3. Calorimetry is simple to do because there is so much steam energy evolved from the reaction that by simply tallying the amount of water vaporized as steam, one gets over-unity every time (so far). Three groups have already gotten *preliminary* over-unity results: 1. Ohmori and Mizuno in Japan, who introduced the phenomenon at ICCF-7 (O/U estimated at 2.6/1); 2. Gene Mallove and FAA engineer Ed Wall here in Bow, NH during the past ten days -- work continuing -- (CONSERVATIVELY 1.4/1); 3. Engineer Mark Hugo of Northern States Power in Minnesota (but his affiliation has nothing to do with his home experiment) -- (CONSERVATIVELY 1.5/1), but owrk still continuing to check for errors. Chemist John Thompson in the Bahamas, who attended ICCF-7, was the first person outside of Japan to reproduce the effect and will do calorimetry on it soon. This will be in IE #20 out on July 22.

4. No one has patents or may be able to get them since the effect was noticed in other forms (1916!) and reported extensively in the Journal of the Electrochemical Society, April, 1950, p.133 in an article titled "The Anode Effect in Aqueous Electrolysis," by Herbert Kellogg of the School of Mines, Columbia University. New York -- Hal Fox found this marvelous article, since it relates to some of the work his Trenergy Company is doing weith charge clustgers and radioactivity reduction. This may, indeed, be related to the underwater electrolysis ability to remediate nuclear waste. But -- OF COURSE! -- NO *calorimetry* was done in 1950. No one had any idea that such simple systems could be O/U. The systems were studied for other reasons. Further: the present tungsten effect is seen mainly on the cathode, but it can make the Pt anode incandescent too at lower water temperature (say 50 C) -- we normally work over 80 C. It is very mysterious -- was so to Kellogg in 1950 and remains so.

5. Ohmori and Mizuno found major evidence for transmutation of elements and volcanic ejection of metals from the tungsten surface -- these SEM photos were reproduced in their article. They find Hg, Os, Kr, Zn, Cu, Ni, Fe, Cr, Si, and Mg -- with anomalous isotopic content. Just as I said, in IE #15/16, this subject is more properly called ©¯Electro-Alchemy"

6. WARNING: Ohomori and Mizuno experienced significant apparent electromagnetic effects on their instruments. They were unsure whether some of the effect on their neutron counter were evidence of neutrons -- I doubt the latter, as did Srinivasan of BARC at ICCF7. I have kept a Geiger counter on during our experiments -- absolutely no sign of major ionizing radiation, but of course it could be localized within the cell.

Ohmori and Mizuno's paper at ICCF7: " Strong Excess Energy Evolution, New Element Production, and Electromagnetic Wave And/Or Neutron Emission in the Light Water Electrolysis with a Tungsten Cathode." T. Ohmori and T. Mizuno, Catalysis Research Center, H okkaido University

Here is my recommended recipe for an experiment to demonstrate the effect:

1. Take a 250 ml glass beaker, fill to about 200 ml level with 0.5 molar (0.5 M) K_2CO_3 -- potassium carbonate solution

2. Get 0.5 mm diameter Pt wire for both anode and cathode leads -- about 15 cm for each lead is adequate length. Shield them with teflon tubing down to the connection point with the Pt or W.

3. Use a small piece of Pt foil -- about 2 mm x 5mm on the anode (positive lead) crimped mechanically to the Pt wire -- no welding is needed.

4. Use a 2 mm x 5 mm size W foil on the cathode - negative side. It is tricky to attach the W foil (we used 0.1 mm thickness, which can be pierced with difficulty and the Pt lead wired through). Or, if W welding rod 1/16-inch diameter is available, wrap the Pt wire around the W piece about 0.5 to 1 cm long. Warning: The reaction is so violent that it is hard to get the cathode piece in rod form not to fall out of its Pt wire cage! Runs up to 10 minutes or so are usually OK. Mark Hugo has run for up to 75 minutes, condunsing steam from the reaction -- but he has put other ingredients into the brew such as Li, and he has used a thicker cathode of 1/8-inch W rod.

5. Get a DC power supply up to 5 amps capacity and up to 200 volts. (I am eager to try beyond 180 volts, but we may need a concrete bunker before we try that! Eager also to try heavy water!) You can use a variac AC source to make DC power-- use a bridge rectifier and capacitor to make DC from the AC output. Install voltage and current metering devices -- digital display preferred, data acquisition system if you want to get fancy on the calorimetry later. I'm sure Scott Little and Mitch Swartz could do this soon if they put their equipment into this specialized service. This is an excellent experiment for Barry Merriman too!

6. Heat up the beaker solution to 80 C either by electrolysis at low DC input power -- e.g. 20 V, 1.8 to 2 amps -- or with an external hot plate. (Turn off the hot plate and **remove it** if calorimetry is being used.) At about 80 deg -- sometimes at a lower temperature -- crank up the volts to 120 to 180. The effect changes appearance as voltage increases. Current will drop substantially to 0.2 to 0.4 amps as the sheath of steam surrounds the glowiing plasma-

sheathed cathode. Keep an alcohol thermometer suspended in the solution to measure the temperature. I would *initially* avoid thermocouples because of the threat of violent electromagnetic interference in this unknown phenomena.

In calorimetric assessment: Heating credit should be taken for the full mass of water in the cell from the initial temperature of say 80 C to the boiling point. However, you may find it difficult to push the average solution temperature up to 100C (we were only in the low to mid 90s), because the steam ejected cools the solution so rapidly. The boiling point is elevated -- McKubre estimated to me in aprivate communciation by only 0.25 deg C for such a solution). This is wonderful, because we WANT steam. It will be no problem at all to power steam engines with this, if pending thorough verification of excess energy (This *is* work in progress that must be confirmed!) we figure out how to get the power ratio high enough, if it is not there already. The main source of the excess is the amount boiled off: water vaporized requires about 2260 J/gm.

Why was this missed by other cold fusion people? Simple: everyone was looking for a sedate reaction that P&F had started with when the real pay off was in these higher voltage systems that trigger with metals like tungsten. W is recommended due to its temperature resistance, but John Thompson has found that other metals such as Al, Cu, Ni, and Zr also work -- as far as the *visible* effect. The colors of the emissions are different -- different hot plasma near the cathode surface.

Other parameters that need to be explored:

* Higher voltage

* Can energy be extracted from the recycled water after steam condensation or if Mills-type hydrinos are formed, do they become "inert"?

* Other electrolytes -- KCl also works, according to Thompson, try higher molarity values

* Other metals

* Pressurized systems -- BE CAREFUL!!!

* Recombine oxygen and hydrogen -- but a very small part of the effect, undoubtedly -- to get extra energy in the output

* Try heavy water in various mixtures with light water

* Detect electromagnetic pulse from the device -- if it is there as they found in Japan

* Examine the element production and non-natural isotope ratios formed

* Scanning Electron Microscopy imaging for morphology of craters

* Look for radiation -- use film fogging techniques, CR-39 plastic detectors, etc.

This will keep a lot of people busy for a long time. As soon as we pin down the thermal characteristics a bit more, we and others well be hell-bent to scale up to larger power-producing units -- with SAFETY FIRST as our motto.

****

This is a typical, very brief O/U investigation run that Ed Wall and I performed here recently in an uninsulated glass beaker on a metal surface -- VERY rough calculation, conservative, we think. Among other factors reducing effect apart from ZERO insulation is the recondensation and re-boiling of material -- we had a plastic cover on the beaker with holes drilled in it. A distillation recondensation device would be preferred to observe water evolved as steam.

Input: 0.7 to 0.8 amps, average 0.75 A input at 168 volts => 26,590 J

Duration: 3 minutes, 31 seconds

Solution reduced from 183 ml to 173 ml due to boil-off

Output = 10 x 2260 J/gm H_2O + 183 x (about 15 C rise during heating to full boiling at about 92 C) x 4.18 J/gm C = 34,060

output/input = 1.28

Credit for uninsulated vessel and re-bailing of condnsed liquid could easily push this to 1.40 and beyond. Much more work needs to be done.

I hope that other Vortexians will try this -- CAREFULLY, please!

****

Ohmori and Mizuno did calorimatry on the last few mintues BEFORE boiling and compared the rate of temperature rise to that of a joule heater's effect on the same volume of electrolyte. That's how they got their 2.6/1. See Infinite Energy #20 for more information in late July.

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75) Do not mix science with fiction

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One thing that often discredits the cold fusion field is the premature rush toward speculations about its technological applications. Those who promote such speculations are supplying skeptics with arguments used against cold fusion. I know that some technological inventions were made long before the underlying science was understood. But in the area of cold fusion, at this stage, the emphasis should be on basic science, not on science fiction and on unverified commercial claims. I saw many very good articles in "Infinite Energy Magazine" of Eugene Mallowe, and in "Journal of New Energy" of Hal Fox, but some articles appear to be junk. The cold fusion field is controversial enough; what is the purpose of mixing its topics with even more controversial topics, such as Edison engine, Mills' engine, hydrinos, or antigravity?

To illustrate premature technological speculations let me show extracts from the "Cold Fusion Goes Commercial" editorial. The entire piece (from the Infinite Energy Magazine, vol. 1, No 2. 1995) can be seen at the following Internet web site:

http://www.planetarymysteries.com/energy/ie.html

"As it stands now, 'cold fusion' and allied 'new energy' technologies, in which energy appears to materialize perhaps literally from the vacuum quantum fluctuations of space or from subtle mass conversion to energy - are possibly the greatest commercial opportunities of this century..... Now for an incredible commercial bombshell -a water-fuel device that did not originate from the mainline cold fusion field at all, but which is very definitely penetrating the marketplace already in eastern Europe. We have learned that in a former region of the Soviet Union, now the country of Moldova (known also as Moldavia), the VIZOR Corporation in Kishinev has already sold thousands of heating units to homes and industries!..... But what is most important is that hundreds upon hundreds of satisfied customers have ratified the technology in the marketplace!..... "

Here is how this device was described, after being tested by a US research team from Texas. "A water-heating device developed in Kishinev, Moldavia by Dr. Yu. S. Potapov has been reported to produce a heat output up to 3 times greater than the energy required to drive it. A Russian physicist, Lev G. Sapogin, has offered a theory to explain this phenomena in his paper entitled, 'On One of Energy Generation Mechanism in Unitary Quantum Theory'. We obtained a Potapov device and conducted a series of energy balance measurements on it. No evidence of over-unity performance was observed." The entire test report is available at the following Internet web site:

http://www.earthtech.org/experiments/yusmar/potapov.txt

A TV show can also be used to promote science fiction. Here is an illustration; it is one part of a transcript of the ABC's program "Good Morning America," shown on June 11, 1997.

"**MG** (voice over): During the last year, Patterson's little beads have led to a huge surprise. Not only do they produce heat. It turns out, they also neutralize radioactivity.

JP: (Patterson) This is the cell system down here . . .

MG It sounds like such an amazing development, the company is attracting big name scientists, like Norm Olsen. He

traveled all the way from Hanford, Washington, where the government stores billions of gallons of high level radioactive waste.

NO (Olsen): If this technology works out as advertised, it means we could significantly reduce the radioactivity of nuclear waste in the United States, and the world.

MG: But does it work as advertised? We decided to put it to the test.

JP: What I have in this cup is radioactive uranium in a water medium.

MG: And that's what's sending that Geiger going crazy, right?

JP: Yes.

MG: So the idea is that the radioactive material will then flow through your device, and actually remove the radioactivity?

JP: Yes.

MG: I don't believe it. Go ahead and push the button. Let's see if it works. (Laughs.)

MG: The experiment began at high noon, with the Geiger counter registering well over 300. But by speeding up the video, you can see that after a couple of hours, the radioactivity was cut down by more than half -- a reduction that would take billions of years to happen naturally.

NO: I am really encouraged by what I have just seen. Our plan now would be to take it back to Hanford Labs, test it out under controlled circumstances, and fully prove it. And go as far as we can go with it. ....."

If I were in Norm Olsen's position I would ask some questions. Is it not possible that the distribution of radioactivity withing the cell changes during the electrolysis thus reducing the count rate? Is it possible that the reduction of radioactivity is due to the fact that some of it is escaping from the cell? I would not take the cell to Hanford Labs before answering such questions for the existing setup. As far as I know, even now, six years later, no accepted evidence exists that radiactivity can be reduces by a process taking place in an electrolytic cell.

For more about this see; http://www.altenergy.org/3/new_energy/cold_fusion/workers/workers.html

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76) Secrecy in cold fusion research

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Secrecy in science and technology is a big issue; I am not trying to address it in general. But that issue was brought to my attention several times while I was reading cold fusion materials, especially the writing of two scientists: George Miley (see item 36) and Benjamin Bush. In a section entitled "Scientific Integrity and Openness." Miley wrote: "Integrity in science has been a 'hot issue' throughout the saga of CF. This issue has sometimes been confused with the problem of 'openness' of discussion. [Integrity] has been seriously hampered by the dominance of various companies and entrepreneurs in CF who hope to gain advantage through patents and proprietary information. Integrity and openness are, in fact, entirely different issues. Integrity must be maintained at all costs; openness is highly desirable, but is not always possible. In order to succeed, companies must often protect their base intellectual property. . ."

He also wrote: " 'Stopping wasteful funding on CF' [became an official policy in the US]. Thus, small companies and individuals end up, by default being the main funding source for CF. As a result, they in turn, rightly feel that their investment is entitled to protection via patents and secrecy. Normally, in other fields, government funded research provides a balance of funding that leads to a flow of open publications covering the basic science underlying the field, while the practical technology funded by companies remains proprietary. In CF, 'open' science only comes from a very few academics or others who can undertake research without 'strings' attached. A majority of the work is more guarded."

B. Bush, interviewed about his research (Infinite Energy Magazine, January-February, 1997, page 32) was not willing to provide a clear answer because , "at this juncture, [the answer] does involve proprietary aspects." Then he elaborated by saying that "there is no question that 'secrecy' tends to hurt the general development and dissemination of science. On the other hand, funding is necessary for equipment, lab supplies, release time from teaching, etc. In an atmosphere where the government is providing no funding, as in the case of CF research, private funding becomes extremely important. Private investors want a return on their money, and patents containing proprietary materials are essential. Thus, in the present atmosphere of no government funding,'proprietary aspect' becomes a 'necessary evil' relative to the development of the Science." At one time I contacted a cold fusion researcher and asked him to write an item about cold fusion patents for this list. He declined on the basis of the "proprietary aspect" of the issue. His pending patents could possibly be threatened, he wrote, by a description of what is going on in the US Patent Bureau. I was surprised.

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77) More evidence of nuclear reactions

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Motivation for this item was provided by the 2000 paper of G. Miley et al. entitled "On the Reaction Products and Heat Correlation for LENRs." The rest of this paragraph is the abstract of their paper. " 'Low Energy Nuclear Reactions', or LENRs, typically involve electrolytes containing light water along with electrodes made of metals such as Ni, Ti and Pd. In these experiments a variety of reaction products (isotopes), with masses both higher and lower than that of the host electrode material, have been observed at the University of Illinois (U of IL). Related results, often termed "transmutation" studies, have been reported by other researchers. These observations suggest that proton-metal initiated reactions occur in such LENR cells. This paper discusses evidence that the production of these reaction products is correlated with the excess heat also frequently observed in LENR cells. Such a correlation for LENR reactions would be equivalent, in principle, to the correlation of He-4 with excess heat that is reported for heavy water-Pd experiments where a D-D reaction is postulated."

The entire paper, a presentation at the 8th International Cold Fusion Conference, can be downloaded from the < http://www.lenr-canr.org > web site; the name of the file is <MilesMradiationm.pdf>. George Miley is a prominent American experimental scientist in the area of cold fusion. (He also wrote on social aspects of scientific research, as illustrated in item #36 at my cold fusion web site.) The paper begins by reminding us about experiments in which accumulation of ^{4}He, presumably resulting from D+D encouters, was observed in experiments with palladium cathodes in heavy water cells. Then the paper displays a figure listing reaction products identified in Miley's cathodes. I was impressed by the number of displayed products, and by the fact that their atomic masses cover a wide range (from lless than 10 to more than 210). The average reaction rate, in terms of [atoms/(cc*sec)], is specified for each product. The authors focus on high yield products (production rates exceeding 10^{13}) because their presence can not possibly be attributed to contamination. Most of the products are not radioactive. Isotopic ratios are often significantly different from those found in nature.

Such products have been reported by many research teams and their presence is a strong indication that something highly unusual is going on. This has already been discussed in earlier publications. Miley and his team go beyond this point; they focus interpretations. The hypothesis is that nuclear reactions are somehow induced when protons (nuclei of hydrogen) interact with the cathode material. Knowing the amount of excess heat generated, and the number of atoms produced, authors are able to calculate the average energy released. The result, 0.15 MeV per reaction, is by many orders of magnitude higher than energies released in most exothermic chemical reactions. But it is about 25 smaller than in common hot fusion reactions. Note that the reported reaction rates were calculated by dividing measured numbers of atoms by the time of operation. The numbers would be much larger if atoms where produced in short bursts (rather than continousely).

I am not a chemist and I take it for granted that the excess heat measured can not possibly be due to chemical reactions. I know, however, that only a nuclear reaction can transform one chemical element into another. The authors report that "a striking pattern [in the mass distribution] consistently observed in these measurements is that the high-yield reaction products occur in four mass ranges, roughly A = 20-30, 50-80, 110-130, and 190-210 [4]. Statistically significant shifts in isotope ratios from natural abundance are also observed for many of the products [2, 3]. Numerous precautions were taken to guard against impurities in these measurements (see references 2, 3). This includes use of special "clean" systems, blank runs, and precision diagnostics prior to and following runs.

Other evidence of the nuclear basis for these results included the observation of low energy X-ray and/or beta emission from electrodes after a run and statistically significant shifts in isotope ratios for key elements. The general trends from these results are reasonably consistent with reaction product measurements by other workers (see the discussion and references in references 4, 5). However, others have often termed their work “transmutation studies” as opposed to “reaction product studies”. For that reason, other studies have not generally focused on the total product yields or on correlations with heat.” Note that abnormal isotopic compositions could not result for contamination.

Is Professor Miley incompetent? Is he a liar? Why is his report, and similar reports of other researchers, are not taken seriously by leaders of our scientific establishment? Why are all cold fusion investigators accused of practicing pseudo science? How does Miley’s cold fusion report differ from reports presented by the so-called “true scientists?” The situation is not good. I tried to do something about this but Physics Today, rejected my “letter to the editor.” My hope was that the letter would trigger a discussion about the way to restaure normality. Why should cold fusion be treated differently than other areas of science?

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78) An older fight for acceptance

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It is well known that neither pure water nor table salt are good electrical conductors. But the resistance of a solution of salt in water is relatively low. How can it be? This was a dilemma puzzling 19th century scientists. It was brilliantly solved by Svante Arrhenius, a Swedish scientist (1859-1927). According to C.G. Beaudette, the grand daughter of this scientist was a researcher at Utah University when cold fusion discovery was announced in 1989. After the press conference she spoke with Fleischmann and Pons. She said “Well, when my grandfather proposed electrolytic dissociation, he was dismissed from the University. At least that won’t happen to you.” She was mistaken, both scientists were dismissed.

The history of Arrhenius’s model of molecular dissociation is being presented by Beaudette as an example of difficulties encountered by those who promote new ideas. Let me quote from the Chapter 11 of his interesting book: “Excess Heat: Why Cold Fusion Research Prevailed,” Oak Grow Press, LLC, South Bristol, Maine, USA, 2000. I strongly recommend this book for a school library. Arrhenius’s “story bridged the turn of the twentieth century. His breach with the orthodox scientific community of twenty years duration, his intellectual survival in a small group with his scientific fellows, and the argument whether his discovery was one of chemistry or physics, all parallel the instance of the Fleischmann and Pons controversy.

Arrhenius finally achieved collegiality between himself and the scientific world by persistently teaching his discoveries. His story is almost prophetic of what happened in "cold fusion" research during its first ten years. He was banished from both his university and the Swedish scientific establishment. He overcame that affliction and went on to great acclaim as a revered elder statesman within the European scientific community. He accomplished much of this by attaching himself to a foreign scientist of recognized integrity.

Arrhenius's principal scientific discovery asserted that when a salt is dissolved in water, each molecule of the salt separates into electrically charged electrolyte particles that had been named ions fifty years earlier. It was ions that interacted chemically with other ions in solution rather than with atoms or molecules. He asserted that these ions carried an electrical charge as they moved about and were the mechanism by which the solution conducted electrical current.

There was open and often bitter controversy during the two decades after he announced the discovery. The distinguished German chemist Wilhelm Ostwald stood at his side as a foreign savior and mentor during Arrhenius's most difficult times. Ostwald attested to the correctness of Arrhenius's thesis and he started a new technical publication as a vehicle for spreading and defending the ionic theory. They traveled the world of science arguing Arrhenius' theory. The two of them made a major presentation and defense of his theories in London in 1890.

Arrhenius's redemption at home began when the University of Stockholm's predecessor institution offered him a position as professor. The offer was made under somewhat humiliating conditions, which he bore with grace. He was quickly accepted by his colleagues and in two years was elected to the presidency of the school. In 1903, he became the first Swede to be awarded the Nobel Prize and it was in chemistry. He enjoyed the considerable recognition of other scientists continuously until his death in 1927.

It is necessary to look back further to understand the nature of Arrhenius' fall from scientific grace. Scientists had struggled to understand electrical conduction in liquids for the previous one hundred years. Svante August Arrhenius grew up near the world-famous State University of Upsala, Upsala, Sweden. As a bright youth, he turned his attention

early to experimenting with electrical conduction through salt solutions. When he entered the University, Professor Robert Thalen, the professor for physics, did not take him seriously and refused him the use of the physics laboratory. He did his experiments in the laboratory of Erik Edlund, Academy physicist.

Arrhenius made the fateful decision to continue with his interest in conductive solutions when he enrolled to study for his doctorate degree. He records the exact moment when he came to his principal discovery about conductivity in solutions. It was on May 17, 1883, that he entered a period of feverish work to write it down. He claimed that the salt molecules in solution divided into electrically polarized particles called ions. He confirmed for himself that he understood what was happening in salt solutions. His full statement of discovery claimed that these ions became the reactive elements for chemical behavior in solution and were also the agents for electrical conduction.

The relationship between theory and experiment was not well understood at this time in the development of modern science. Arrhenius had conceived a theory of ion formation and action. Professor Per Teodor Cleve, his doctoral advisor, was a distinguished scientist and the discoverer of the two metals: holmium (holmio was the Latin name for Stockholm) and thulium. Cleve considered theory to be something like Henry Ford's history, that is "bunk." Arrhenius's thesis dissertation was closely fought, and the outcome was only a partial victory for him. He received his doctorate degree of the fourth class, the lowest of four possible grades, and designated non sine laude approhatur, approved not without praise. He could not pursue an academic career at the university.

He responded to the setback by sending copies of his thesis to several prominent scientists outside of Sweden. One went to Wilhelm Ostwald, professor of chemistry at the Polytechnical School at Riga, in the Russian provence of Livonia, later Latvia. Ostwald found his thesis compelling and decided to help him. He traveled by train to Stockholm to meet and befriend Arrhenius. The result of Ostwald's mediation was the offer of a teaching position for Arrhenius in the Stockholm Technical High School (the Hogskola, later part of Stockholm University). The world of science was focused at that time entirely in Europe and North America. Ostwald's continued patronage of Arrhenius and his thesis gradually brought the world of science to terms with the existence of ions in solution.

Arrhenius gained the respect and fellowship of his colleagues in the Hogskola, a result of his increasing international renown. During this time, Arrhenius married Sofia Rudbeck, one of his pupils. The marriage lasted only two years and produced a son, Olav Vilhelm. Later he became deeply involved in the establishment of the Nobel Prize awards after the death of Alfred Bernhard Nobel in 1896 and in the adjudication of his will." His principal contribution to the establishment of the Nobel Academy was his insistence that the nomination and award selection procedures be international in their vision. The Academy elected him a member of both the chemistry and physics award selection committees. J. H. van't Hoff, a colleague in Holland, received the prize in chemistry in 1901, the first year of the chemistry awards. Arrhenius received it in 1903 after a thoroughly contentious candidacY His discovery of ionic disassociation in solution raised the significant question whether it was a physical or chemical discovery.

The physics award committee included Professor Thalen, from Upsala. It had no members from the Hogskola other than Arrhenius. The Upsala University members looked down on the Hogskola as an inferior institution. Thalen, who strongly influenced the physics committee's position concerning Arrhenius's candidacy, successfully persuaded the committee that Arrhenius's discovery was more properly assigned to chemistry thus absolving the physics committee of his candidacY Cleve, who had once refused any interest in the young Arrhenius's theories, now advanced Arrhenius's cause effectively in the chemistry award committee and was successful in achieving for him the chemistry award.

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79) Calorimetry of beta decay in 1930's

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The book of C.B. Beaudette ("Excess Heat: Why Cold Fusion Research Prevailed," Oak Grow Press, LLC, South Bristol, Maine, USA, 2000) has already been used to quote an interesting story (see item 78). Another story from that book refers to the dilemma of beta decay energy which confronted physicists in 1930's. Calorimetry played an important role in that dilemma, as in cold fusion. Another similarity was in the apparent violation of the energy conservation law. The atomic masses of elements involved in beta decay were known and scientists calculated the amount of energy released in each transformation. The energy obtained from calorimetric experiments, however, turned out to be about three time smaller than expected.

According to Beaudette. "While those involved in the measurements thought this conflict meant that the missing energy got transported away by a particle presently unkown, most of physics refused the hypothesis and simply waited. They did this on a supposition that there might yet be an error in the heat measurements. In about twenty years, instruments were invented that were able to detect the neutrino. In proved to be the new particle that carried away the 'missing' energy. The calorimetric measurements and their corresponding hypothesis of a new particle were vindicated. This pattern of disbelief may be what is happening in cold fusion research where the measurements will be held in abeyance untill the nuclear answers are in."

Analogies, of course, have limited validity. The beta decay dilemma resulted from difficulties associated with detection of theoretically anticipated neutrinos, the cold fusion dilemma, on the other hand, resulted from the absence of an accepted theory. Calorimetric measurements of beta decay energy were always reproducible but calorimetric measurments of cold fusion are not always reproducible. This, according to Beaudette, shows "that the determining variables were under control" in beta decay experiments but not in the initial cold fusion experiments. Irreproducibility (see item 54 on my list) is not necessarily an indication that an experimental procedure is faulty. Fleischmann and Pons were the most qualified scientists in the world to perform electrochemical experiments for testing the hypothesis of excess heat.

Describing Fleischmann Beaudette wrote: "When he arrived [from Imperial College to Durham University], most electrochemistry was done by measuringthe current and voltage applied to the electrolytic cell. Fleischmann brought to the laboratory much more in the way of instrumentation. This improvement was widely recognized, and led to a rejuvenation of the field..... In 1967, Hills [chairman of chemistry department at Southampton University] invited Fleischmann to accept the position of Faraday Professor of Electrochemistry. His charge, when he arrived at Southampton, was to build a world class electrochemistry group. . . .

From 1970 to 1972 Fleischmann was president of the International Society of Electrochemists. In 1985.....he bacame a Fellow of the Royal Society of London, the most prestigeous honor for a scientist that England had to offer." What is the basis for accusing him to be a voodoo scientist? He came to the University of Utah to work with his former student, Pons. According to Beaudette "they published eight major papers on their measurements of anomalous power in scientific journals between July 1990 and 1995, and these have easily withstood the published criticism. ..... At Southampton, Fleischmann had gothered together components for an experiment in the early 1970s wherin hydrogen gas would be loaded into palladium metal in extreme amount. He had chosen the deuterium form of hydrogen for this experiment to see if nuclear fusion might be triggered." That was the hypothesis he came to test in Utah.

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80) Secular Theology?

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As I wrote in item 3, criticism of CF was mostly focused on a speculative hypothesis (nuclear origin of excess heat) and not on the essential experimental fact (existence of excess heat) and on the methodology used by scientists who discovered it. In the Preliminary CF Note (1989) Pons and Fleischmann wrote: "It is inconceivable that this [excess heat] could be due to anything but a nuclear process. . . It is evident that [chemical] reactions are only a small part of the overall reaction scheme and that other nuclear processes must by involved. .....The bulk of the energy release is due to an hitherto unknown nuclear process." CF was declared to be pathological science (also in 1989) because reality of suspected nuclear processes has not yet been demonstrated. This is not very different from a situation in which critics rejected Galileo's findings because they conflicted with the existing view. The theory of circular orbiting was formulated (by I. Newton) long after Galileo's death.

According to (1), "to discard a well made observation is to violate modern protocol [accepted scientific methodology]. If widely practiced, such a course would quickly undo science. The most interesting and perplexing observations, though accurately measured, would have to be refused by the scientific community because their cause was obscure. Does this mean that any claim of observation must be accepted as worthy of scientific study? Certainly not. It means something quite different. It means that the controversy must center about the quality of the measurements and not about source or cause of the phenomenon. Karl Popper provides an escape from the difficulty of accepting into science an observation that went contrary to established theory. He asserts that science advances, not by proving theories correct or by defending them to the ends of the Earth, rather, by accepting (not adopting) experimental outcomes that contend with theory. His example was that the observation of a thousand white swans does not prove that all swans are white, but the observation of a single black swan undoes forever a theory that says all swans are white. ..... If conflicting data is prohibited from contention, then theories are no longer falsifible. Were it to enable such practice, **science would evolve into secular theology."**

Beaudette's book has many interesting observations, and quotes, pertinent to this important subject. That is why I recommend it to all those who are interested in history of science, and in philosophical aspects of its methodology. Another thing that impressed me was Chapter 14 describing work of those scientists who confirmed the initial excess heat findings of Fleischmann and Pons. The ERAB report, resulting in the blacklisting of cold fusion, was published only seven months after the discovery of excess heat was announced. This was unfortunate; many projects described in Beaudette's book, were far from being completed, or even conceived. What was the reason for not waiting another year or two before finishing the investigation? Was it a fear that producing a devastating report could become more difficult later because high caliber scientists started to be involved? Was it a rush to discredit the cold fusion field as soon as possible? Was it an attempt to protect science from charlatans? Was the ERAB panel trying to protect society from spending too much money on fraudulent projects? Such questions naturally come to mind in reading Chapter 14, especially about background and credentials of involved scientists.

Reference:

1) Charles G. Beaudette, "Excess Heat: Why Cold Fusion Research Prevailed," Oak Grow Press, LLC, South Bristol, USA, 2000.

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81) Where are Theories ?

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This is another expiration based on Beaudette's book (1). In Chapter 16 he speculates about possible mechanisms of cold fusion. First he quote P. Hegelstein who wrote, in 1998, "that major outstanding theoretical problem is not how to elude the Coulomb barrier; and it is not how to account for anomalously low neutron emission rate. The big issue is, and always has been, whether a large energy quantum can be transferred efficiently from a nuclear reaction to new degrees of freedom associated with the surrounding environment."

Then he muses about a mechanism involving coherent phonons. Such phonons (quanta of thermal oscillation energy) excite atomic nuclei to energy levels at which reactions become possible. The process of excitation (accumulation of energy) is slow, presumably lasting several minutes or so. Beaudette writes: "The theorists suggested that the prolonged period when the nucleus is in an under-excited state makes the formation of stable reaction products possible." Personally. I can not take such speculations seriously; they conflict with what I know about nuclear processes. Only experimental findings of qualified scientists can be taken seriously, even when they conflict with accepted theoretical models. That is the essence of our scientific methodology, as emphasized in unit #80.

Later in the chapter Beaudette wrote;" It appears, at the time of writing, that the excess heat reaction is one that joins together two deutron atoms to make a single helium-four atom. In this process an energy of 23.8 MeV is released and transferred to the metal lattice as heat. What sort of 'joining' this reaction might be, is not known yet. It cannot be fusion as known by the high energy physicists, because that reaction generates lethal radiation and fast neutron particles that are not present in the excess heat data. It can not be the reaction of muon-induced (catalyzed) fission, because that reaction generates strong radiation not present in excess heat data. Several theorists suggest that the process might be the result of a multibody reaction, one that brings together more than two particles into the reaction process."

It is important to keep in mind that Beaudette is not a theoretical physicist; he is a retired electrical engineer interested in cold fusion. Earlier in the book he described a cold fusion experiment in which he participated as an observer. That experiment, at Stanford Research Institute, convinced him that generation of excess heat is real. He met with many cold fusion researchers at one of their international conferences. In my opinion his overall description of the cold fusion field is excellent and his book deserves a place in libraries, especially at high schools and universities. The level is appropriate for a person whose background is limited to an introductory physical science course.

In the Chapter 17 Beaudette summarizes the most interesting and puzzling data about nuclear transmutations taking place in cold fusion experiments. Once again he returns to the issue "secular theology." He quotes what John Huizenga wrote (2) about Fleischmann's mentioning of "fission of palladium." Fleischmann was, no doubt, unfamiliar with the fact I had researched nuclear fission for decades and had coauthored a book . . . on the subject. Knowing that the energy threshold for palladium fission was several tens of millions of electron volts, I had to conclude that Fleischmann was either joking . . . or exposing gaping holes in his knowledge of nuclear physics."

Let me mention that I personally know Huizenga and that the book he coauthored with R. Vandenbosch was a major reference in my own research. Huizenga was also the chairman of ERAB which produced the negative report about cold fusion in 1989. This report, well documented in (2), will remain an important historical event, no matter what the final verdict about cold fusion. Commenting on the above, Beaudette wrote: "That is the kind of comment that is skeptical rather than critical. Huizenga was not sufficiently intrigued to ask to see the data that had led Fleischmann to his supposition." Would my reaction to Fleischmann's claim be the same as that of Huizenga? It probably would. But I

agree with Beaudette that rejecting a claim (made by an experimentalist) because it did not agree with theory was not appropriate. Experimental findings conflicting with accepted models of reality should be scrutinized more carefully that other findings but conflicts with theories are not sufficient causes for rejections.

I do not know how Huizenga would have reacted to Muzino's findings if he had known about them. Would he have reacted as scientist, which I know he was, or would he have reacted as a politician? Should I contact him and ask what he thinks about new findings today? I do not think this would be appropriate; the last time we talked was more than twenty years ago. In the field of nuclear chemistry he was my teacher, not a personal friend.

Reference:

1) Charles G. Beaudette, "Excess Heat: Why Cold Fusion Research Prevailed," Oak Grow Press, LLC, South Bristol, USA, 2000.
2) John R. Huizenga, " Cold Fusion: Scientific Fiasco of the Century," 2nd. edition. New York, Oxford University Press, 1993.

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82) Speculations of a retired physicist

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[P.S. On August 20 his paper became item #103 of this list]

A retired physicist (RP) sent me an essay in which he reflects on excess heat from thermonuclear reactions. He speculates that "cold fusion" takes place in tiny spots at which a crystal material (loaded with hydrogen) might be compressed to very high temperatures (millions K) by electromagnetic shock waves. In a letter accompanying the essay RP wrote: "Enclosed is a synthesis of my studies on problems associated with development of practical nuclear fusion. It contains elements of both 'hot' and 'cold' research. I hope that it can bring together these two feuding camps that jointly provide the research to bring, fusion energy reactors to reality. It is too important a goal for us to waste time and energy on a Hatfield-McCoy feud among physicists."

RP does not believe that cold fusion is basically different from hot fusion. He speculates that "data from cold fusion experimental reports indicate that nuclear fusion of atoms trapped in microstructures may be due to the imposition of external forces that generate compression waves. These waves compress lattice cells and heat the trapped deuterium atoms to fusion energy levels." In solid environments conditions are such that the energy released in thermonuclear fusion goes into random lattice oscillations rather than into radiation. Excess heat cannot be generated unless the Lawson criterion is satisfied. The essay elaborates on conditions under which that criterion can be satisfied in nanoparticles.

According to RP, who takes findings of cold fusion researchers seriously, it will be possible, at least in principle, to build a fusion reactor with the following properties:

a) It does not emit gamma rays and nucleons during its operation.
b) It does not produce radioactive residuals.
c) It is completely controllable and safe.
d) It is simple to build and operate.
e) It has high net power to weight ratio.
f) It can be scaled over a wide range in size and power.

A research program for achieving the goal is outlined; it refers to nanoparticles, to stabilization of electric arc with electromagnetic waves, to sonoluminescence, to plasma-acoustic shock waves and to other topics with which I am not familiar. To motivate inventors RP tells them that fusion reactors might exist on the sun surface. Referring to solar prominances he writes: "Solar loops rise from the surface of the sun [flares] to altitudes of 100,000 km. It would be expected that the temperature of the loop would decrease with altitude. Some loops have been observed to have their temperatures rise from 4500 K at the base to several million K at an altitude of about 10,000 km. This phenomenon has not been satisfactory explained." RP speculates that very high temperatures observed might be due to thermonuclear reactions. Nanoparticles, such as Carbon 60 (first discovered in solar spectrum), can possibly play an essential role in generation of excess heat. They are also essential in the conceptual description of his anticipated terrestrial reactor.

****

After writing the above I emailed it to RP. I also asked about the date at which his paper will be posted on his web site. He said that will try to do this before August 15. The URL of the web site, and the real name of RP will be inserted into this item later. His reply also contained comments shown below; I hope they will be useful. These comments are added with his permission. The RP wrote: " Your synopsis does not express central points of my paper as I would have presented them.

1. Hot fusion and cold fusion are closely related.

2. Neither approach, as now conceived, will produce a practical nuclear power device.

3. Knowledge gained from amalgamation of these two approaches suggests a new reactor concept that satisfies the "practicality" requirements.

a. no gamma or neutron emission
b. no radioactive byproducts
c. completely controllable and safe
d. simple to build and operate using current commercial components
e. high net power to weight ratio
f. scalable in size and power

4. Explanations are proffered for natural phenomena and experimental observations currently described as "anomalous"
.
5. Nanoparticles (uni-element molecules composed of 10 to 100s of atoms) that trap hydrogen are essential to this design. These products are produced commercially.

6. A schematic drawing of a conceptual design for a nuclear fusion device is presented and possible developmental experiments are described.

7. It would be practical for stationary, mobile, airborne and space vehicles.

8. This concept is particularly suitable for inclusion in scientific and technical courses.

9. The fundamental stabilized arc device (without nanoparticles) was built and operated by J. Yenser of Grumman Aircraft in the late 1950s.

Note: I think that putting items in the synopsis out of context may deter some people who are not well versed in the subject from reading the paper.

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83) Disassociate cold fusion from unscientific claims

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On July 21, 2003 I posted the follwing Phys-L message about the cold fusion conferennce in Boston.

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Today, 7/21/03, is the last day to register for the ICCF10 (The 10th International Conference on Cold Fusion at MIT, August 24 to 29) at a reduced price. Go to the http://www.lenr-canr.org site, if you are interested. The list of submitted papers can also be seen there. Fleischmann himself is making two presentations. I am going because I want to meet him and others who contributed to an important scientific controversy. I also want to see cold fusion demonstrations; unfortunately, the list of demos has not yet been posted.

Contrary to what the leaders of our scientific establishment claim I no longer think that qualified cold fusion researchers are practicing voodoo science. Nor have I definitely concluded that chemically induced nuclear proceses are real. That is why I am going to the conference. Perhaps I will have something new to share after my return. But in September my priorities will shift back to teaching.

The phenomenon of pseudoscience is very real and society should be protected from those who exploit ignorance in order to benefit from unscientific claims and manipulations. Making money on therapeutic magnets, for example, is a scam; the healing effects of such gadgets have not been validated, as far as I know. The same applies to devices delivering electric energy from a so-called “vacuum.” How can society be protected from con artists without confusing charlatans with honest scientists addressing non-conventional topics? I hope this difficult issue will be addressed at the conference.

In my opinion cold fusion researchers should be as active in exposing pseudoscience as those who do so under the banner of mainstream science. How actively have they done this? How often do cold fusion researchers criticize each other? I suspect that this does not happen too often. I noticed, for example, that journals publishing cold fusion papers also publish papers devoted to topics of more questionable validity, such as perpetual motion devices, antigravity or hydrinos. Many cold fusion researchers probably disagree with such articles. But how often do they express this openly?

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The above message was also sent to two conference organizers. One of them responded in length and gave me permission to share what he wrote. The reply (in blue), is shown below; my comments (in red) are shown at the end.

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1) You asked: “What do you think about this issue?”

I do not think much of it.

2) You wrote: “In my opinion cold fusion researchers should be as active in exposing pseudoscience as those who do so

under the banner of mainstream science."

Why? They are not policemen. They are busy. Frankly, I don't see much point to spending a lot of time trying to judge what is pseudoscience and what isn't. People doing pseudoscience cause little harm, especially compared to people doing some types of conventional science such as plasma fusion.

3) You asked: "How actively have they done this?"

Not at all, as far as I know. I would advise them not to associate themselves with pseudoscience in either a positive or negative way. In fact, I do not think anyone should try to judge research he has not carefully investigated, for months or years. Cold fusion scientists are no more qualified to investigate something like "magnetic healing" than anyone else is. Only a medical researcher who has made a careful, honest, serious study of this topic has any business passing judgement on it. If no medical researchers have taken up this topic and performed experiments, then we have no way of knowing whether it is pseudoscience or real science. Experiments are the only standard of truth. You can never tell, a priori, whether a claim is true or false. Theory may predict a likely outcome, but only an experiment can produce an outcome (if you are lucky).

4) You asked; "How often do cold fusion researchers criticize each other?"

Always. Vociferously.

5) You wrote: "I suspect that this does not happen too often."

You couldn't be more wrong. I do not think you have any basis for this suspicion.

6) You wrote: "I noticed, for example, that journals publishing cold fusion papers also publish papers devoted to topics of more questionable validity, such as perpetual motion devices, antigravity or hydrinos."

Which journals? Infinite Energy perhaps, but not the Japanese Journal of Applied Physics. I do not think you can generalize. And how do you know whether hydrinos are pseudoscience or not? Unless and until someone tries to replicate the hydrino experimental results no one can judge that issue.

7) You wrote: "Many cold fusion researchers probably disagree with such articles. But how often do they express this openly?

Very often.

8) You wrote "The phenomenon of pseudoscience is very real and society should be protected from those who exploit ignorance in order to benefit from unscientific claims and manipulations. Making money on therapeutic magnets, for example, is a scam; the healing effects of such gadgets have not been validated, as far as I know."

You may be incorrect about that. There have been some surprising positive results from valid studies. See The New York Times SCIENCE, December 9, 1997, "Study on Using Magnets to Test Pain Surprises Skeptics. A small trial raises hope, but it is not the last word." by L.K. Altman, M.D.

Quote: "No one was more skeptical about using magnets for pain relief than Dr. Carlos Vallbona, former chairman of the department of community medicine at Baylor College of Medicine in Houston. So Dr. Vallbona was amazed when a study he did found that small, low intensity magnets worked, at least for patients experiencing symptoms that can develop years after polio. . ."

However, it is extremely unlikely these results will be replicated or confirmed because there is such rabid opposition to unorthodox ideas these days, so we will probably never know.

* * * * *

I agree with some of the above. In 5, for example, the word “guess” would be more appropriate than “suspect.” And in 6 I should have said “marginal journals.” I suppose that all cold fusion papers in JJAP were published because they passed the rigor of the strict “peer review” scrutiny. I was happy to read that cold fusion people often criticize each other work. That is a sign of health in science. The article you atached made me more receptive to the claims of Dr. Vallbona who said that magnets did reduce pain of many postpolio patients. I was not familiar with statistical studies conducted to verify the effect. To illustrate my point I should have chosen a less controversial example.

And what about item 2? My guess, based on some reading, is that con artists and charlatans do exist in every society. They often try to exploit simple people by asking them to buy things or to invest in projects. In that sense pseudoscience can be harmful. If I were involved in cold fusion research, and if my article were published in the same journal as an article about “hydrinos,” “quantum heeling” or “antigravity,” I would try to disassociate myself from these claims publicly. I would do this to protect my own image, not to be a policement.

Item 3 refers to validation of scientific claims. That is a big issue. To validate some textbook claims teachers often perform demonstrations. In addition students may be asked to validate things via laboratory work. But most claims are accepted on the basis of authoruty of textbooks and articles published in scientific journals. (In practice students accept what teachers say and teachers accept what scientists say.) Nobody has means and qualifications to verify everything by the way of experimenting. Existence of quarks, for example, can not verified in a typical university lab. That is why most of us wait untill experiments become 100% reproducible, as declared by recognized experts, before accepting new findings. You will see this issue addressed at my ICCF10 presentation.

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84) From Physics Teachers on Phys-L

Ludwik Kowalski (July 30, 2003)
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Let me share two URLs, and messages they generated in the “Hidden side of science” thread. Authors are physics professors at three universities, two from the US and one from Australia. All three belong to Phys-L, an Internet discussion list for science teachers. The list has about 700 subscribers, mostly physics teachers from 35 countries. I think that what Teacher 2 wrote about cold fusion is typical among educated observers.

Teacher #1 (JULY 24, 2003):

A Phys-L teacher, XXX, who occasionally comments on my cold fusion items (in private) shared interesting Internet resources about the often-ignored aspect of science.

1) The abstract from the first essay, by Brian Martin, is shown below.
The entire piece is at:

http://www.scientificexploration.org/jse/articles/martin/1.html

“**ABSTRACT:** Those who challenge conventional view or vested interests in science are likely to encounter difficulties. A scientific dissenter should first realize that science is a system of power as well as of knowledge, in which interest groups play a key role and insiders have an extra advantage. Dissenters are likely to be ignored or dismissed. If dissenters gain some recognition or outside support, they may be attacked. In the face of such obstacles, several strategies are available, which include mimicking science, aiming at lower status outlets, enlisting patrons, seeking a different audience, exposing suppression of dissent, and building a social movement.”

2) The second essay, by Daniel Drasin is entitled
"Zen and the Art of Debunkery." It can be fetched from:

http://members.aol.com/ddrasin/zen.html

Both are worth reading and thinking about. That is why I am sharing these URLs with the list. Any comments?

Teacher #2

I would say that anyone who is advising someone proposing an unpopular theory to scientists to "mimic science" is not coming at the problem from a scientific perspective.

And to argue that science has an "entrenched power structure," is only looking at half the situation, and then from a very jaundiced perspective. Science is conservative, as well it should be. New, revolutionary ideas need to be well established before they take over. If scientists ran after every chimera that came down the pike, they would seldom get anything done. But when something looks promising, even if it isn't likely, then lots of people will get active in "checking it out." That happened in the case of cold fusion, and what happened then is entirely characteristic of what happens in these cases. It happened in the case of the fifth force flap as well. That is, many labs try to reproduce the experiment, or design their own to test the idea, and at first many get corroborating results, but a few don't. Gradually the experiments are more and more carefully done and as that happens the number of corroborating results starts to decrease until only a few holdouts remain.

In the case of high T superconductivity, just the opposite happened, as time passed more and more of the verifying experiments were successful, as people learned the techniques better, and before long, the phenomenon entered mainstream science.

On the other hand, cold fusion has shown several of the characteristics of Langmuir's "pathological science." In addition to decreasing numbers of corroborating experiments, and a lack of alternative experiments (neutron flux measurements, etc.), there were lots of ad hoc explanations from the originators as to why this or that other experiment didn't work. The originators became more and more secretive and withdrawn. They refused to make their raw data available for examination, or to conduct their experiments in the presence of others, and everyone who got different results "weren't doing it right." It seems there was only one right way to do the experiment--change the slightest thing and it won't work, and they were not very forthcoming on the ways to fix the experiments.

It is seldom the case that some phenomenon can be detected by only one type of experiment and the circumstances have to be "just right" for it to work. It's not unheard of, but when it happens it puts and extra burden on the discoverers to be as open as possible in helping others to make their experiments work.

It is also crucially important for the discoverers to make every effort to find ways they could be wrong. Feynman's dictum applied here--first, don't fool yourself. If you aren't fooling yourself, you will not likely fool others. I don't think Pons and Fleischmann carried out this part of the "new science" regimen very well.

Of course, the "established" scientists are going to be reluctant to give up their pet theories, but, for the most part, not "unto the death." When something new is shown to work, scientists accept it quickly. Only a small rear guard might hang back or resist to the end. Something as revolutionary as cold fusion, with its incredible potential, is not going to be suppressed for long, if it is real. There is too much at stake. I doubt the folks in the high-energy fusion game will be too happy to see it come, but they are a relatively small part of the physics community, and if it worked, others would have flocked to the fold and we would be getting energy into our homes from that source by now, and the fusion research program at Princeton would have been shut down by now.

The argument that "establishment science" will do its damnedest to suppress any new ideas in order to protect its own "turf" is just nonsense put forth by the voodoo practitioners who are on the outside looking in. They are outside because they refuse to practice science as it should be done, instead insisting that they are a "little guy" being kept from success by "big science" and their monied backers.

Given the economic potential of this phenomenon if it exists, the "monied backers" would have flocked to the cause of cold fusion if there was any hope that it was real. In fact, I understand the Sony did provide some funding for Pons & Fleischmann at their lab in France for a few years, until they finally tired of ever seeing a return on their investment.

Obviously, many things that are presently thought to be not possible will be found to be possible in the future. I am not an expert on cold fusion and I cannot say with total confidence that it will never be proven to exist, but if it does, at some point someone will come up with the repeatable experiment that P&F apparently didn't have, will demonstrate it so others can repeat and redesign the setups and it will be shown to be real, and soon thereafter will enter mainstream science and within a short time after that will become a viable part of the world power grid.

But it's been, what, fifteen years since P&F burst on the scene? In that time they have gone from front page news to a scientific backwater. If they, or others have something, let them show it, with papers in the archival journals of record, and not backwater publications that might be more interested in publicity than scientific integrity.

That's my $0.02 worth. I have better things to do with my time than to read the ranting of an outsider, who, if your quoting of him is characteristic, has little if any understanding or appreciation of how science works. Its shades of Joe Newman all over again.

Teatcher #3

Teacher 1 quoted:
>> In the face of such obstacles, several strategies are available,
>> which include mimicking science, aiming at lower status outlets,
>> enlisting patrons, seeking a different audience, exposing
>> suppression of dissent, and building a social movement.

Teacher 2 wrote:
> I would say that anyone who is advising someone proposing an
> unpopular theory to scientists to "mimic science" is not coming at
> the problem from a scientific perspective.

It is a pity that before writing your scathing reply you didn't take some time out to peruse Martin's paper (it probably would have taken less time than formulating your reply). Let's look at what Martin is getting at when he advises "mimicking or thodox science": Since mainstream scientists expect contributions to be in a certain standard format, then writing articles in this format may increase chances of success. Since submissions from institutional addresses are usually treated more seriously than those from home addresses, it may be useful to set up an institute even if it contains only one person! Alternatively, it might be possible to obtain an honorary position at an established institution, such as a university. There are a few open-minded departments that may be willing to provide a haven for dissenters." Whether or not that is "a scientific perspective", it's pretty good advice.

You continued:
> And to argue that science has an "entrenched power structure," is
> only looking at half the situation, and then from a very jaundiced
> perspective. Science is conservative, as well it should be.

You write "entrenched power structure"; Martin wrote "science is a system of power" which is not quite the same thing. Let's look at a little of what Martin wrote here. "Some types of interests are corporate, government, bureaucratic, professional, career, and psychological. In each case they can exert strong pressures on the direction of research and shape the response to challengers. Note that interests influence science without the necessity of conscious bias, since interests shape people 89s world views."

Certainly true, almost a motherhood statement. There's much more of interest as he develops this theme, with plenty of references given, but I won't overload this e-mail. For most of rest of your reply you go on about cold fusion: the points you make are certainly of interest but they are not relevant to your dismissal of Martin's paper, since he nowhere mentions cold fusion in that paper. So I'll snip a lot of your reply and preserve your final paragraph, which really got up my nose.

<snip>

> That's my $0.02 worth. I have better things to do with my time than
> to read the ranting of an outsider, who, if your quoting of him is
> characteristic, has little if any understanding or appreciation of
> how science works. Its shades of Joe Newman all over again.

Now, Hugh, Martin is a physicist; he is not an outsider. His paper is not a rant; it presents a well-argued and well-sourced argument. One may or may not agree with what he writes but he deserves to be read and not dismissed so cavalierly and with such scorn. I admit to a personal interest in this matter. Brain Martin did some teaching for me almost 30 years ago when he was a post-graduate student. That teaching was unconventional and successful. Since than he had spent many years understanding and appreciating how science works. Would that more of us had done the same.

Message #2

I plead guilty to not looking at the article. But the phrase "mimicking science" so turned me off that I couldn't resist the cheap shot. My perhaps misdirected arrows at Martin's article aside, I still think that my assessment of the cold fusion

scenario is accurate. That had nothing to do with Martin's article. I see CF as classic pathological science, as described by Langmuir (reprinted in Physics Today, October 1939, p. 36). I cannot say for certain that CF isn't real. I suspect nobody can. But I would guess, based on the number of the characteristics of pathological science it has exhibited, the odds against it are pretty long, and that was the main thrust of my rant. The quotation from Martin's article that Ludwik posted seemed cut directly from that mold. Yes, I probably should have looked at the article, but I was using a few spare minutes I had while waiting for a long printing job to complete, and didn't feel like taking on a major research operation, just in venting some spleen. If I have spattered some unjustified blood on Martin, I apologize.

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85) From Mizuno's book.

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Mizuno' s book "Nuclear Transmutation: The Reality of Cold Fusion" (Infinite Energy Press, Concord, New Hampshire, 1998) has already been referred to in my notes. It was translated and introduced by Jed Rothwell. I am going to summarize the book to capture what is worth remembering. Here is a general observation of this Japanese scientist. "The researcher must be sincerely interested and enthusiastic. Enthusiasm is what makes him willing to throw himself into work wholeheartedly. The key to good university research is to find the right topic and then concentrate on it. The university's job is to ensure an environment in which scientists can concentrate on their work, and ultimately return the benefit of their research to society. "

I think that Tadahiko was lucky to be well positioned (in terms of his experience at the Nuclear Engineering Department at Hokkaido University) to focus on cold fusion research as soon as the discovery was announced in 1989. He was working on targets used to produce neutrons. These were metallic foils saturated with deuterium. My own experience with such foils was limited but I remember how they were used. They were bombarded with accelerated ions to sustain nuclear reactions producing neutrons. The number of neutrons was proportional to the magnitude of the ionic current and to the concentration of deuterium atoms in the foil. Mizuno became an expert in preparing such targets. Such targets were made via electrolysis of heavy water solutions; the cold fusion technique used by Fleischmann and Ponds.

In 1971 he observed "excess heat" and speculated on its nuclear origin. Mizuno writes: "I had been performing exactly the same kind of experiments for more than twenty years, but I had completely overlooked the reported phenomenon." He is referring to what occurred in August 1978. The amount of water evaporated from an electrolytic cell in two weeks of continuos operation. was several times larger than what would be possible with the amount of electric energy received. In trying to explain what happened Mizuno was left with "only two possibilities: the electric current might has increased, rapidly electrolyzing all the liquid, or a large amount of [unexplained] heat caused the liquid to boil away. But at that time we could not imagine either of those scenarios, so we finally wrote off the incident as a mystery with no solution."

Mizuno became one of those who have studied various aspects of cold fusion for many years after the announcement of Fleischmann and Pons. Together with Akimoto he observed 2.45 MeV neutrons "at a level 10 - 20 times greater than background" but the rate of emission was much less than one per minute. Together with Azumi he observed tritium; the rate of production of that product was several orders of magnitude higher that production of neutrons. This alone was a good indication that a nuclear process responsible for neutrons and tritium must be very different from taking place in thermonuclear collisions (in hot plasma). The rate of generation of heat, calculated from the rate of tritium production, was shown to be too small to be observable. This was a good indication that the unexplained excess heat was not coming from reactions producing tritium.

In reading Mizuno's book one becomes aware that measuring excess heat was not the main priority; he and his colleagues were trying to demonstrate occurrence of nuclear processes in an electrolytic cell. But in one case the excess heat was extremely large; gallons of water evaporated in relatively short time would require more electric energy than was actually supplied to the cell. In 1994 Mizuno started using so-called "proton conductors." These are ceramic materials placed into hydrogen gas heated up to 1000 C. The electric field applied to a ceramic cylinder (via metallic contacts at the opposite faces) makes the protons (ions of hydrogen) drift through the material. Generation of excess heat was observed but the most interesting was production of new elements (alchemy).

Such observations have already been reported by other scientists but Mizuno and Ohmori were probably the first to

report on highly abnormal isotopic ratios. In 1996 similar reports were made, at a conference in Texas, by George Miley and John Bockris. Karabut's findings, described in the item 13 on my web site, confirmed these early observations. A discovery of new chemicals at a very low level of concentration always brings the issue of possible contamination; a discovery of highly abnormal isotopic ratios, on the other hand, can not be explained without accepting a nuclear process of some kind. In my opinion, isotopic shifts are the strongest arguments to support claims of nuclear transmutations. Asked what should a panel of experts (appointed again to investigate cold fusion) do I would answer "start by focusing on researchers reporting abnormal isotopic compositions." Competent mass spectrometrists should compare isotopic ratios from cathodes "before" and "after" the experiments.

Mizuno writes: "I immediately wrote up my results in a paper which I submitted to a number of journals. The submissions I sent overseas came back before long with referee comments turning them down. The reasons were: 1)Nuclear changes caused by chemical reactions cannot be accepted; 2) There is no theoretical explanation in the paper; 3) The writing and grammar were poor. I rewrote the paper and sent it again, but it was again rejected. In response to journals that had said there was no theoretical explanation, I wrote a theory, adding the proviso that it was strictly deductive (derived from data). They responded by rejecting the paper because the theory was too strange. In short, the policy was that no paper about cold fusion would be accepted under any circumstances, as I well understood." The paper was eventually accepted in Japan, after it was revised to satisfy comments made by the referees.

Much later Mizuno makes this interesting observation: " . . .it is not necessarily the case heat production means a [nuclear] reaction, an no heat means no reaction. There may be endothermic reactions that absorb heat instead of producing it. This is an important clue to understand the reaction mechanism. In many experiments until now, samples that did not produce heat were put aside and not analyzed. I believe important data may well have been overlooked when these samples were ignored." A panel of appointed experts (se above) should collect as many old samples as possible and measure isotopic ratios of all elements. A confirmation of abnormality could mean only one of two things; 1) Samples were deliberately fabricated to deceive; 2) Some nuclear processes were indeed taking place during cold fusion experiments. Recognition of chemically activated nuclear processes would mark the beginning of a new phase of investigation. In that phase cold fusion research would be treated like any other scientific field; it would no longer be labeled as pathological science.

Near the end of the book the author describes the 1996 theory of an Italian physicist E. Conte. Mizuno writes: "Up to now many theories to explain the mechanism of cold fusion have been proposed, but they have been inadequate. First came the ordinary D-D reaction hypothesis, then cracking (fractofusion), muon catalyzed, multibody, and neutron catalyzed fusion hypothesis. With each of them it was difficult to thoroughly explain all aspects of the phenomenon. Each of these hypotheses addressed a different aspect of the problem in an ad hoc fashion. Compared to the others up until now, Conte's theory is both simpler and more powerful because, by introducing a simple extension of the conventional quantum mechanical wave function, the theory accounts for all observed effects directly from the first principle. ....."

The theory is based on the assumption that, under right conditions, atomic electrons are able to get enough energy "to tunnel into the nucleus" and "combine with protons to form neutrons." The observed transmutations result from nuclear reactions induced by escaping neutrons. It is not clear to me what the word "tunneling" means in the context of electrons; the conventional wisdom is that an excess of energy leads to excitations or to ionization of atoms. As far as I know, a generally accepted theory of transmutations remains to be developed. On the other hand I am familiar with the K shell capture mentioned by Mizuno. It is "a form of radioactive decay in which one proton from inside the nucleus captures one electron from an orbit of the same atom to form a neutron." This process competes with the so-called "beta plus" decay; as a student I had a lot of difficulties with the theory of beta decay. The entire cold fusion field is still at the embryonic stage. Will it ever reach a stage at which at least one nuclear transmutation is 100% reproducible? (By this I mean a stage at which a competent scientist is able to demonstrate the effect in any well equipped lab.)

Appendix 1 and 2 contain two publications of Mizuno et al. In the second publication the authors report unusual isotopic composition among many transmutation products found on the cathode of their electrochemical cell. They write: "The anomalous isotopic distribution of these elements shows they do not come from contamination. For example, natural copper is 70% ^{63}Cu and 30% ^{65}Cu. But the copper found in the cathode was 100% of ^{63}Cu, with no detectable levels of

Cu. Natural isotopic distribution varies by less than 0.001% for copper." Either their data were fabricated to deceive or they are real. What else can it be? My own experience in working with a mass spectrometer tells me that such instruments cannot possibly be responsible for "an honest error" of confusing 30% with 0%. In the book Mizuno tells us that samples were examined in several laboratories specializing in mass spectrometry.

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86) Pseudoscience in Russia

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In unit 56 and 57 I shared what I read about con artists pretending to be scientists in America. It seems they have been much more aggressive in the former Soviet Union, and in the new Russia. I am rereading Kruglyakov's 2001 book "The Highwaymen of Science." The author has already been introduced in unit #10. What follows is my own translation of a letter sent by Kruglyakov to the president of the country V.V. Putin . The Russian text with signatures of three physicists (Alexandrov, Ginzburg and Kruglyakov, representing the Russian Academy of Science) can be found on pages 301 to 303 of the book.

"Dear Vladimir Vladimirovich:
Your goal, as you stated, is to recreate a strong and flourishing Russia. You know that the country's existing scientific potential must be used to accomplish the task. We want to assure you that the Russian Academy of Science is ready to contribute. At the same time we want you to know that the influence of pseudoscience is growing in our country. This is so dangerous that the Russian Academy of Science had to create a special commission to oppose pseudoscience and falsification of research. Democracy and freedom of speech, great successes of the post-Soviet period, in the absence of feedback control mechanisms, started to produce some negative results..... "

It occurred to me, after typing the above, that an English translation of this important letter probably exists somewhere. The Internet search engine, Google, was used to find what Kruglyakov wrote (in Skeptical Inquirer magazine Volume 26, Number 4, July/August 2002) about pseudoscience in Russia. The article can be found at:

http://www.csicop.org/si/2002-07/dangerous.html

It is a good summary of Kruglyakov's book; the letter to Putin is mentioned at the end. The letter is essentially a short version of what is written in the article. After realizing this I stopped translating and decided to use quotations extracted from the Skeptical Inquirer article. My own comment is added at the end.

****

"The end of the twentieth century was marked by a boom of astrology, mysticism, and occultism in many countries. In the USSR (during the last years of its existence) and then in Russia the situation was even worse in a sense. The system's collapse and the wreck of old ideals-along with the absence of new ones-caused many people to hope for some kind of miracle. The mass media contributed to this tendency. Through their irresponsibility, pseudoscience has filled newspapers, magazines, radio, and TV.

In recent years a new phenomenon has arisen. Pseudoscience has become a powerful, well-organized force. Over the last decade in Russia, about 120 academies have appeared, many of which don't deserve the name 'academy.'

Some of them give their stamp of approval to professionally inadequate doctors of science in various fields. Others do the same in pseudoscientific disciplines, giving diplomas to astrologers, UFOlogists, and others of the sort.

In Russia, even research institutes with pseudoscientific tendencies have appeared. I'll give only two examples: the International Institute of Space Anthropecology and the International Institute of Theoretical and Applied Physics. The

first has even managed to attain state accreditation with the help of the Russian Ministry of Science. The second has received financial support both from the Ministry of Science and the Ministry of Defense for the well-known swindle of torsion fields.

Peaceful coexistence between science and pseudoscience is impossible. From time to time, science attempts to unmask pseudoscience. The latter fights back with fierce hatred. Pseudoscientists are anxious to settle accounts with the Academy of Science, because the Academy is a great obstacle to these newly half-baked 'scientists.' Here are a few quoted statements of such people: . . . Thus pseudoscience predicts the full breakdown of science unless the scientific paradigm is changed. Meanwhile, according to a statement of academician Z.I. Alferov, recently awarded the Nobel Prize, '. . . the crisis in quantum physics is not observed. For the most part in the physics kingdom, it is calm now.'

Here is what one of the main theorists of the so-called science of torsion fields, 'academician' G. Shipov said: 'Now there is no doubt in the existence of telepathy, levitation, clairvoyance, retrovision, or that energy of consciousness plays some certain role in physical processes.' And since science does not recognize this, therefore, 'official science lags behind the new developments.' . . . Here is another case, involving M.D. Maley, chairman of the Interdepartmental Commission on Scientific and Technical Problems of the Industry of Defense of Security Council of the Russian Federation. The purpose he pursued looked rather reasonable: 'From the viewpoint of the Security Council, our task is to filter correctly the basic directions and orient the present and future management of the country with respect to a launching position of Russia in this scientific-technical revolution.'

To prepare for scientific breakthroughs, Mr. Maley created a 'Large State Research Center.' This is praiseworthy in itself; a high-ranking government official facilitates the development of a science. Alas, when one hears the purposes, you can't help being horrified at the ignorance of the official: 'Replacement of the concepts of quantum physics by neutron physics, vacuum as emptiness by the concept of neutrino fields is in prospect for us..... We have some works at the R&D [research and development] stages that contradict common sense and cannot be described by any equation.' . . . Astrologers, claimants of extrasensory powers, and newly appeared 'scientists' of other 'professions' more and more actively push themselves through into the State Duma, ministries, and even into the President's circle. Here are recent examples: This entire swindle is apparent to any physicist at once. Nevertheless, I had to carry out the official investigation. It revealed that Mr. Grabovoy never took part in tests of nuclear weapons in Semipalatinsk. Therefore, he did not test 'a crystalline module' there. At the same time, it was revealed that this 'doctor of technical and phys-math. science' has never defended any theses. In lists of the Italian Academy of Science, 'academician' Gravovoy was never mentioned. It is sad that the governmental Rossiyskaya Gazeta misled its readers; alas, not for the first time. . . .

Alternative medicine has dramatically developed. It is attracting numerous unscrupulous swindlers, robbing sick people who cannot find help from traditional medicine. New medical devices claiming to cure patients of any illness are appearing on the market. A device called 'New Cardiomag' recently became available at a price of only 500 rubles (about $16). It supposedly helps with hypertonia, ischemia, arterial hypertension, stenocardia, and headaches. One might question the honesty of developers of the device since one of them, doctor of medical science A.P. Naumov, has written in an advertisement for the 'Cardiomag' the following: 'This is an ecologically pure autonomous source of gravitation field, pulse bipolar current, and direct magnetic field with special energy characteristics (Isvestiya, March 14, 2001).'

It is incomprehensible why the Academy of Medical Sciences keeps silent about such fraud. It is time to express its opinion about that. The ever-growing activities of pseudoscience attempt to get money from the government, consumers, and industry while avoiding the standard procedures of review by experts. There are many examples of pseudoscientists managing to get money from state sources. The most well known is the swindle based on so-called torsion fields. In addition, there are some 'studies' on anti-gravity, and on transmutation of elements with an attempt to obtain gold (not involving the known method of nuclear reactions but instead a modern version of alchemy).

In such an atmosphere, at the end of 1998 the President of the Russian Academy of Science, academician Yu. W. Osipov, arranged a special Commission Against Pseudoscience and the Falsifications of Scientific Studies. One of the commission's very first actions was to prepare a special appeal that was considered and accepted by the Presidium of the Russian Academy of Sciences. This appeal was published in many Russian mass media outlets.

Members of the commission appeared many times in newspapers and magazines and on radio and TV, addressing the government with suggestions. I think I can say that this commission and its work with the mass media resulted in some corrections of the situation. Some allies appeared among journalists, astrological forecasts vanished from some newspapers, and some science sections appeared. In addition, scientists more frequently were asked to be guests on TV programs. However, these are only small steps. A victory over pseudoscience is still far away."

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Kruglyakov's book contains his March 16, 1999 paper entitled "On Problems of Opposing Pseudoscience." That paper was read and discussed at the meeting of the presidium of Russian Academy of Science. One of the academicians said: "It is true that wild and hard to accept hypotheses are sometimes published today; for example, that our planet is hollow inside. Such ideas should not be published. I am referring to existing but not yet understood phenomena; they might be erroneously characterized as false." In reading this observation, made by academician A.L. Jashin, I thought about some cold fusion reports. Kruglyakov's commission work is badly needed in Russia and I hope it will be successful. But his book categorizes cold fusion as pseudoscience. That is a big mistake, in my opinion. As a nuclear physicist the author is probably aware that the cold fusion situation is no longer the same as it was in 1990. But he writes as if cold fusion researchers still believed that excess heat is produced from conventional thermonuclear collisions.

In a review of Russian cold fusion conferences (pages 306 to 310) he prefers to ridicule rather than address the claims made by several researchers. What I have in mind is generation of helium (commensurable with excess heat) and unnatural isotopic ratios among the reaction byproducts. Instead of apriori dismissing such claims a physicist should examine the evidenceand ask for a demonstration. What was the basis for saying that Sovvatimova was "inspired by Bazhutov's model?" She presumably reported experimental data. Instead of being ridiculed she should have been invited to repeat the experiment in the presence of qualified scientists. Her work should either be dismissed or accepted on the basis of what was observed. The same applies to the claim made by Kaldomasov. What is wrong with trying another liquid? Several American workers reported excess heat and transmutations from an electrolytic cell based on common water.

What does cold fusion research have in common with astrology, mysticism and occultism? By the way, Edward Kruglyakov is laboratory head and deputy director of the largest institute of the Russian Academy of Sciences (Budker Institute of Nuclear Physics). Academician Kruglyakov is the State Prize (1986) and Artsimovich award (2001) winner. Born in 1934 he is a graduate of Moscow Institute of Physics and Technology. He is certainly not a pseudoscientists. But his attitude toward cold fusion does not look very scientific to me.

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87) Targeting a “straw man”

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My first opinion about cold fusion was based on Huizenga’s famous ERAB report and on what was written about the phenomenon in 1989 or 1990. Only recently did I realize that the criticism of Fleischmann and Pons was, in part, an attack on a straw man. Who was the first to introduce the term “cold fusion?” The essence of the initial announcement was the existence of unexplained excess heat; and the idea that it can not possibly be caused by a chemical reaction. But the scientists were attacked as if their main claim was observation of a conventional thermonuclear reactions at ordinary temperatures. That claim became the proverbial straw man. How can such reactions happen despite the coulomb barrier? Where are neutrons and protons always accompanying thermonuclear reactions? Where is tritium? Absence of such byproduct was then used to discredit the real discovery.

I knew Huizenga personally and I respected him as a scientist. But by leading the attack on a straw man he was acting more as a politician than as a scientist. Is he aware that a large number of researchers have now confirmed accumulation of helium in heat-generating experiments? And that the rate at which helium is produced is roughly commensurable with what would be expected from any kind of D+D fusion. Would he reject such findings because we still do not have a theory? Unfortunately, I do not know how to answer such questions. I was tempted to contact him recently but decided against it; Huizenga is probably very old now. His “successor,” Robert Park, seems to ignore new findings; who appointed him to be a spokesman for APS? Park uses the same arguments against cold fusion as Huizenga used 13 years ago.

The term cold fusion was partially responsible for the staw man arguments. My own attempts to stop using this term failed; this is reflected in essays posted at my web site. Last night I learned that Edmund Storms also thinks that the term cold fusion will eventually prevail. Here is what I wrote to him and how my e-mail message was answered:

Dear Dr. Storms:
I agree that the term “cold fusion” was partially responsible for bad reception of the discovery of excess heat in 1989. The alternative, LENR-CANR, is gradually replacing the undesirable phrase. But it is difficult to use this acronym in a lecture or in a conversation. First it is hard to pronounce, second, it is awkward to use. if it was up to me I would use an alternative. Suppose that the LECAN acronym is used as a single adjective, as in LECAN field, LECAN reactions, LECAN phenomena, LECAN opponents, etc. This is less rigid and easier to pronounce. Formally LECAN would stand for a long adjective: “Low-Energy-Chemically-Activated-Nuclear.”

I am just sharing an idea; it is probably too late to make a change. Am I the only one who is not happy with LENR-CANR? (You probably know that about 40 years ago the term “low energy nuclear reactions” was used to describe transmutations induced by projectiles whose energies were up to about 20 MeV. Later it became 200 MeV, or something close to it.) I am looking forward to meeting you in person at the ICCF-10 conference. Best regards, Ludwik Kowalski

Dear Ludwik,
The long form, LENR-CANR, is not intended to be used in conversation, only as the name of the web site. I frequently just use LENR, which is adequate. However, everyone knows what "cold fusion" means and it will cease to have the bad
connotation as the idea is accepted. I suspect, "cold fusion" will be the name most people will use in the future. See you at ICCF-10. Ed

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88) Rejections of cold fusion publications

Ludwik Kowalski (August 5, 2003)
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Montclair State University, Upper Montclair, NJ, 07043

On November 30, 2002 a nuclear scientist, E. Storms (1) sent a letter to the editor of Scientific American. Here is the content of that letter (2).

Dear Sir:
Your analysis in the December issue about why science is neither respected nor understood by the general public I found to be very much to the point. Those occasions when science accepts claims that are later found to be false clearly give science a black mark. Unfortunately, in an effort to avoid such embarrassment, science also rejects claims that are later found to be true. I ask you, which is the greater threat to science and mankind, accepting a claim that can have no possible benefit or rejecting a claim that can have great benefit?

I could offer many examples of how good ideas have been rejected in the past, but I would like you to consider one very important claim that now has almost universal rejection, yet is supported by a growing body of data. As a scientist, I was trained to judge the reality of nature from good data based on replicated experiments. Yet, I find that the scientific community increasingly bases what is real on the opinions of a few respected journals and academics using theoretical arguments, regardless of what is being discovered by other scientists operating in the real world. How is the general public expected to respect science when it does not follow its own stated rules of evidence?

The discovery I would like to use as an example of this double standard is what is called LENR or low energy nuclear reactions. This has also been given the very inaccurate name of cold fusion, a name that now causes rejection and ridicule. This ridicule comes from people who have no understanding about what is now known, yet their opinions are accepted as fact. Is this the way science is supposed to operate?

If you wish to be true to your stated wish to make science more respected, I suggest you educate yourself about this important phenomenon by reading information available at www.lenr-canr.org. There you will find over a thousand publications that support the reality of such anomalous nuclear reactions, as well as several reviews in full text that answer important questions raised by skeptics in the past. Serious scientists rejected "cold fusion" in the past for good reason. These reasons no longer apply. If science cannot correct a past rejection, then what good is the scientific method? Can anyone respect a scientist who cannot change his/her mind after being presented with better data? Respectfully, Edmund Storms, Ph.D.

Why was this letter not published in Scientific American? To understand it one must be familiar with the highly unusual polarization of the scientific community on the issue of LENR that occurred after 1989. In my opinion, claims made by prominent scientists should be presented to the general public, even when such claims are questioned by other prominent scientists. Scientific American has been doing this for over one hundred years. But the current editor of that journal, John Rennie, decided not to publish the letter. Here is the reply:

Dear Dr. Storms:
Thank you for your e-mail proposing that Scientific American reevaluate the status of LENR-CANR research and consider publishing an article on the subject. As you suggested, I did look over a number of the offerings at www.lenr-canr.org. Unfortunately, I still don't see evidence in those papers, or in the mainstream physics literature, that LENR-CANR has achieved any significantly new level of credibility in the eyes of the general

physics community. The site does point to a large number of publications that ostensibly offer evidence of the phenomenon, but sheer numbers of papers is not sufficiently compelling--as I'm sure you know, even the creationists can point to thousands of "publications" and "scientists" seemingly supporting their position.

I noticed that the LENR-CANR site, on its page appealing to readers to "help spread the word" about the phenomenon, described Scientific American as having "gone far out on a limb opposing cold fusion. They, along with the leaders of the American Physical Society and the Department of Energy, have made their institutions into bastions of opposition to cold fusion." We at Scientific American don't feel that we've gone out on a limb in criticizing cold fusion harshly in the past. As for our being a bastion of opposition to it, I don't think we have an intractably committed position. If LENR-CANR can be demonstrated satisfactorily for acceptance by the physics mainstream, we would be more than happy to publish more favorable articles about it. Your problem starts with establishing more credibility in their eyes, not ours. Sincerely, John Rennie

Referring to the above, Jed Rothwell (the manager of the LENR-CANR web site) wrote:

Rennie has dismissed 2,000 papers, including research from laboratories such Los Alamos, the Naval Research Laboratory and Mitsubishi Heavy Industries, mainly by equating these papers with creationism. Waving your hand and saying that "X is like creationism" does not make X resemble creationism. Cold fusion is an experimental finding, which is completely unlike creationism, or actual evolutionary theory, for that matter. It is not based on theory or an interpretation of the fossil record, but rather on long-established laboratory instruments and procedures in calorimetry, mass spectroscopy and so on. The only way to disprove cold fusion is to find technical errors in the techniques that are applied by specific cold fusion researchers.

The editor of Scientific American responded:

The people you need to convince about the scientific credibility of cold fusion aren't journalists. They're professional physicists who review submissions for respectable technical journals. If you can convince mainstream scientists that LENR-CANR is real and significant, magazines like Scientific American will drop into line.

In other words, he is saying that Scientific American is not a technical journal; its decision to silence a high caliber scientist was bureaucratic rather than scientific. I can understand this. But I do not think that Storms' letter would do any harm to the general public. On the contrary, it would promote interest in science. I can see the dilemma of the editor. He thinks that it is not his job to evaluate experimental findings; his own scientific background is likely to be different from what is needed. The editor would not publish an article on a LENR-CANR phenomenon unless it had been validated in purely scientific journals. But editors of leading scientific journals in the US also reject LENR-CANR papers. Let me illustrate this. In 2001 E. Storms wrote a review of the entire field (4) and tried to publish it in four journals: Physical Review, Review of Modern Physics, Chemical Review and Journal of Electroanalytical Chemistry. He was not successful. The disappointed author wrote:

The editor of the first journal said the Phys. Rev. does not publish reviews, although this is not true if the subject is to their liking. The editor of the second journal rejected the paper with the comment 'Cold fusion is a classic example of pathological science. I will certainly not publish articles supporting its disproven claims.' Three of four reviewers of the third journal rejected the paper because they did not think cold fusion is real and could not trust me to be unbiased in arguing this belief. The fourth journal rejected it because 'the subject was not appropriate for the journal.' Submission was done in series, with a new journal chosen after a rejection.

What is my own position on the situation in the cold fusion field? I used to say that the field is pseudoscientific. This was based on what I read from books, such as (6), and articles published shortly after the discovery of excess heat was announced by Fleischmann and Pons. But I am no longer comfortable with this position; recent reviews of the field, and numerous publications, seem to be very scientific to me. But I am still not convinced that the phenomena are real. Being an outsider I am waiting for a new evaluation of cold fusion phenomena by a competent panel of appointed scientists. I am also waiting for a single 100% reproducible demonstration of a nuclear effect resulting from a chemical process. Most physics teachers, like myself, are not equipped to conduct sophisticated research; that is why leaders of our

scientific establishment (scientists at APS, editors of major scientific journals and administrators of research granting agencies, such as NSF) should support calls for a new evaluation of the controversial field. My own call for a new evaluation, expressed in a letter to the editor of Physics Today, has not been published. That letter was prompted by the APS ethical guidelines article published in Physics Today. I wrote:

....As a physics teacher I am confused by the situation. What should we tell students when they ask about the discovery of Fleischmann and Pons? Most teachers have no time and no means to validate claims made in the area of "cold fusion," and need guidance. An objective summary of what has been done in that field, in the last ten years, would help us to describe it correctly. The issue is not only scientific; it is a topic of general interest. Most educated people know about the "cold fusion episode" and opinions about it are divided. Some say it was "a fiasco" while others say it was an "important discovery." How should teachers address this topic in the context of "public affairs between science and society," or in the context of discussing "institutional support for new ideas and innovations?"

After waiting several months I asked the editor about the status of my letter. The reply was: "So far, I have a split decision on the possible publication of your letter. I expect soon to have a tie-breaking input from a third reviewer. I will let you know as soon as I have a firm decision. Thank you for your patience." I was encouraged. But several days later I was informed about the rejection of my letter. On July 3, 2003, the editor, Dr. Hanna, wrote: "Our decision, after some valuable discussion, is not to publish your letter. Thank you for writing and for your interest in Physics Today." Curious about the rejection arguments I asked for the copies of reviewers' reports. Here is the reply to that request:

Thank you for your inquiry. Please let me explain. I know that scientists who submit articles to peer-reviewed scientific journals expect reviewers to give them a critique of their letters. Physics Today is not, in the strictest sense, a peer-reviewed scientific journal; it is, instead, a special-interest magazine for physicists. Generally, my reviewers are staff writers and editors (all physicists) who may give me little more than 'thumbs up' or 'thumbs down' on a letter. As a rule, we do not give out the specific comments of the reviewers, because we consider them to be internal business. Thank you for your inquiry. I hope my explanation has helped.

Likewise, my short note about the new situation in the LENR-CANR field was rejected by The Physics Teacher. The editor, Karl Mamola, wrote me:

Our editorial staff has completed its review of your manuscript 'On Reproducibility of Data in Cold Fusion Experiments.' The process included consultation with two of our referees. I regret to inform you of our decision not to publish the paper. The first referee offered only brief comments: 'I don't think The Physics Teacher is the right journal for this paper. Most readers of this journal are in no position to judge these rather esoteric matters. Indeed, most teachers wouldn't know what the fuss is all about.' In the light of the referees' comments and of our own careful reexamination of the manuscript, we believe that TPT readers would not be able to make sufficient use of the paper to warrant its publication. While we are not able to use the manuscript you submitted, we appreciate having had the opportunity to read it and we are grateful for your continued interest in The Physics Teacher.

The second referee wrote:

I must recommend against publication of this paper in *The Physics Teacher*. Despite the claim of 'helping us teachers,' the paper seems actually to be an attempt to have a serious journal endorse cold fusion as a research field to be taken seriously. The pages of *The Physics Teacher* are not the appropriate place to make such arguments. ..... Even if there were some validity to the now 14 years' worth of attempts at cold fusion (I haven't heard of any), *TPT* is about the last place in the world for this to be published.

I do not know why a person not familiar with new findings, such as abnormal isotopic ratios and commensurable accumulation of helium, was chosen to referee my note. How should rejections of LENR-CANR publications be interpreted? They reflect the situation in which, as observed by a friend (5), biasing prevents people from being objective readers of the book of nature. This, however, does not refer to the editor of Scientific American; I consider him to be a journalist with s scientific background. Only scientists read and interpret the book of nature. The friend

wrote: “If we become certain that LENR doesn't exist, then when Schwinger and Storms and McCubre later present positive evidence for it . . . it just proves their incompetence. Since we know that LENR is bogus, then positive replications are demonstrations that those labs are incompetent. Note that no amount of positive evidence can *ever* be convincing. A preexisting conviction acts as a mental filter.

But where did such a preexisting conviction come from? In science it's wrong to first adopt a viewpoint and then to use the selection of evidence in order to support that viewpoint. Politicians do it. The legal system is based on it. Science is totally different: a bend-over-backwards search for the truth rather than a defense of a an existing position during a debate. If one claims to be scientific, yet also adopts a position not based on evidence, then that is pseudoscience. It is a particular form of pseudoscience known widely as ‘Cargo Cult Science.’ One essential element is missing: the no-holds-barred search for truth.

Why would anyone ever adopt a firm position and start selecting evidence? One reason is ridicule. It's a known phenomenon in sociology. Once a person has ridiculed a particular concept, that person is trapped. They've surrendered their objectivity and their self-image is on the line. A huge conflict of interest arises because, if their ridicule was wrong, it's an embarrassing error of major proportion, and they've opened *themselves* up as a target for ridicule. Once such a conflict of interest exists, only an overwhelming amount of contrary evidence can ever sway them into reexamining their position. The practice of ridicule very often leads to a firmly closed mind, so scientists should avoid such behavior at all costs.”

I find these observations interesting; ridiculing political opponents is commonly used to avoid real issues. Robert Park and Edward Kruglyakov also use this trick in writing about cold fusion. They lean on arguments that were convincing in 1993 but not convincing in the context of recent findings. People like Park and Kruglyakov are spokesmen for the bureaucratic component of mainstream science. The editors of many journals find it safe not to be in conflict bureaucrats. It is significant, for example, that John Rennies wrote: “I did not see evidence . . . of the level of credibility;” he did not write: “I do not see evidence supporting the existence of cold fusion phenomena.” Why would anybody look at the LENR-CANR web site for the evidence of “credibility?” It is a site to look for the evidence of “existence” of cold fusion. Instead of ridiculing cold fusion its opponents should have a web site criticizing new findings in that field.
Such site would be very useful to those who are not able to read the book of nature in sophisticated laboratories.

Only highly trained scientists using expensive equipment are in a position to validate cold fusion claims. We read their papers and form our own opinion. This fact should be recognized; the situation was very different one hundred years ago. Neither editors of Scientific American, nor we, physics teachers, can perform experiments with sophisticated mass spectrometers in ultra-clean labs. In this new situation the role of a journal like Scientific American becomes even more important than in the past. I have no idea why Storms’ letter was not published. The author is a recognized authority in nuclear chemistry, a research veteran from Los Alamos, one of the best nuclear technology laboratories in the world. Why was he silenced by John Rennie?

P.S. Let me end with a positive indication. This morning I was informed that my letter to the editor of The Physics Teacher has been published (6).

References:

1) Edmund Storms is a radiochemist who worked on important projects at Los Alamos National Laboratory for thirty-four years. He is a recognized authority in the field of material science, as reflected in several books, and in over seventy research publications. His accomplishments, and his scientific biography, are described at <http://home.netcom.com/~storms2/index.html>

2) E-mail received from Dr. Storms (2003)
3) E-mail received from Dr. Storms (2003)
4) E. Storms, “Cold Fusion: An Objective Assessment.” The entire paper can be downloaded from the http:/www.lenr-canr.org web site.
5) William Beatty reflections are presented in the appendix of item #64 at my web site; http://blake.montclair.edu/~kowalskil/cf/

6) Ludwik Kowalski, The Physics Teacher, vol 41, June 2003 (see my item #97)

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89) Hydrinos again:

Ludwik Kowalski, <kowalskiL@mail.montclair.edu>
Montclair State University, Upper Montclair, NJ, 07043, (8/11/03)

Norm Winningstad, a retired engineer and businessman, probably read what I composed about hydrinos in unit 57. That is why, I suppose, he sent me an e-mail message with an attached pdf file. That file contained a recent article from Journal of Physics published by R. Mills and P. Ray (J Phys. D: Appl. Phys. 36, 2003, pages 1535 to 1542). The title of the article is "Extreme ultraviolet spectroscopy of helium-hydrogen plasma." I see that the authors do not use the term “hydrino,” the term “fractional Rydberg states of atomic hydrogen” is used instead. Am I right on this? Did the authors really demonstrate the existence of such states?" Unfortunately, I can not answer this question by myself; my background in spectroscopy is limited. The topic is likely to be familiar to astrophysicists; sun is made from a mixture of hot hydrogen and helium. Hopefully, some experts will evaluate the paper for people like myself. By the way, the role of authority in science is not at all minor; we depend on each others expertise.

The unit #57 on my list was triggered by a theory presented by Mills. That is why I wrote that existence of hydrinos is based on theoretical arguments rather than on confirmed experimental data. Was I wrong? It depends on how the new article will be evaluated by experts. My guess is that raw data will be confirmed but the interpretation will be challenged. Perhaps competent spectroscopists will find less exotic explanations of lines observed by Mills and Ray. In any case, I see no reason to associate hydrinos with cold fusion. Mills himself wrote (in a recent article on excess heat) a “hitherto unknown exothermic chemical reaction“ is responsible. In other words, he does not think a nuclear process is involved.

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Journal of Physics, as one can see at http://www.iop.org , is an electronic journal in which articles are said to be peer-reviewed. But what does this mean in practice?
To answer this question look at the following e-mail message. BLP stands for the research lab headed by R. Mills. Their web site

http://www.blacklightpower.com/theory.shtml

has links to published papers. Explaining how to search this site the author writes:

“Ludwik, I did a little digging, and I would recommend you go to the BLP web site, and click on the left on Technical Papers. Then at 2nd from the top, click on Abstracts. Then go to #1, about the NASA report. Go to # 45, which has a Journal of Applied Physics peer-reviewed article (2002) Vol. 92, No. 12, pgs. 7008-7022. #49, Journal of Molecular Structure. #62, IEEE Transactions on Plasma Science, Vol. 30, No. 2 (2002) Pgs. 639-653.

I will be the first to admit that it is frustrating to have most of the new articles listed as ‘submitted’ or ‘in Press.’ I assume the first means it is **not peer-reviewed yet**, and the second, that it is **peer-reviewed, but not yet printed**. You will note the list is long, largely because of the crescendo of writing lately at BLP as a result of their experimental work being fruitful, due to years of optimizing. I suspect the reason that they do not name the journals where the papers were ‘submitted,’ or where they are ‘in press,’ is because BLP experienced having papers through the peer-review process, only to have **the editor over-rule his own peer-review panel**, just before printing, because it was leaked to Park and/or Zimmerman, who blew into the editor's ear about how foolish he will look, if he publishes some thing contrary to Schroedinger's Equations.”

I do not know what “printing” means for the Journal of Physics; it an an electronic journal. What would prevent an

author from posting an article which was submitted but not yet reviewed? Or what would prevent an author from posting an article which was peer-reviewed but not approved by the editor? I wish the article of Mills and Roy were published in Physical Review, or another traditional journal. In that case seeing it on paper would mean that both the referees and the editor decided that the paper is reliable.

****

Browsing the Internet I found this web page: < http://www.phact.org/e/blp.htm >. The title is "BlackLight Power - do they have something significant?" Let me quote some pieces. Referring to Mills' theory somebody wrote:

Eric, I have a Ph.D. in physics and I have went through the mathematical and theoretical derivations of Mills in the book he published. In summary his results are incorrect. This is what he does.

1) He starts out with the 3-D shroedinger equation to make things look respectable.

2) Then he solves the z, theta, portions by separation of variables the usual way to make it look even more believable.

3) Then a miracle happens and he assumes that he can solve the radial, r, portion by assuming that r is continues and not quantized.

4) He uses this solution to rewrite all of his Quasi Quantum Mechanics and obtain all of his wacky results.

In summary, Mills got a hold of some undergraduate quantum mechanics books and rederived everything assuming that the radial part of the equation is continuous and not quantized. His results are BS

This resonates with what somebody else wrote about Mills' theory.

I have examined Mills's work, as posted on the BLP web site, in some detail. Since I haven't been to BLP I can't claim any knowledge of what's going on in his labs. However, I can say with total confidence that the theoretical aspects of Mills's work are utter rubbish. The "theory" of hydrinos is completely full of mathematical mistakes, from start to finish. As a work of theoretical physics, it's totally meaningless, and it's so badly flawed that there really is no way to "repair" it. For those of you who complain that the theory is often dismissed out of hand by professional scientists who do not give it due consideration, here's a bit of explanation for why the theory is so totally incorrect.

This is followed by a long point-by-point elaboration. Referring to published experimental results somebody else wrote:

Dear Eric:
I put another hour into blacklightpower.com. So far it seems that there are no papers in peer-reviewed journals on the website. The technical papers available seem to have no journal reference. The "papers" presented at ACS meetings are talks, not papers, and are not peer-reviewed. Technically, there is not much I can follow, but they say in the first "technical paper" that conversion of a hydrogen atom (whose existence as a free species is doubtful, but is on a metal surface apparently) to a hydride ion makes it smaller. The sizes on my big laboratory chart say the opposite. There was no obvious source of electron to make the negative hydride ion. A statement is made in the first technical "paper" that "Alkali nitrates are extraordinarily volatile, and can be distilled at 350-500 deg. C. This sounded wrong, so I looked up all the ones I could find in the CRC Handbook. Lithium nitrate decomposes at 600° C, sodium nitrate at 380°, potassium nitrate at 400° and cesium nitrate at over 400°. So two blunders were found, making the whole business suspect.

And here is the quoted abstract from the February 1996 NASA technical report
Document ID: 19960016952 N (96N22559) File Series: NASA Technical Reports Report Number: NASA-TM-107167 E-10118 NAS 1.15:107167
(NASA Lewis Research Center). Authors: Niedra, Janis M. et al.

Replication of the Apparent Excess Heat Effect in a Light Water-Potassium Carbonate-Nickel Electrolytic Cell

Replication of experiments claiming to demonstrate excess heat production in light water-Ni-K2CO3 electrolytic cells was found to produce an apparent excess heat of 11 W maximum, for 60 W electrical power into the cell. Power gains range from 1.06 to 1.68. The cell was operated at four different dc current levels plus one pulsed current run at 1 Hz, 10% duty cycle. The 28 liter cell used in these verification tests was on loan from a private corporation whose own tests with similar cells are documented to produce 50 W steady excess heat for a continuous period exceeding hundreds of days. The apparent excess heat can not be readily explained either in terms of nonlinearity of the cell's thermal conductance at a low temperature differential or by thermoelectric heat pumping. However, the present data do admit efficient recombination of dissolved hydrogen-oxygen as an ordinary explanation. Calorimetry methods and heat balance calculations for the verification tests are described. Considering the large magnitude of benefit if this effect is found to be a genuine new energy source, a more thorough investigation of evolved heat in the nickel-hydrogen system in both electrolytic and gaseous loading cells remains warranted.

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90) My talk at the 10th International Cold Fusion Conference

Ludwik Kowalski (August 10, 2003)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

The 10th International Cold Fusion Conference (ICCF-10, Boston) begins in two weeks. It is time start describing it. I am a nominal coauthor of a speculative theoretical paper presentation. The real author is Engvild, Kjeld Christensen; he is from Risoe National Laboratory in Denmark. Knowing that I am going to the conference he asked me to submit the poster as a coauthor. It was one day before the deadline. My own presentation, also submitted as a poster, was turned into a 25 minute talk. Instead of repeating what is in the poster (voices from Phys-L about cold fusion) I decided to share personal reflections based on what I learned about cold fusion. The view of an outsider, I suppose, may of some interest to researchers.

[#1] A preliminary comment on irreproducibility
Physics teachers know that electrostatic demos are highly irreproducible.

This must have been frustrating before the surface effects of humidity were recognized.

[#2] Another preliminary comment
Suppose that four probes were deposited on our planet by creatures living somewhere else in the universe. Conditions on their planet are totally different from ours. The probes landed in the same spot but several days apart. The first showed a wind of 50 mi/hr blowing north, the second and the third showed no significant wind while the fourth showed a wind of 30 mi/hr blowing to the west. Should the investigators conclude that the idea of moving air, formulated after the first reading, is erroneous? Certainly not. They should keep experimenting.

[#3] Last preliminary observation
Cold fusion is highly controversial. **What should a confused physics teacher do?**
(a) Avoid the topic because authorities declared cold fusion to be non scientific 13 years ago?
(b) Risk his or her reputation and discuss the issues?
(c) Play it safe and support official pronouncements?
Those invited to look into **Galileo's telescope** were in a similar situation**.**

[#4] I am Ludwik Kowalski,
a physics teacher from Montclair State University, Upper Montclair, NJ, 07043

The synopsis of this talk is available on my web site devoted to cold fusion.
To see it click on item #90 at **http://csam.montclair.edu/~kowalski/cf/**

[#5] Why am I here?
(a) I am interested in cold fusion. (b) I want to meet active researchers. (c) Are cold fusion phenomena real or not? I want to know the truth. Finding the truth is very difficult for an outsider. Let me show you a letter from a student **-->**

[#6] A letter from a student:
Dear Mr. Kowalski,
Help! My name is Maggie Johnson and I am a sophomore at Saratoga High School. In my chemistry class, I am doing a project on Cold Fusion. I was looking on the Internet for web sites on Cold Fusion, and I came across links to your Cold Fusion items. I was wondering if you could give me some advice or information?. . .

[#7] How to answer such requests?

A year ago I would reply that cold fusion is pseudoscience. But I am no longer comfortable with this kind of reply.

[#8] Why am I not comfortable?
My first opinion about cold fusion was based on Huizenga's famous ERAB report. I knew the author personally and I respected him. His criticism of cold fusion was convincing because it was based on the **"straw man,"** that is, on the idea that cold fusion is a thermonuclear reaction between two colliding hydrogen ions. Did he create this straw man or was it done for him by the unfortunate name "cold fusion?" Was he aware that he was fighting a straw man? I do not know.

[#9] Old arguments.
Those who criticize cold fusion today, Park in the US and Kruglyakov in Russia, essentially repeat Huizenga's arguments. What was convincing in 1989 is no longer convincing today. (a) Why do they ignore generation of helium? (b) Why do they ignore more sophisticated calorimetry? (c) Why do they ignore unnatural isotopic ratios? (d) Why are they not here learning about such findings?

[#10] I am still questioning.
After becoming aware of the "straw man" aspect of critical arguments I started questioning my own position. I am still not convinced that cold fusion is real but I no longer say that it is voodoo science. My reply to Maggie was limited to giving her references: Mallowe and Beaudette. I kept reading and learning.

[#11] Sharing what I know.
I decided to share what I learned about CF with other teachers. I did this on my web site: , http://csam.montclair.edu/~kowalski/cf/ Feel free to explore my short essays and to share them with others.

[#12] Why am I still puzzled?
(a) I am not able to perform experiments in school. This is likely to be typical.
(b) How comes that hundreds of sophisticated research scientists, trying to convince skeptics, have not developed a 100% reliable demo in 13 years? This is very indicative; windows of opportunity did exist in several countries.

[#13] What kind of evidence would convince a teacher?
(a) Not **calorimetry** in an electrolytic cell, unless the excess heat is very large (both in absolute value and percentage wise). Only experts can interpret very small effects.
(b) **Induced radioactivity** measurable with a Geiger counter, or with a gamma rays spectrometer, or with CR39? Oh yes, we are ready to test for radioactivity.
(c) **Reduced radioactivity**? Not very convincing; how can one be sure that the effect is not due to redistribution of materials within a cell? It can also be due to a removal of a layer of filtering material. Only experts can answer such questions.
(d) **Isotopic abnormalities?** Oh yes! This would be very convincing if sophisticated equipment were available in our schools.

[#14] How do we become convinced?
(a) Students learn from teachers and textbooks.
(b) Teachers learn from textbooks and journals.
(c) Scientists learn by performing experiments in the areas of their expertise.
(d) Bureaucrats follow administrative directives.
Which of these methods convinced you that neutrons and protons are made from quarks and gluons? Most likely you accepted a published consensus reached by experts.

[#15] Strategy for success?
(a) Convince **science teachers** by offering a reliable demo. Even a 70% reproducible demo would be useful.
(b) Convince **experts** with abnormal isotopic ratios. Convince **experts** with excess heat and with generation of 'ashes.'
(c) Demonstrate to **general public** that your most active opponents use 'straw man tactics' and that they ignore recent findings.
(d) **Do not make unrealistic promises** to general public. Keep making your papers available over the Internet.

(e) Openly criticize voodoo science.
(f) Publicize unfair practices of editors, and other bureaucrats.

[#16] Deplorable practice
Cold fusion papers are often published in journals which also publish papers dealing with antigravity, hydrinos, quantum healing devices, practical energy from zero point fluctuations etc. etc. Your papers are mostly experimental, they are not based on speculative extrapolations or on wishful thinking. But a reader may be left with such impression. The idea of having an electronic journal devoted only to cold fusion topics is very good, IMHO.

[#17] A new appointed panel to investigate cold fusion?
Most teachers are waiting for a consensus on cold fusion. They would welcome an official evaluation of new cold fusion findings. NSF first, DOE later! In other words, the primary focus should be on scientific findings and not on possible practical benefits. The evaluation should not be rushed, as the ERAB report was in 1989.

[#18] Trying to publish
Two times I failed to publish an appeal for the second evaluation of the cold fusion field. It was something like this: Can a nuclear process be triggered by a chemical process? The answer, based on what we know about nuclear phenomena, is negative. On the other hand many experiments seem to indicate the opposite. These experiments were performed many years after the first evaluation of "cold fusion" was made by our Department of Energy. As a teacher I would very much appreciate a second evaluation of the field by a panel of competent investigators.

[#19] Rejections
But my calls for the evaluation of new cold fusion claims were defeated by journal editors, and by their referees. I am referring to my jornals: Physics Today and The Physics Teacher.

[#20] From the rejection letter:
"The first referee offered only brief comments: 'I don't think The Physics Teacher is the right journal for this paper. Most readers of this journal are in no position to judge these rather esoteric matters. Indeed, most teachers wouldn't know what the fuss is all about.' The second referee's comments are attached [see below]. In the light of the referees' comments and of our own careful reexamination of the manuscript, we believe that TPT readers would not be able to make sufficient use of the paper to warrant its publication."

[#21] The second referee wrote:
"I must recommend against publication of this paper in *The Physics Teacher*. Despite the claim of 'helping teachers,' the paper seems actually to be an attempt to have a serious journal endorse cold fusion as a research field to be taken seriously. The pages of *The Physics Teacher* are not the appropriate place to make such arguments. . . .
[#22] The second referee continues:
" ..... Even if there were some validity to the now 14 years' worth of attempts at cold fusion (I haven't heard of any), *TPT* is about the last place in the world for this to be published." **Why was this individual chosen to be a referee?** S/he is not aware of progress made since the discovery of CF was announced in 1989.

[#23] From the Editor of Physics Today:
"So far, I have a split decision on the possible publication of your letter. I expect soon to have a tie-breaking input from a third reviewer. I will let you know as soon as I have a firm decision. Thank you for your patience."

[#24] What is next?
Sending my letter to referees was seen as an indication of objectivity. But two days later I was informed that Physics Today will not publish my letter. I asked for the comments made by referees.

[#25] From the Editor of Physics Today
". ....Please let me explain. I know that scientists who submit articles to peer-reviewed scientific journals expect reviewers to give them a critique of their letters. Physics Today is not, in the strictest sense, a peer-reviewed scientific journal; it is, instead, a special-interest magazine for physicists. Generally, my reviewers are staff writers and editors (all physicists) who may give me little more than 'thumbs up' or 'thumbs down' on a letter. As a rule, we do not give out

the specific comments of the reviewers, because we consider them to be internal business.

[#26] Predicting reconciliation
In my opinion, reconciliation between cold fusion and mainstream science is possible. But can this be achieved by a loose collection of individuals working on separate experiments? Those who ridicule you rely on a bureaucratic structure. Try to bypass them.

[#27] Reason for optimism?
"The scientific process is self-corrective. This unique attribute sets science apart from most other activities. The scientific process may on some occasions move slowly, sometimes even along a circuitous path. The significant characteristic of the scientific method, however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error." Who wrote this? Your critic, John Huizenga.

[#28] From the 1989 ERAB report
"The Panel recommends against the establishment of special programs or research centers to develop cold fusion. However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system."

[29] What will the history verdict be?
(a) Sooner or later, perhaps in 50 years, the cold fusion puzzle will be resolved.
(Like the "puzzle of cybernetics," or the "puzzle of genetics," both in USSR.)

(b) There are only two possibilities: cold fusion phenomena are real or they are not real. But this will not be the end of the story. The social aspects of scientific progress should be addressed.

[#31] If confirmed
If the reality of cold fusion is confirmed then causes of its condemnation by the scientific establishment must be studied. How to make sure that **a bureaucratic barrier is never again used** to slow down research?

[#32] If not confirmed
On the other hand, if cold fusion is shown to be fictitious then the long-lasting self-delusion, involving hundreds of recognized scientists in several countries, must be explained. **How do we know that such self-delusion does not happen more often than we think?** In any case, Cold fusion has been a significant event in the history of science; it will not be forgotten. It will be like the opposition to Galileo, several centuries ago.

#[33] What is "an impossible phenomenon?"
(a) A phenomenon not accountable by our current conceptual model (theory).
(b) A phenomenon that does not exist in the real world (it is a product of imagination).
Do not confuse these two meanings in a debate.

[#34] Nobel laureates
J. Schwinger and E. Teller, Nobel laureates, are often mentioned in cold fusion debates. But I did not know about a very critical review of R. Park's "Voodoo Science" book by another laureate, B. Josephson at:
http://www.tcm.phy.cam.ac.uk/ ~bdj10/articles/park.html

[#35] THANKS FOR LISTENING

http://csam.montclair.edu/~kowalski/cf/

kowalskil@mail.montclair.edu

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Teachers debate cold fusion

http://blake.montclair.edu/cf/poster.html

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What follows is a collection of messages about cold fusion from teachers. The messages were posted on the Internet discussion list, Phys-L, or were sent to me in private. They illustrate a wide range of opinion. PHYS-L is a list dedicated to learning and teaching physics with 700 subscribers from over 35 countries, the majority of whom are physics educators. To search in the archive of messages posted on Phys-L go to:
http://lists.nau.edu/archives/phys-l.html

Teacher 1:

The topic of cold fusion is most interesting but I'm surprised that it is still a topic of current debate on this list-server. **I recall that someone reported that cold fusion had been achieved a few years ago but no one has been able to reproduce the effect to this date. However, I thought that most scientists now agree that cold fusion can never be achieved.** Is there any new evidence to the contrary .....or is it like the alchemists who spent many fruitless decades trying to turn lead into gold?

Teacher 2:

I think there exists **new evidence for three things**:
1) Anomalous heat is now more reproducible, and not only via electrolytic loading. It has been observed in a deuterium gas discharge tube with a palladium cathode, and in a vessel containing palladium and hot deuterium gas at high pressure. But success depends on factors which were not known when cold fusion was announced.

2) Unusual nuclear processes (transmutations, emission of nuclear particles and electromagnetic radiation) accompany generation of excess heat. This happens in electrolytic cells, in gas discharge tubes and in high pressure vessels.

3) These puzzling processes do not resemble familiar thermonuclear fusion taking place in very high temperature plasma. The ratio of tritons over neutrons, for example, is highly skewed (by many orders of magnitude) in favor of charged particles. Another dramatic difference is that the reported production of 4He is not associated with the emission of 23.8 MeV photons; the released energy appears as heat.

Most of us are not able to verify these findings. That is why I think that i**t would be desirable if a new panel of experts** (physicists, chemists and material scientists) were created by our scientific establishment to evaluate the validity of recent findings and claims. I do not want to be like those who refused to look at what Galileo was showing because, according to Aristotle, such things were not possible.

Teacher 3:

The whole atmosphere around CF has been filled with **poisonous material**, some valid and some emotional. One must be very careful, on entry into such an atmosphere, to be protected by a useful theoretical proposal or at least a plausible explanation that can be subjected to experimental tests. **On the basic level there are two obvious questions:(1) How could hydrogen atoms fuse at such a low temperature? (2) If they do fuse,** how is the energy released (if not in gamma rays, then how) i.e. what reaction occurred?

If one has no proposed answer or proposed experiment to get an answer, then one is **in a state of massive weakness.** Your message seems to be that there is new evidence for an interesting mystery, and the early workers were not fairly treated. **The author of such a message will be classified as an apologist or defender, no matter how he qualifies such words.** If, however, he has a plausible proposal, it could possibly be different. I infer that the major skepticism in the mainstream nuclear science community stems from the silence on the basic two questions above. Such skepticism seems to me to be justified until something reasonable is proposed or, better yet, demonstrated. Until then, essentially all responses will be "impurities or errors".

Teacher 4:

First of all, just because someone says 'a new result is only accepted if there is at least a plausibility argument advanced to support it' doesn't make it so. **You don't have to look any further back than the announcement of high-Tc superconductors to see that such is not the case.**

Teacher 5:

When **challenging the "laws" of physics** there's a right way and a wrong way to go about it. (The same applies to any other activity.)

A1) The rules need changing, and the scientific community handles it well.
A2) The rules don't need changing, and it is handled well. Example: The null results of Eötvös.
B1) The rules need changing, and it is handled poorly.
B2) The rules don't need changing, and it is handled poorly. Examples: N-rays, cold fusion.

And there is a fifth class, where the scientific community responds scientifically but fails to bring the broader society along. Examples: copper bracelet therapy, magnetic bracelet therapy, homeopathic medicines. The task of challenging established ideas is not assigned only to giants like Michelson and Rumford and Rutherford, but also to every worker-bee in the scientific community. To summarize:

-- Primarily we should discuss the right way to challenge the established rules. And the necessity for doing so.
-- Secondarily we should discuss N-rays, cold fusion, homeopathy, etc. as counterexamples, as perversions. We shouldn't call them "positive".

Teacher 6:

The skepticism about "cold fusion" arises not because of any "conspiracy" on the part of "orthodox" science, but rather because literally **hundreds of competent scientists have attempted to reproduce the effect without success.** The nuclear reactions associated with fusion are well understood, and have well known signatures (reaction products such as neutrons and gamma rays). These have been looked for with the most sensitive of detectors, and have not been found.

Teacher 7:

I believe that it is nigh-impossible to change people's opinions regarding "Cold Fusion," so I usually am not tempted to dive in and argue about it. When any reversal of opinion requires the losing of face, then reversals of opinion cannot occur in public. Therefore why even try? If "CF" is eventually shown to be valid, then everyone will leap on the bandwagon, but there will be no detailed investigations of the ones who spent years ridiculing the topic. "Who, me? I was always a supporter!"

Teacher 8:

I enter this discussion with great fear, but I can't help getting this out of my system.
(1) Can we agree that there can be a small amount of **muon induced fusion** in a tabletop experiment, but not enough to make significant temperature changes?
(2) Can we agree that the other types of cold fusion that are being discussed **require us to violate Coulomb's Law and therefore are unlikely to occur?**

Teacher 9:

If the CF phenomenon is genuine, then it means that the staggering amounts of money put into Tokamak-style fusion might have been wasted. It means that hundreds of people devoted their careers to a technology which might prove of little worth should electrochemical-fusion result in efficient reactors. Obviously the pressures on such people would be tremendous.

Teacher 10:

In my reading of CF literature, **I've not encountered any "conspiracy theory" stuff.** In the "perpetual motion" and "antigravity" crackpot fields **the situation is far different.**

Teacher 11:

One reviewer wrote: “Professor Park does more than debunk, he crucifies... You'll never again waste time or your money on astrologers, quantum healers, spoon benders homeopaths, perpetual motion merchants, or alien-abduction fantasists.” But isn't "cold fusion" different from the above? I suspect that Fleischmann and Pons might become Nobel laureates.

Teacher 12:

What makes the CF area different from voodoo science?

1) A large number (several hundred) of cooperating scientists in about 10 countries are involved.
2) Two Nobel laureates (Teller and Schwinger) were theorizing about AE at one time.
3) Nearly all of the AE researchers have doctorates; many of them are (or were) associated with highly prestigious laboratories and universities.
4) These researchers organize one international conference each year and make results of their findings known to all who are interested.
5) Their methodology of validation is not different from that practiced by so-called "mainstream" scientists.They experiment, they hypothesize, they change their minds, they publish.
6) They are not secretive; they want to be heard and be criticized scientifically.
7) They want to have access (as authors) to all mainstream journals in order to benefit from the peer-review process.
8) They want their proposals to be considered by NSF, DOE and other granting agencies.
9) They are highly unhappy about the "blacklisting” of the entire field triggered by mistakes of those who tried to exploit the prematurely announced 1989 discovery.

Is it not obvious that claims made by “astrologers, quantum healers, homeopaths, spoon benders, perpetual motion merchants, or alien-abduction fantasists” **are completely different from those made by AE scientists?**

Teacher 13:

In science it's wrong to first adopt a viewpoint and then to use the selection of evidence in order to support that viewpoint. **Politicians do it. The legal system is based on it.** Science is totally different: a bend-over-backwards search for the truth rather than a defense of a an existing position during a debate. If one claims to be scientific, yet also adopts a position not based on evidence, then that is pseudoscience.

Teacher 14:

The cold fusion area is not voodoo science. But the phenomenon of pseudoscience is very real and society should be protected from those who exploit ignorance in order to benefit from unscientific claims. Selling healing bracelets is one example of this; the therapeutic effect of such gadgets, as far as I know, has not been validated. How can society be protected from con artists without confusing charlatans with honest scientists addressing non-conventional topics? This is a difficult issue.

Teacher 15:

I see CF as classic pathological science, as described by Langmuir. I cannot say for certain that CF isn't real. I suspect nobody can. But I would guess, based on the number of the characteristics of pathological science it has exhibited, the odds against it are pretty long.

Teacher 16:

It's not hard to destroy your career as a scientist. Simply take UFO sightings/evidence seriously and devote major time to investigating them. Or dedicate yourself to researching parapsychology. Pick any one of a number of "taboo" subjects such as Cold Fusion to study, and the greater scientific community will excommunicate you by closing off funding and the channels for publication.

Teacher 17:

It is very difficult to find a word to describe what it is that motivates the honorable person in his or her search for scientific truth. It is not exactly curiosity, which can be much more easily satisfied in the library. Feynman refers to the thrill of being the sole possessor of a secret of the universe for a few moments and, subsequently, revealing that secret to the world. There must be something involved in this feeling that comes from the ego, but that is not to say that the feeling should be disparaged.

Teacher 18:

The cold fusion situation in the USA can be characterized as a conflict between a group of scientists and the government (represented by DOE and NSF). This reminds me of two other conflicts of that nature; both in the former USSR. Soviet geneticists and Soviet cyberneticists were “excommunicated” on ideological basis rather than on the basis of scientific argumentation. These fields, labeled as “reactionary science,” were later rehabilitated. (But many “voodoo scientists” died in prisons and concentration camps before being rescued.)

Teacher 19:

What might skeptics say about recent cold fusion data?

1) Muzino and others in Japan are liars, like Karabut and others in Russia, like Bressani and others in Italy, like Lonchampt and others in France, like Bockris and others in the US. The data are fraudulent.

2) These people only pretend to be scientists. Their Ph.D. diplomas were counterfeit; their professorships at famous universities were bought; the books and hundreds of articles they published were produced by somebody else. They are members of an international “mutual support society.”

3) We already know everything about nuclear phenomena; facts which disagree with existing theories are not acceptable. Absence of commensurate amounts of neutrons and protons is a sufficient reason to ignore claims about unusual nuclear processes.

4) Cold fusion researchers were often wrong in 1989. Therefore what they are finding now must also be wrong. They should never be forgiven for announcing a discovery via a press release, or for claiming that excess heat experiments are very simple.

5) Claims made under the banner of cold fusion were not described in articles published in leading journals. Therefore they cannot be accepted. The editors of these journals refuse to publish cold fusion articles; they know better what is right and what is wrong.

6) Neither the Department of Energy nor the National Science Fundation supports research in the area of cold fusion. Therefore such research is not worth taking seriously.

7) Practical applications of cold fusion have not been demonstrated; therefore the underlying phenomena cannot be real.

8) We know nothing about recent cold fusion findings; therefore they must not be correct. The entire field was declared pseudoscientific in 1989 by a panel of experts. The opinion of experts must be respected; it can not be challenged by new findings.

9) Experiments should be 100% reproducible before they can be accepted.

Only the last reservation is sensible. Lack of reproducibility is a clear indication that some important parameters are still not under control by experimentalists, and that conclusions are tentative.

Teacher 24: (about anti-relativists):

The most basic problem with the "anti-relativists" is their intellectual dishonesty -- the refusal to think and recognize their errors no matter how many times, or in how many ways, such errors are clearly identified by those who actually understand. This sort of epistemological/psychological aberration is not anything new to relativity. In a 1922 letter to Max Born, Einstein incidentally mentions a "monumental blunder" he made "some time ago (my experiment on the emission of light with positive rays)." In his commentary on this letter, in the book "The Born-Einstein Letters," _Macmillan_, 1971, Born first mentions the "anti-relativists" who fail to learn from their mistakes as Einstein did. In one paragraph, Born notes:

"Finally, there are the pure cranks, outsiders who can point to no positive scientific achievements themselves but who believe that they have found defects in some new doctrine such as Einstein's theory of relativity. One would think there would be fewer of these as time goes on. But this is not so. Over the years a large number of first-class physicists and mathematicians have thoroughly investigated the theory of relativity and none has found fault with it. It is hard, therefore, today to take seriously anyone who believes he has discovered a mistake. **I have frequently taken the trouble to uncover the errors in papers written by cranks of this type, but never in all my experience has any of them admitted that he had made a mistake, as Einstein did."** Born's comments were written a long time ago, but his words remain true today. There will always be a small but hard-core group of intellectually dishonest "anti-relativists" who neither

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A preconference C.F. workshop (8/24/03)

Workshop Objective:
This workshop is intended to provide an introduction to the field [of cold fusion] for persons interested in gaining a basic background in the science and technology aspects. Both theoretical and experimental topics will be covered for deuterium and proton based reactions.

1) **Introduction and course objectives**,
George Miley (15 min)
2) **Introduction to LENR,**
Ed Storms (45 min)
3) **Review** of experimental measurements involving DD reactions
Mike McKubre (1 hour).
4) **Review** of experimental measurements involving Transmutation Reactions
John Dash (1 hour)
5) **Working Lunch** Discussion session
led by George Miley (1 hour).
6) **Theoretical background for D reactions,**
Peter Hagelstein (1 hour)
7) **Theoretical background for Transmutation Reactions,**
Akito Takahashi (1 hour).
8) **Diagnostics for measurements** (Calorimetry, He4, Transmutation products/isotopes, Radiation emission, Metallurgical analysis)
George Miley, Mike McKubre, Ed Storms and John Dash (1 hour).
9) **Summary and discussion,**
Panel, G. Miley, chair and lecturers. (30 min)

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93) Cases from Russia

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The situation in Russia, as described by Kruglyakov, can be found in the unit #86 of my list. This units focuses on selected fragments from his 2001 Russian book, "The Highwaymen of Science." On page 93 one reads that a claim for the "excess heat" was made in Russia in 1959, long before cold fusion was discovered. Several engineers from a Soviet factory, "Santechnika" [which translates into medical technology] announced a device constantly generating more heat than the amount of electric energy received. Scientists dismissed the claim but a poetess, Marietta Shaginian, came to the defense of inventors. She wrote; "**don't these scientists realize the importance of the discovery; it is a technological revolution!**" Her defense was widely publicized and created great excitement in the entire country. Finally Kapitsa, and other important scientists, had to be involved. "Fortunately, it was not difficult to find an error: an increase of temperature in the surrounding water had been ignored."

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Another claim goes back to 1970s and the pseudoscientist is A. Cherniecki; in his case the rate of heat generated was several times larger than the rate of electric energy supply. The inventor believed that physical vacuum was supplying excess energy to his system. **"Many supporting articles, appearing in the press, were presenting only one side of the story. Three years later, however, it became known that the inventor, using various pretexts, did not allow experts to measure the input power."**

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After describing old claims Kruglyakov jumps to 1980's and focuses on Akimov, an active promoter of torsional fields which are said to propagate much faster than light, which can cary information, and which can kill. Kruglyakov reprints his letter to the editor of a widely read Russian Newspaper (RN). That letter exposes con artistry of Akimov in the former Soviet Union and in new Russia. It also criticizes harmful policy of the government newspaper to support pseudoscience. I could not stop thinking about similarities between E. Kruglyakov in Russia and R. Park in the USA. Why do these two physicists place cold fusion in the same boat as torsional radiation and other speculative (as opposed to experimental) claims? Don't they see obvious differences between situations in the field of cold fusion (see item #18) and situations in real pseudoscience areas (see items 56 and 57)? Significant differences between situations in two countries, however, did not escape my attention.

Instead of focusing on Kruglyakov's letter to the editor, entitled "On the other side of science," let me show the reply produced by the science editor of R.N. Valentinov. The reply, entitled "On that side of science," appeared on May 19, 1998. It illustrates position of a science editor who covers his support for sensationalism by pretending to be open-minded. Valentinov wrote:

[Kruglyakov] **"often accuses those who give money to Akimov -- the Science and Technology Commission, the Ministry of Defense and the FSB [the successor of KGB].....I suppose that defense specialists in charge of**

evaluations of Akimov's projects, and generals handling finical aspects of his work, will answer for themselves, if they decide to raise the curtain of secrecy a little. Kruglyakov accuses them to be scientifically and technically illiterate. His main target, however is not the army, it is the press. He asks: 'why are pseudoscientific projects, costing a lot of money, are advertised, rather then criticized, by popular press?' That money should be used to support research in mainstream science. "

If my memory can be trusted, the hypothesis that every rotating object, including elementary particles, is surrounded by the torsion field was suggested in 1902. But the field is so small that they can not be detected by the existing instruments. . . And then [in 1980's], Shipov and Akimov, asked: 'is it true that these fields cannot be measured?' That resulted in a scandalous accusation." Akimov is not the only one working in this field [names of researchers are provided]". Kruglyakov claims that journalists, informing society about work of pseudoscientists," are hurting real science.

"But how do journalists, reporting what they hear from researchers, can interfere with progress of science? . . . Was it us, journalists, who were responsible for wild accusations of our brilliant geneticist, Vavilov? Did we cooperate with those who killed the genius in the Vladimir prison? Are we responsible for the persecution of the cybernetician, Glushkov? That persecution delayed our scientific progress by decades. No, this was done by their scientific colleagues who 'paved the way for real science.' Consider N.A. Kozyrev, who found the new way to investigate time. . . . Was it us, journalists, who sent him to Stalin's camps for ten years? . . . No, all this was done by his colleagues, often academicians [Kruglyakov is an academician]." Kozyrev was later recognized for his discoveries, but "mental inertia" of scientists was responsible for attempts to keep this fact unknown.

"I am well familiar with this affair. I remember how academician Krat, the director of Pulkowski observatory, was trying to prevent me, physically, from interviewing Kozyrev in his office. Then he threatened me with consequences along the party line, in case I dare to publish the interview. But I did publish it and was nearly expelled from the party, and from work. Fortunately some vigilant journalists supported me from pages of official party newspapers. . . .

Explaining his own 'path to truth,' Kruglyakov refers to specialized journals where any submitted paper is independently evaluated by two or three experts and by a collective of editors who are recognized scientists. This process is supposed to designed to eliminate erroneous publications. And it often does this. But it is not uncommon that a work declared to be 'erroneous' turns out to be highly innovative. Academician Kruglyakov, I suppose, would like scientific publications in newspapers to undergo the scrutiny of experts. This implies censorship. Our report on the work of Akimov would never be published under such system. The same would also happen to all publications challenging somebody's obsolete claims, questioning somebody's competence or not convenient for other reasons."

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Kruglyakov book contains comments on Valentinov's reply. Ridiculous nature of the stuff published in RN is illustrated by the following quotation from a published article:

"Loud cursing over a plant was like [an X ray] dose of 40 thousand roentgens. It resulted in the braking of the DNA chains, in the decay of chromosomes and in the scattering of genes. ..... Grains of wheat, after receiving a dose of cursing equivalent to 10 thousand roentgens, developed normally after being blessed."

This reminds me of "pet stones" that were advertised in the US, about two decades ago. The author's main point is that RN, is more interested in publicity than scientific integrity, that its scientific editor is a hypocrite and that promotion of ignorance harms national interest. Later in the book Kruglyakov writes: "Valentinov claims that RN journalists report what they hear from scientists. 'We trust scientists with advanced degrees,.....they [not us, journalists] are responsible for purity of science.' Beautiful words. But pages of RN have not been reporting science for a long time; only mysticism and devil worship. Your words, Mr. Valentinov, about confidence in people with scientific degrees do not match your

stubbornness in glorifying accomplishment of scientifically illiterate people."

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Two academicians, E. Alexandrov and V. Ginzburg supported Kruglyakov but their letter to the editor of RN was not printed. That letter is reprinted in the book. The academicians wrote that "the freedom of opinion in exact sciences is poorly understood by some people. According to Valentinov, a physicist is free to either believe or not to believe in the theory of relativity. ..... **One must recognize that the unlimited freedom of choice does not exist because the laws of nature are objective. They do not depend on preferences of scientists. Differences of opinion can only exist at the level of hypothesizing. Once confirmed, a hypothesis becomes reality and freedom of opinion is no longer allowed. Conflicting points of view, however, can be encountered because scientists are human; they can be either right or wrong.**"

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On page 177 Kruglyakov describes a tragic transformation of a gifted astrophysicist , N. Kozyrev [mentioned above by Valentinov], into a pseudoscientist. "**Before the war that scientists worked in Leningrad. In 1938 he was arrested and sent to a prison. According to witnesses, he noticed that after standing on the icy floor for a long time the feeling of coldness was replaced by the feeling of some warmth. Kozyrev speculated that time turns into energy. Then he started applying this idea to stars -- that might be the source of their energy. He could not possibly be familiar with the idea of nuclear origin of stellar energy. The hypothesis formulated in the prison was further developed after his return. It was a sad story. Those who knew him as a gifted physicist helped Kozyrev to defend a doctoral dissertation; he was the discoverer of lunar volcanism. But at the same time he was engaged in strange experiments. I know that his conclusions were not confirmed by two appointed teams of scientists. Kozyrev was a man broken by the depressive political system. Without being arrested he would probably have a brilliant scientific career. His bizarre ideas are now promoted by very strange people."**

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Those accused of being pseudoscientists in Russia often compare the accusers with those who, in Stalin's times, persecuted geneticists and cyberneticists. These people were also accused to be pseudoscientists; they were often arrested and sent to concentration camps. This issue is addressed in several places. On page 14, after quoting another academician, Kruglyakov writes: **"Let me remind you that the persecution [of Soviet biologists] was not initiated by scientific institutions. It is true, however, that some scientists did participate. Unfortunately scoundrels can be found everywhere; only the bravest were able to oppose terror openly. It is very different now; today everybody is free to defend his point of view. ..... Let me also remind you [page 186] another detail [page 186] from our horrible past. Physicists often helped persecuted geneticists. Igor Kurchatov, for example, had a large department of biology in his institute [working on the atomic bomb project]."**

On page 199 Kruglyakov writes: **"Do you remember destruction of genetics and cybernetics? ..... That destruction was organized in the Propaganda Department of the Central Committee of Communist Party. Don't you know that physicists were saving geneticists when they could? Let me tell you that only a miracle saved physics from a similar destruction. I had a conversation with I. Golovin, the deputy director of Kurchatov's Institute. He said that one day Beria [the secret police chief] suggested to Kurchatov that idealistic physics, such as quantum mechanics and theory of relativity, should perhaps be purged. Igor Vasilevich immediately replied that the atomic bomb design is based on these two theories. The purging is not desirable because the country needs the bomb. Beria informed Stalin about this conversation. This resulted in the cancellation of a session whose purpose was to discuss an attack on physics."** Stephen Speicher, from Phys-L, reminded us a story from George Gamov's autobiography. In that book ("My World Line," The Viking Press, 1970) Gamov describes his own encounter (in 1931)

with those who believed that teaching relativity and quantum physics is not good for Soviet ideology.

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94) Browsing the Internet

Ludwik Kowalski (August 13, 2003)
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1) **The web site at:**

http://dmoz.org/Science/Physics/Nuclear/Fusion/Cold_Fusion/

contains links to many sources for reading about cold fusion; they even have a link to my own web site on blake. The link to Scientific American Frontiers yielded three interesting articles, very critical of cold fusion. Like Huizenga, the authors are fighting the straw man. But this is likely to be typical among mainstream scientists. These articles were written in 1997.

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2) **Matti Pitkänen,** a theoretical physicist from Finland, at:

http://www.physics.helsinki.fi/~matpitka/coldf.html

has a short reviews the entire cold fusion field followed by a theoretical interpretation. He lists questions that any theory of cold fusion should be able to answer:

a) Why cold fusion is not a bulk phenomenon?
b) Why cold fusion of the light nuclei seems to occur only above the
critical value x=about .85 of D concentration?
c) How fusing nuclei are able to effectively circumvent the Coulomb wall?
d) Why gamma rays are not produced, why the flux of high energy
neutrons is so low and why the production of ^{4}He dominates
(also some tritium is produced)?
e) How nuclear transmutations are possible?

Then he presents his theory; I am not able to follow it. He often refers to the TGD model. I never heard of it. Here is a sample of text which means nothing to me, probably because I am not familiar with the concepts involved.

Trojan horse mechanism provides a manner to avoid Coulomb wall: since the two D nuclei feed their electric fluxes to different spacetime sheets, they do not see the Coulomb wall. The description of the reaction looks roughly like follows. Two D nuclei collide. The incoming D feeds its em gauge flux to the atomic spacetime sheet labeled by prime k=k_{em}>=131 (the p-adic prime p is p=about 2^k, k prime or power of prime). The target D is attached to Pd lattice and feeds its electric gauge flux to k>= 137 non-atomic spacetime sheet so that there is no Coulomb wall. In the collision deuterons stick together and the transformation of di-deuteron to ^4He occurs instantaneously in the time scale of em interaction since strong interactions are involved. Also reactions involving strong decay to ^3H +H and ^3He+n are possible. The *same* diagram involves also the photon exchange interaction of D nucleus with the electrons at the atomic spacetime sheet and electrons or Cooper pairs at the non-atomic spacetime sheet: it could be this mechanism which makes possible the transfer of surplus energy to the electronic degrees of freedom instead of gamma rays.

Trojan horse mechanism might work also at the level of chemistry making possible to circumvent electronic Coulomb wall and might be an

essential characteristic of the catalytic action. In separate context I have dubbed this mechanism as 'Houdini effect'. Biosystems are full of extremely effective catalysts, which suggests that the claimed biofusion [biofusion1,biofusion2] is based also on Trojan horse mechanism (perhaps it is someday possible to produce metabolic energy by biofusion or perhaps Nature has already discovered the trick!)..... The model can be summarized conveniently using quantum field theoretical language. ^4He is described as a scalar particle having standard minimal coupling defined be the covariant derivative D_{mu}= partial_{mu} +ieA_{mu} plus magnetic moment coupling of form mu*B to photon field. Gradient coupling does not allow the decay of the virtual state since this would lead to a nonconservation of angular momentum. Since transition magnetic moment is of order e/M(He) (e denotes electromagnetic coupling), magnetic decay is supressed by a factor of order E^2/M^2(He), where E is the energy of the gamma ray.

What does it mean "to feed electric fluxes?" What is a "spacetime sheet?" What is the "em gauge flux?" What is "p-adic prime?" My lack of understanding does not mean that the explanation is not valid. Perhaps the explanations make sense to theoreticians. Perhaps I should look at references at the end of the document. I suspect that this will not be enough; somebody else must interpret the theory for me. In the absence of help I will remain suspicious. **P.S.** A quick look at

http://blues.helsinki.fi/~matpitka/cbook.html

makes me even more suspicious.

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3) Rotational fields?

Kruglyakov wrote about claimed effects of axion (spin) fields. According to him such fields are likely to exist but they are too weak to be detected. Practical applications based on such fields are highly unrealistic. But fraudulent claims about devices generating rotational fields are common in Russia. Here is a translation of writing about rotational fields.

http://www.pmicro.kz/MISC/UFL/Almanach/AxionA.htm

There is even a suggestion that the field can be used a weapon to fight terrorism. Crazy? A trick to get money from the government?

******

4) An item on cold fusion from an encyclopedia:

The Columbia Encyclopedia, Sixth Edition. 2001. ". . . Research into the possibility of cold fusion, by Fleischmann and others, nonetheless continues, because of intriguing but inconclusive experimental results -- such as claims of the production of excess heat, helium, or tritium where heavy water reacts with metals -- and because of the desirability of producing relatively nonpolluting fusion energy in quantity at any temperature. Cold-fusion proponents believe that the fusion mechanism is different from that of "hot fusion" in that it encompasses some type of unusual nuclear reaction in the metal lattice involving deuterium and possibly other atoms. Several dozen models to explain the observed phenomena have been advanced, but none accounts for the full range of experimental observations."

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5) Charle Platt has a description of some leading US cold fusion experimental scientists still active in the field. It can be seen at:

http://www.wired.com/wired/archive/6.11/coldfusion.html

a) **Edmund Storms** is not an antiestablishment pseudoscientist pursuing a crackpot theory. For 34 years he was part of the establishment himself, employed at Los Alamos on projects such as a nuclear motor for space vehicles. Subsequently he testified before a congressional subcommittee considering the future of fusion. He believes you don't need millions of degrees or billions of dollars to fuse atomic nuclei and yield energy. "You can stimulate nuclear reactions at room temperature," he says, in his genial, matter-of-fact style. "I am absolutely certain that the phenomenon is real. It is quite extraordinary, and if it can be developed, it will have profound effects on society."

b) **George Miley**, who received the Edward Teller medal for innovative research in hot fusion and has edited *Fusion Technology* magazine for the American Nuclear Society for more than 15 years: "There's very strong evidence that low-energy nuclear reactions do occur. Numerous experiments have shown definitive results - as do my own."

c) **John Bockris**, formerly a distinguished professor in physical chemistry at Texas A&M University and a cofounder of the International Society for Electrochemistry: "Nuclear reactions can occur without high temperatures. Low-energy nuclear transformations can - and do - exist."

d) **Michael McKubre**, director of the Energy Research Center at SRI International: "I am absolutely certain there is unexplained heat, and the most likely explanation is that its origin is nuclear."

Credentials of **Martin Fleischmann** have already been described in my unit #4. Work of several scientists from the US Navy labs are described in my unit #51.

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95) A catalytic model?

Ludwik Kowalski (August 13, 2003)
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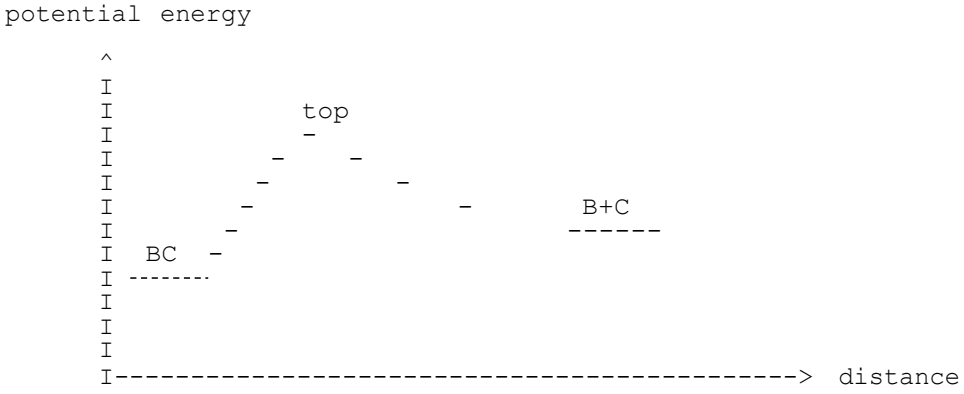
Catalysts are atoms or molecules affecting rates of reactions. These atoms or molecules may be independent, as in a gas, or be parts of structures, as in solids. In what follows no distinction is made between single atoms and groups of atoms. The term particle will refer to both a single atom or a group of atoms.

To simplify, let me assume that a catalyst is a gas A mixed with two reacting gases, B and C. In the absence of gas A one can imagine a reaction B+C --> BC. The number of B and C particles decreases in time while the number of BC particles increases. But at what rate does this happen? The answer depends on initial and final energies. If the energy of BC is higher than the energy of B+C then the reaction is called endothermic; such reaction will not take place spontaneously, even when a catalyst is present. The reaction rate is zero, unless energy is supplied.

On the other hand, if the energy of BC is lower than the energy of B+C then the reaction is exothermic and it can occur spontaneously. But the rate of an exothermic reaction depends not only on initial and final energies; it also depends on how potential energy changes with the distance between reacting particles. A typical situation is schematically represented below.



Note that although the potential energy of B+C is higher than that of BC it increases when the particles approach each other. We say that a potential barrier exists between the initial state (B+C) and the final state (BC). The height of the barrier (between the top and the B+C in the above figure) is called the activation energy. In general, the rate of an exothermic reaction depends on the activation energy; the rate is slow when the activation energy is high, and vice versa. Explosive reactions have very low activation energies. The role of a catalyst is to provide a path along which the barrier is lower. How can this happen?

In the presence of catalyst A our idealized reaction can proceed in steps, such as:

A+B --> AB followed by AB+C --> A+BC

Note that A does participate in the reaction; it disappears in step one and it reappears in step two. The net result is consumption of B and C, accumulation of BC, and a constant number of A particles. The rate of A+B-->BC, in the presence of a catalyst A, may increase when the activation energies in step one and two are lower than for the single step reaction (without a catalyst)..

What does all this have to do with cold fusion? I had to refresh my understanding of catalytic reactions after I noticed the word catalyst in several cold fusion papers. It is common knowledge that two D^+ ions repel each other electrically. But they attract each other strongly when the distance is very small. A cold fusion reaction, $D^+ + D^+ \rightarrow He^{++}$, was said (in 1989) to be theoretically impossible because its activation energy is of the order of one million of eV. That is why extremely high temperatures (tens of millions of K) are necessay to observe thermonuclear reactions in gases. But cold fusion seems to be happening on surfaces of some metals, such as Pd, Ti, Ni, etc.
Accordin to Storms, certain spots on such surfaces might act as catalysts facilitating fusion of deuterium ions.

The mechanisms of catalytic processes facilitating cold fusion are not clear. But one aspect is undeniable; the energy of BC (in this case He^{++}) is much lower than the energy B+C (in this case $D^+ + D^+$). The difference between B+C and BC, about 24 million eV, is much larger that the activation energy (about 1 million eV). One is tempted to think that the energy to “climb the hill” is somehow borrowed from the energy available in the “downhill fall.” This would be impossible in the independent single-step reactions (as in hot fusion) because released energy escapes in the form of 24 MeV photons. But in a system of millions of interacting atoms (a crystal catalyst) the borrowing may perhaps be possible. Instead of producing a 24 MeV photon, as in thermonuclear fusion, the energy released during each cold fusion event might go into the crystal and be subsequently used to promote another fusion. Conceptually this can be compared to a banking operation; borrowing on credit, paying back later and making profit. Many successful companies (exothermic events) must be involved to make this possible. Banks are social catalysts.

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96) Another negative result.

Ludwik Kowalski (August 13, 2003)
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It is easy to generate a negative result; postulate something that nobody expects to happen and conduct an experiment showing that this does not happen. In unit 31 I already described a negative attempt to observe excess heat. Here I will describe a negative attempt to observe induced gamma radioactivity. But first a little start. About a week ago a colleague, Robert Dorner, told me about a modern "plasma metal cutting tool" he purchased. A highly controlled electric arc is established (in air) between the nozzle of the device and a metallic plate. The unit operates at 220 volts and the current of about 20A. An electric arc in air reminded me off the experiments of Hal Fox, from Salt Lake City, except that the medium is air instead of water. It also reminded be of Karabut's experiment, in Russia, except that the medium is air instead of deuterium gas. Is it possible that gamma radioactive isotopes are produced when metals are cut by the arc? Our conversation resulted in an experiment we performed this afternoon in Bob's garage. No induced gamma radioactivity was observed with a simple Geiger counter.

What follows is a description of what we did. The metal cutting tool used was "Plasma 30," from Harbor Freight (www.harborfreight.com). It has a nozzle from which hot compressed air is forced to flow along the electric field lines (between the wire in the nuzzle and the plate). The exposed areas melted and ejected metallic pieces were collected in a stainless steel dish. These pieces were then placed in front of a Geiger tube whose thin window was protected with two layers of a common office paper. Here are the results;

```
1) For our counter the center of the plateau curve was near 1300 volts.
   That voltage was used in all subsequent measurements.
2) Background before the experiments --> 226 counts in 5 minutes
3) Aluminum sample --> 232 counts in 5 minutes
4) Copper sample --> 245 counts in 5 minutes
5) Steel sample --> 233 counts in 5 minutes
6) Bismuth sample --> 220 counts in 5 minutes
6) Carbon brush sample -->  458 counts in 10 minutes
7) Background after the experiment --> 218 counts in 5 minutes
```

I know that it is silly to report such results. I am posting this description to show what one can do in a student-oriented project.

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97) My Letter to the Editor of TPT.

Ludwik Kowalski (August 14, 2003)
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WHAT FOLLOWS WAS POSTED THIS MORNING ON THE PHYS-L SERVER. I AM ADDRESSING ABOUT 700 SUBSCRIBERS OF THAT DISCUSSION LIST. HOPEFULLY, SOME INTERESTING REPLIES WILL APPEAR ON THE LIST AND IN TPT.

My letter to the editor of The Physics Teacher was published in June of 2003. It is a comment on a student question about cold fusion. The editor, Karl Mamola, wrote to me: "This is the second summer that we have published letters on our website during the break in our publishing cycle, so that readers do not have to wait until September to read our Letters to the Editor." I would prefer the letter to be published on paper; it would then be seen by at least ten times more people. But the fact that the editor decided to publish my letter, considering the negative attitude of many, is significant.

Please, help to publicize the letter. The best way to do this is to reply with another letter. I do not think that The Physics Teacher is the right place to argue for or against the validity of various cold fusion claims. This should be done in scientific conferences and in peer reviewed papers. But TPT is a perfect place to let our establishment (AIP, NSF, DOE, Academy of Science, etc.) know that teachers and students would welcome a second formal evaluation of the field by appointed experts. Below is the content of my letter. The Internet reference is:

http://ojps.aip.org/journals/doc/PHTEAH-home/letters/jun2003.pdf

It is easy to submit a reply to a letter to the editor; e-mail your comments to Dr. Karl Mamola at: tpt@appstate.edu In doing this please indicate what you think about my appeal. Is a new evaluation of cold fusion claims desirable or should we stick to arguments found in the official 1989 evaluation? Also share examples of what students say or ask about cold fusion. Tell others how you personally deal with the subject.

ANSWERING QUESTIONS ON COLD FUSION

Let me begin by quoting a recent email message from a student.

"Dear Mr. Kowalski,
Help! My name is Maggie Johnson and I am a sophomore at Saratoga High School. In my chemistry class, I am doing a project on Cold Fusion. I was looking on the Internet for websites on Cold Fusion, and I came across links to your Cold Fusion items. I was wondering if you could give me some advice or information?"

How should a physics teacher answer questions about cold fusion? I am no longer comfortable saying that "cold fusion is voodoo-science." Can a nuclear reaction be triggered by a chemical process? The answer, based on what we know about nuclear phenomena, is negative. On the other hand many experiments seem to indicate the opposite. Some of these experiments have been described in refereed journals, others are available over the Internet. I am referring to papers published long after the first evaluation of "cold fusion" made in 1989 by a board of experts appointed by our Department of Energy. Their authoritative report (1)

was based on data available nine months after the initial announcement by Fleischmann and Pons. Many objections found in the report are still valid but some are at odds with new data. Accumulation of helium, for example, confirmed by several investigators, was not known when the report was released. How can progressive accumulation of helium be explained?

New findings about “cold fusion” phenomena are available to students over the Internet, for example, at www.lenr-canr.org . Many articles downloadable from that site were published by scientists associated with prestigious institutions. What should a physics teacher tell students about phenomena reported by these scientists? I have no clear answer to this question. That is why I think that a new authoritative evaluation of the “cold fusion” field, by a panel of competent investigators, is needed.

Ludwik Kowalski
Montclair State University,
Upper Montclair, NJ, 07043.

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98) A cold fusion demo at MIT

Ludwik Kowalski (August 14, 2003)
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The web site of the ICCF-10 (10th International Conference on cold Fusion) has a brief description of a demonstration of excess heat from an electrolytic cell. Professor John Dash and his students (Portland State University) will perform the demo in an MIT laboratory. Here is how this event is described on the conference web site:

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Demonstration

An excess heat experiment of John Dash is to be fielded in connection with ICCF10. This will be a technical demonstration to be done at MIT, in order to better inform the attendees about his work, and to respond to technical questions.

Technical description:

A control cell containing two platinum foil electrodes and an electrolyte containing sulfuric acid and light water is connected in series to an experimental cell containing a platinum foil anode, a palladium foil cathode, and an electrolyte containing sulfuric acid and heavy water. After passing current for several hours, the experimental cell is found to produce up to four watts of excess thermal power in comparison to the control cell. Excess heat is produced in about 75% of the experiments. Evidence of localized concentrations of silver is found on most of the palladium electrodes after electrolysis.

Three high school students and one graduate student will be on hand to discuss their results on these experiments.

Popular description:

Prof. John Dash and his students will show how they have investigated the excess heat effect associated with "cold fusion". An instrument to measure heat, called a calorimeter will be used to show how production of excess energy is measured. Using this apparatus, the students at Portland State University have generated this excess energy effect with frequent success. Perhaps unexpectedly, this energy is obtained from an electrolytic process involving only heavy water, palladium and platinum, with an acid used to conduct current through the solution.

A viewing of Dash's set up is planned for Monday night with the other events when the public is invited, in order to stimulate discussion.

Acknowledgment:

This research is performed in the low energy nuclear laboratory at Portland State University, Portland, OR, under the direction of Professor John Dash. It is funded by a grant from the Academy Of Applied Science and by a grant from the New York Community Trust. The New Energy Foundation provided transportation expenses to bring this demonstration team from Oregon.

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Additional information about student's cold fusion experiments, with pictures of students and equipment, can be found at:

http://www.lenr-canr.org/Experiments.htm#HighSchoolStudents

In the middle of the document one finds this general description:

"High School Students Do Cold Fusion

Every summer, high school students work with Prof. John Dash, of Portland State University, in cold fusion experiments, as part of the Apprenticeships in Science and Engineering program, which allocates high school students to summer internships all over Oregon and Southern Washington. In 2003, Corissa Lee and Shelsea Pedersen participated. They will be seniors next semester. They were assisted by Ben Zimmerman, who was an apprentice in 2002, and who will be attending the University of Chicago this Fall. Zimmerman describes the 2003 program:

'Our experiment is very rudimentary electrolysis of palladium in a D20 and Sulfuric Acid electrolyte, running under modest current (from 3-4 amps) with a non-reactive identical control cell for comparison of heat flow. So far, we've analyzed temperature readings and found that the cells used so far produce on average .5 watts, and as high as .9 watts as excess. Also, we've analyzed the palladium cathodes of similar experiments and found anywhere from 2 to 20% of unaccounted for silver with an SEM after electrolysis from cathodes that produced excess heat.' "

At the bottom of the document one has a link to a poster summarizing a research project of Jon Warner, J. Dash, and S. Frantz. It is actually a poster, entitled, ELECTROLYSIS OF D_2O WITH TITANIUM CATHODES: ENHANCEMENT OF EXCESS HEAT AND FURTHER EVIDENCE OF POSSIBLE TRANSMUTATION. That poster was shown at the 9th International Conference on Cold Fision (China, 2002).

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99) Shortening of a half-life?

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In item #98 I wrote about cold fusion experiments performed by students at Portland State University. Inspired by their teacher, John Dash, they studied generation of excess heat in an electrolytic cell. I will be able to see their demo at the 10th International Conference on Cold Fusion (ICCF-10). Will I be convinced that the excess heat is real? Or that it is not due to a chemical reaction of some kind? Too bad that my familiarity with electrochemistry is so limited. I would be in a better position criticize a nuclear effect demo because I am a nuclear physicist. Unfortunately, there will be no nuclear cold fusion demos at the conference, only papers describing various nuclear effects. John Dash's paper, for example, is entitled "effects of hydrogen loading by aqueous electrolysis on radioactivity of uranium." Is he going to say that the electrolytic loading has the same effect as loading it in a gas discharge tube? The gas discharge data have been published at the previous cold fusion conference (ICCF-9, China, 2002).

John's paper, presented in China, can be downloaded from the library at http://www.lenr-canr.org . Here is the abstract from that paper: "Uranium foils were attached to the cathode of a glow discharge apparatus. A plasma of either hydrogen or deuterium ions was used to bombard the uranium. The rates of alpha, beta, and gamma radiation emissions were significantly greater for the bombarded uranium than for the original material." For alpha particles, for example, the rate of decay, after the processing, was four times higher than before processing. Uranium foils were used as cathodes in a glow discharge tube (5 tor, 500 volts, 5 mA). The processing lasted about 500 hours.

How can a nuclear physicist accept such an allegation without actually performing an experiment? But the apparatus used was similar to that of Karabut, who also observed several unbelievable nuclear effects. The first step should be to formulate the so-called "null hypothesis," as in statistics, and to do everything possible to validate it. Failures to validate the null hypothesis can then be used as the basis for accepting, still tentatively, a highly unusual claim. I met John in Albuquerque last Fall and I read his China paper. Expressing my disbelief I said that I would be willing to spend several days in Portland, at my own expense, and participate in experiments. My offer was not accepted; I suspect this had something to do with a pending patent application, or with John's desire not to share potential rewards for a "revolutionary discovery." I can understand such motives. What would I do in John's situation?

1) Not being a nuclear physicist I would welcome the offer of somebody who is. The patent aspect could be solved by asking that person (Ludwik) to sign a legal document, as they do in national labs. Another precondition could have been an understanding that the person will not be a coauthor of a publication resulting from the study.

2) My first concern would be the possibility that the discharge apparatus, or the gas used, was radioactively contaminated in the past. I suppose that John has checked for the absence of contamination, for example, by placing control foils into the apparatus (at zero volts). Without such a test one might suspect that contamination was responsible for the observed effect. The radioactivity of natural uranium is weak.

3) My next concern would be the possibility that a thin layer of some covering material was removed during the processing. The removal of a "dead layer" from a thick uranium target could possibly lead to a higher alpha emission rate after processing. The same parasitic effect could possibly result from a change in the smoothness of the uranium foil. According to Figure 8, erosion of the surface of uranium did take place during the processing.

4) Instead of using a large area alpha probe (Ludlum 43-5) I would use a small Si detector connected to a multichannel analyzer. A significant shift toward higher energies, resulting from processing, could be a warning signal that a change

in the emission rate might be an artifact. I do not think that it was necessary to use a counter whose window area was about 100 times larger that the area of the alpha source. The signal to noise ratio would be more favorable for a smaller detector.

5) Here is my main question. Why is a change in the alpha decay rate much larger than changes in the beta or gamma decay rates? It seems to me that all three counting rate changes (alpha, beta and gamma) would be identical if uranium nuclei started to transform more rapidly. Why was this question not addressed in the paper? Will it be addressed in John's ICCF-10 paper?

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100) Physics Today rejected my letter

Ludwik Kowalski (August 14, 2003)
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As I wrote yesterday (see item #97), my Letter to the Editor has been published by The Physics Teacher. My attempt to publish a Letter to the Editor of Physics Today, however, was not successful. Let me document this failure. Here is what I wrote:

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To the Editor

The January 2003 issue of Physics Today had an interesting item entitled "New APS Ethics Guidelines Address Research, Misconduct and Professional Responsibilities." The author, Jim Dawson, summarized main points of the **new ethics guidelines** recently adopted by the American Physical Society (APS) panel on public affairs. I was particularly interested in this segment:

"You should hang on to your data, you should respond to inquiries from other scientists, and you should be responsible as a referee. . . The new guidelines, approved on 10 November, come in several parts. A policy statement on how to handle allegations of research misconduct defines misconduct as 'fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results. ..... ' Such behavior is termed an 'egregious departure from the expected norms of scientific conduct that can lead other scientists along fruitless paths.' It also 'diminishes the vital trust that scientists have in each other' and undermines public confidence in science. The statement goes on to say, 'It is imperative ..... that the institutions responsible for funding and performance of scientific research, as well as the relevant professional societies, take appropriate steps to discourage such conduct' ..... "

I welcome the new guidelines but I would like to examine them it in a slightly different context than emphasized in the article. How can a physics teacher make sense of "cold fusion?" Is there any evidence that this field, in terms of what has been done in the last ten years, is a "departure from the expected norms of scientific conduct" or that it "can lead other scientists along fruitless paths?" I think the 1989 ERAB report was correct in that no convincing evidence existed to support the premature claim of Fleischmann and Pons about a new source of useful energy.

But the situation has changed in the last ten years. Scientists conducting research in this area no longer claim that demonstrations of excess heat are easy (as initially announced) or that the underlying mechanism is simply a fusion of two nuclei, as in hot plasma. But they "hang on to data" indicating that something significant was discovered in 1989 and that it should be studied, as recommended in the ERAB report. I see no evidence that the data are "fabricated." Is it true that editors of many peer reviewed journals automatically reject manuscripts dealing with "cold fusion?" Is it true that "institutions responsible for the funding and performance of scientific research," such as DOE and NSF, automatically reject research proposals dealing with "cold fusion?" Is it true that young researchers avoid this field for fear of endangering their professional careers?

I have heard such allegations from several "cold fusion" scientists. They claim that the entire field has been blacklisted in the US. If this is true, then, in my opinion, the situation should be reviewed in light of new ethical guidelines. Those guilty of falsifications should be exposed as pseudo-scientists while those who made "honest errors" should be

criticized, as in any other field of science. And those whose claims are accepted as valid should be rewarded (in the form of published papers and financial support for research) as in any other area of science.

As a physics teacher I am confused by the situation. What should we tell students when they ask about the discovery of Fleischmann and Pons? Most teachers have no time and no means to validate claims made in the area of "cold fusion," and need guidance. An objective summary of what has been done in that field, in the last ten years, would help us to describe it correctly. The issue is not only scientific; it is a topic of general interest. Most educated people know about the "cold fusion episode" and opinions about it are divided. Some say it was "a fiasco" while others say it was an "important discovery." How should teachers address this topic in the context of "public affairs between science and society," or in the context of discussing "institutional support for new ideas and innovations?"

****

After waiting several months I sent an e-mail message asking about the status of my letter. On Thursday, June 12, 2003, I receive a reply from Marty Hanna, Letters Editor at Physics Today. He wrote: So far, I have a split decision on the possible publication of your letter. I expect soon to have a tie-breaking input from a third reviewer. I will let you know as soon as I have a firm decision. Thank you for your patience."

My immediate reply was brief. "Dear Dr. Hanna: Thanks for giving my Letter to the Editor a chance. In reading it again I see a typing error which may not be noticed. Please correct it before publishing. The ERAD acronym should be replaced by ERAB; it stands for the famous 1989 Energy Research Advisory Board. By the way, I hope that a similar board of experts will be appointed to examine new cold fusion data. My "confusion about cold fusion" is probably typical; many physicists need help to interpret information widely available in that field."

On Thursday, July 3 Dr. Hanna wrote " We have completed our review of your letter commenting on the APS ethics guidelines story in our January 2003 issue. Our decision, after some valuable discussion, is not to publish your letter. Thank you for writing and for your interest in Physics Today." Unhappy about this I wrote "I would very much appreciate if you could send me the reports of the referees evaluating my letter to the editor. I wish you nice holidays." The immediate reply was " Thank you for your inquiry. Please let me explain. I know that scientists who submit articles to peer-reviewed scientific journals expect reviewers to give them a critique of their letters. Physics Today is not, in the strictest sense, a peer-reviewed scientific journal; it is, instead, **a special-interest magazine** for physicists. Generally, my reviewers are staff writers and editors (all physicists) who may give me little more than 'thumbs up' or 'thumbs down' on a letter. As a rule, we do not give out the specific comments of the reviewers, because we consider them to be internal business. Thank you for your inquiry. I hope my explanation has helped.

The input from three qualified referees would be very useful to me. Whose "special-interest is being protected by not showing me what they had to say? I am a member of APS and I expected a better treatment from my journal. It is ironic that my letter was triggered by an article about ethical standards in science.

Let me remind you that the rejection of my cold fusion note, by TPT, has already been described in item #49. Even recognized experts encountered difficulties in trying to published cold fusion articles, as illustrated in items #33 and #9. This is not a healthy situation; here is how it was described by E. Storms: "Serious scientists rejected 'cold fusion' in the past for good reason. These reasons no longer apply. If science cannot correct a past rejection, then what good is the scientific method?" Physics Today should promote exchanges of information among recognized physicists from different disciplines. It is an ideal place to ask a question formulated in my letter. Why did Dr. Hanna reject the letter after one of the referees recommended it? Which special interest is he protecting by not publishing the letter? I am probably not the only physics teacher confused by the existing situation in the area of cold fusion. What is wrong with asking for another evaluation of that field by an appointed panel?

P.S.
After posting the above I realized that part of it is a repetition of what has aleady been shown in item #88. I am sorry for this.

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101) Excess heat again

Ludwik Kowalski (August 15, 2003)
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I am reading the article the John Bockris published in Journal of New Energy (vol 4, no 2. 1999, p. 40). It explains how the so-called "excess heat" was measured by this famous electrochemist. I do not know why, in some places, Bockris confuses energy with potential; I am sure he knows the difference between these two concepts. He writes, for example, "this energy is represented by the electrochemical potential applied to the cell." In another place he writes that "I^2*R heat is included in E, the total potential applied to the cell." I would not use E for the difference potential and I would not confuse E with rate of using electric energy. Sloppy formulations tend to discredit a scientists. But I will ignore them.

Suppose that the difference of potential, E, is applied to a cell in which water is electrolyzed, and that the current is I. The input power is E*I but the heat generation rate is (E-1.54)*I because 1.54*I is the rate at which energy is used to produce bubbles of oxygen and hydrogen. The excess heat generation rate is a difference between the heating rate measured, P, and the (E-1.54)*I, provided no recombination of hydrogen and oxygen takes place inside the cell. In Figure 5, for example, the excess heat was zero during the first 60 hours, was slowly increasing up to 40 mW during the next 50 hours and remained constant (40 mW) in subsequent 50 hours. I am not impressed by 40 mW but I believed that it can be measured very accurately.

How do they measure heat generation rates, P.? The cell is surrounded by a constant temperature bath (plus or minus 0.1 degrees C). But the measured temperature inside the cell, after the equilibrium is reached, exceeds the temperature of the bath by dT. It turns out that a relation exists between the true heating rate, P, and dT. The relation is linear as long as dT is not larger than about 7 C. The coefficient of proportionality between P and dT, for example 0.8 W/C, is determined by calibrating the cell with ohmic resistors. Knowing the coefficient k one can always determine the true heating rate as P=k*dT. The excess heating rate, H, as indicated above, is given by:

$$H=k*dT-(E-1.54)*I$$

where H is watts, dT in C, E is in volts and I is in amperes. This approach, used by Fleischmann and Pons, was also employed by Bockris and his coworkers. Different calorimetric approaches were later developed by other scientists. The way in which H was measured by Karabut, for example, was described in unit #13.

The remaining part of the paper deals with evidence for nuclear effects: production of tritium, production of helium and transmutations. But that evidence is not very convincing. The paper has a lot interesting information on people, and on their motivation in early 1990s. But it offers no description of reproducible observations. Even suspected cases of fraud are mentioned.

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Fleischmann, Jones and students

Ludwik Kowalski (August 29, 2003)
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The 10th International Conference on Cold Fusion (ICCF10) ended yesterday (8/29/2003). The next conference, ICCF1, will take place next year in Marseilles, France. The school year starts next Tuesday and I will have much less time to devote to cold fusion. This is probably my last CF item for at least a month or two. Let me share some details.

Both Martin Fleischmann and Steven Jones were present and gave interesting reports at the conference. I knew that Fleischmann is still active but current cold fusion work of Jones was new to me. As expected, Fleischmann's presentation was mostly about electrochemical details while Jones' presentation was about a recent experiment in which nuclear particles (emitted from deuterided metal foils) were detected by using traditional nuclear counters. We talked about his very convincing nuclear signature and he invited me to visit the lab and possibly work with him. Too bad this did not happen when I was desperately trying to be invited somewhere (it was a sabbatical year) to participate in a cold fusion experiment. I will certainly visit Jones' lab in January. He is also preoccupied with tritium in some volcanic gases.

Another thing I want to describe is a student demonstration of a cold fusion experiment. Three high school students and one graduate student, guided by professor John Dash, set up a demo for the conference participants. Two electrolytic cells were connected in series with a power supply delivering a constant current of 3.5 amperes. Cell #1, labeled "control," had two platinum electrodes and used an ordinary water electrolyte. Cell #2, labeled "sample," had a palladium cathode and a platinum anode. It used a heavy water electrolyte. The DOP on the control cell was 3.03 volts while the DOP on the sample cell was 2.80 volts. In other words the electric energy was delivered to cell #1 at the rate of 10.6 W while to cell #2 it was delivered at the rate of 9.8 W. The two cells were geometrically identical; they also had identical catalysts recombining gases (hydrogen and oxygen).

On that basis one would expect cell #2 to be warmer than cell #1 (10.6 W versus 9.8 W) . But the equilibrium temperatures measured did not confirm this expectation. The temperature of cell #1 turned out to be 89.1 C while the temperature of cell #2 was 89.8 C. The difference of temperature, 0.7 degree, could be interpreted as an indication that some kind of additional energy was converted into heat in cell #2. Quite an impressive demo for a summer project for three high school students at Oregon State University! I am not saying that the nuclear origin of excess heat has been demonstrated in this simple experiment. As far as I know it was the first student-conducted experiment trying to demonstrate the reality of cold fusion. Sensing an opportunity to immortalize a historical event I grabbed a camcorder and filmed everything. I also interviewed students and their guests.

It would be nice to edit the footage and produce a 30-minute film. Unfortunately I will be too busy to do this right now. Can somebody who has time and desire help me with this? If so please contact me in private; it can be a nice item for an AAPT conference presentation. John Dash gave me a signed permission to use the recorded footage. Fleischmann and Jones also gave me signed permissions to publish photos and footage recording their conference presentations. My own presentations at the conference (a poster and a 25-minute talk) were also filmed but are much less important in the context ot the ongoing cold fusion debate. I showed the "Letter to the Editor, " published in The Physics Teacher (June, 2003),

http://ojps.aip.org/journals/doc/PHTEAH-home/letters/jun2003.pdf.

and I asked questions about means of communication between the cold fusion community and other scientists. The

conference was a very important step in my attempts to form a definite opinion about reality of what has unfortunately named “cold fusion.” I knew several scientists by names; now I know them personally, more or less. Let me tell you that most of them are real scientists. The main critic of cold fusion, Robert Park (from AIP), was invited to the conference. But he did not come, presumably due to a timing conflict. He missed a great opportunity to learn about what is going on in several countries. He also missed an opportunity to argue about cold fusion with those who work in the field. This would be much more challenging then publishing “voodoo science” declarations. Robert Park worked on surface physics phenomena before becoming a spokesman for AIP. His absence deprived me of an opportunity to hear an open cold fusion debate between a knowledgeable opponent and knowledgeable proponents.

Post Scriptum:
A subscriber to Phys-L, an Internet discussion list for physics teachers, read and commented on the above description. Replying to his message I wrote:

“ . . . It would be foolish to use these data as an indication that something nuclear is going on. My emphasis was on the fact that a teacher, John Dash, was brave enough to give students a project based on a controversial subject. No attempts were made, as far as I know, to argue that excess heat can not possibly be attributed to chemical reactions, etc. The assumed ‘steady state’ situation can also be questioned. No calorimeter was used in the experiment to measure the amount of thermal energy.

The nature of the apparent excess of thermal energy should be discussed but not necessarily at the level at which students learn about differences between volts, watts and amperes. At a more advanced level I would ask students to calibrate the cell, that is to establish a relation (probably not linear) between the excess temperature and excess thermal power. The next questions would be:

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a) how can the observed temperature difference of 0.7 degrees C be explained?
b) what else can we do with available tools?
c) what additional tools would help us?
d) etc. etc.
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Personally I am very critical of calorimetric data when heating rates are very small; nuclear signatures reported at the conference are much more convincing. They show that something very unusual is taking place in some cold fusion experiments. But that is a different subject.”

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104) Another case of new alchemy

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New version (with references + appendix) of this draft now exists as item #123 in this list. Read it instead of this item.

Is it possible that data presented by Japanese researchers were simply invented to deceive those who came to the conference? Yes, this is possible. But transmutations of heavy elements in "cold fusion" experiments have been reported by at least ten different teams of researchers. The authors are Ph.D. scientists and recognized authorities in their fields. An international conspiracy of some kind? Yes, this is also possible. But accusations of that kind must be supported by some kind of evidence. As far as I know, nobody accused top cold fusion researchers of fabricating data in order to deceive. By the way, I think that John Bokris, a highly distinguished electrochemist from Texas A&M, was the first to report on nuclear transmutations of trans-helium elements. This was ten years ago.

I am also working on item #124 (A more penetrating review of one of S. Jones' articles). A less penetrating review of his three articles presented at the 10th International Cold Fusion Conference has already been posted as item #113

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105) New alchemy from Japan

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Another conference participant, Dr. Talbot Chubb, was also impressed by Iwamura's findings (see my unit #104). I did not know that the research started several years ago and that the results were confirmed by a team from Osaka University. What follows is Chubb's description, as posted at the www.lenr-canr.org site.

"Nuclear reactions replicated at Osaka University

The ICCF10 international gathering of 'low energy nuclear reaction (LENR)' scientists has just been completed in Cambridge, Massachusetts. The results presented at this meeting seem destined to affect the course of solid state and nuclear science. Probably the most important of the results were those concerned with a unique form of nuclear transmutation reported a year ago by Iwamura *et al*. of Mitsubishi Heavy Industries. (Click here for their papers from ICCF-7, ICCF-9 and a link to their paper in the Japanese Journal of Applied Physics.) The Mitsubishi transmutations occur on a deuterided metal substrate. The transmutations convert carefully deposited surface cesium atoms into the rare earth praseodymium. These transmutation reactions have now been duplicated by Osaka University scientists. They have repeated the transmutations several times. The Osaka praseodymium product has been verified by neutron activation analysis (NAA) at the Japan Atomic Energy Institute.

Meanwhile, Mitsubishi Heavy Industries has continued to make progress. The Mitsubishi scientists have further confirmed the identification of the praseodymium product, using a number of independent diagnostic techniques. They have shown that the transmutation occurs both with chemically deposited and ion-implanted cesium atoms. Surface profiling studies have been carried out and have located where the reaction occurs by measured the depth distribution of cesium loss and praseodymium creation. The results show that the nuclear reaction is a surface or near surface reaction on the substrate metal. Precise chemical analyses of the bulk metal substrate have shown that the praseodymium nuclear product is much too plentiful to be due to impurity migration from the bulk.

In the Mitsubishi process a nuclearly active form of deuterium is created from a flowing stream of deuterium atoms inside a metal. The flowing stream is forced to encounter and overcome specially designed internal diffusion barriers. A new form of active deuterium is created during this inhibited diffusion process. The active deuterium is able to spread out and interact with the nuclei of target atoms despite the deuteron charge. The nuclear reactions are of a specific type. They are deuteron addition reactions in which 8 deuterons (or 2 alpha particles) are absorbed by a target nucleus. The cesium conversion reactions can be viewed as the inverse of alpha-particle radioactive decays, which were discovered and characterized by Becqueral, Curie, and Rutherford near the end of the 19th century. The cesium reaction has been called a 2-alpha addition reaction. The full range of addition reactions that can occur using nuclear active deuterium has yet to be determined. The Mitsubishi work identified both: (1) a reproducible method for creating the active deuterium, and (2) a clear diagnostic method that quantifies its presence.

The Osaka and Mitsubishi studies provide solid evidence that deuteron or alpha-addition nuclear reactions can be made to reproducibly occur on solid metal at a temperature below that of boiling water. The new results were reported by Iwamura of Mitsubishi Heavy Industries and Higashiyama of the Nuclear Engineering Department of Osaka University. The Osaka low energy nuclear work is lead by Akito Takahashi. The original Mitsubishi discoveries have been published in English in the internationally respected Japan Journal of Applied Physics (Iwamura et al., 2002), and are available on the web.

The new discoveries remind one of the beginnings of neutron-capture physics. In 1932 Chadwick discovered the neutron. His neutrons were produced by the impact of alpha particles on beryllium. Within a few years a large number of previously non-existing types of nuclei were synthesized by exposure of various target elements to neutron irradiation. During these neutron-absorption studies uranium fission was discovered and the new element plutonium was synthesized. By the end of 1942 the first controlled nuclear reactor was already in operation. A nuclear power plant was generating electricity in 1955.

It seems likely that the larger international community will build on the Japanese work. Further attempts to replicate the Mitsubishi protocol are in progress. Hubler at the U.S. Naval Research Lab announced plans for replication testing in consultation with the Mitsubishi scientists. It is to be hoped that the world community will quickly join in an effort to better understand the new active deuterium matter form, its reaction physics, and its usefulness in generating safe nuclear energy heat."

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106) Why is Ramsey silent today?

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Here is a fragment from a message received this morning from Kjeld Engvild. He is a scientist from Risoe National Laboratory in Denmark. Kjeld wrote: ". . . I have been following the CF discussions for many years now (mainly as a lurker) and I still don't know if (although I hope) the effect will turn out to be genuine. The field seems to have been stuck for some time, but the latest results from Mitsubishi may turn up something, although the results seem almost incredible, and certainly not explainable within the present theoretical framework. ..... "

Replying to the above, I wrote: "..... I was also very impressed by the results from Mitsubishi. I even wrote two items about this work on my cold fusion web site (#104 and #105). But like so many other cold fusion experiments, this one is far above what can be done by a typical physics teacher, even in a university. How come that so many highly qualified scientists (probably more that in the Manhattan project) could not offer a single "nuclear signature" experiment for people like me in 13 years? Even a 75%
reproducible demo would be convincing..... " Unless I have such demo I will remain neither a believer nor a denier of cold fusion. The only thing I am certain is that the field is ready for another formal evaluation.

Several speakers at the 10th International Conference on Cold Fusion, which I attended last week, also expressed the desire for a new evaluation. One of them, Michael Staker, said that several years ago Norman Ramsey, the Nobel prize laureate, expressed a similar wish (in a private conversation). This is very significant because Ramsey was one of those who signed the ERAB report. According to Eugene Mallowe, Ramsey was the author of that pair of report sentences: "**However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system.**" This was added to the report to persuade Ramsey not to resign from the panel headed by Huizenga. But what prevents Ramsey, who is a chemist, from publicly requesting a new evaluation of the cold fusion claims? His appeal, for example, in a letter to the editor of Physics Today, could not be ignored. I think that it would lead to a new national study of cold fusion.

Why am i optimistic that that the puzzle of cold fusion will eventually be solved? Because the "scientific process is self-corrective. This unique attribute sets science apart from most other activities. The scientific process may on some occasions move slowly, sometimes even along a circuitous path. The significant characteristic of the scientific method, however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error." Who wrote this? John Huizenga, the author of the highly negative ERAB report.

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107) Biological manifestations of CF ?

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Let me continue sharing what I learned at the 10th International Conference on Cold Fusion (Boston, August 2003). In his review of cold fusion Edmund Storms mentioned that some people reported transmutation of elements by bacteria. The classic book in this area is that of C. Louis Kervarn (ISBN 0-916508-47-1). The title is "Biological Transmutations." The book, costing $18,50, can be ordered by calling 1-800-867-7258 (from Happiness Press in Magalia, California). According to Kervarn (1901-1983) bacteria can speed up the decay of radioactive ^{203}Hg (whose half-life is 46 days). He refers to an experiment of L. Magos et al. in England.

In that experiment radioactive $^{202}HgCl_2$ was placed into the kidney of a rat. The radioactivity was measured with a Geiger counter. During the first 32 hours the decay curve was exponential as expected. But then the counts per minute started to go down with respect to the exponential curve. Sixteen hours later (at t=48 hrs) the counting rate was 50% lower that along the initial exponential curve. The experiment was performed at room temperature and evaporation of mercury was negligible. According to authors the first 32 hours correspond to the growth of bacterial colonies. Then bacteria (pseudomonas) start transforming radioactive mercury into something that is not radioactive.

How come that this experiment, performed in 1970s did not become famous? Famous for demonstrating alchemy or famous for reporting results which could not be confirmed by other scientists. After all the announcement of Magos et al. was as unexpected as the announcement of Fleischmann and Pons. Why didn't it trigger a debate? Does this support the idea that motivation of the cold fusion uproar, in 1989, was political rather than scientific? I do not know how to answer these questions. And I do not know if a less radical explanation could not be found. One may suspect, for example, that bacteria, for some reason, contribute to the redistribution of ^{202}Hg within the rat kidney. This would change the counting geometry and explain the abnormal decrease of the counting rate. (The radioactivity of that isotope consists of gamma rays of 0.28 MeV and of beta rays of 0.21 MeV.)

At the same conference (ICCF10 in Cambridge, August 2003) I met a Ukrainian scientist from Kiev. He is also deeply involved in transmutations of chemical elements caused by bacteria. Vladimir Vysotsii and Alla Kornilova are the authors of a recent book "Nuclear Fusion and Transmutation of Isotopes in Biological Systems." (Mir, Moscow, 2003, ISBN 5-03-003647-4). The book has two parts, the original Russian text and its English translation. Vladimir gave me a dedicated copy of this interesting book.

It will take some time before I grasp everything. Here is an example of transformation of Mn into Fe. The beauty of this experiment is in its ability to detect ^{57}Fe only (via Mossbauer spectroscopy). Bacteria, whose metabolism is based on iron, but not on manganese, were grown in heavy water containing $MnSO_4$. The amount of ^{57}Fe in water, before introducing bacteria into it, was minimized; it was too small to measure. This was not surprising; natural iron has only 2% of that isotope. But 72 hours later, after iron-hungry bacteria were allowed to grow in the ironless environment, the situation was very different. The amount of ^{57}Fe was much higher than the threshold of detection. Several control experiments, for example, using ordinary water under identical conditions, lead to a conclusion that the progressive accumulation of ^{57}Fe was due to the $^{55}Mn + ^{2}D \rightarrow ^{57}Fe$ nuclear reaction.

Yes, I know that the coulomb barrier of several MeV should prevent such reactions. The coulomb barrier theory has been validated in experiments involving so-called hot plasma. But it would be a mistake, I think, to reject experimental

data on the basis of a disagreement with that theory. Being applicable in the hot plasma environment it may turn out to insufficient to understand some unknown processes taken place in crystals or in bacteria. The only way to justify a rejection of experimental data is to show an error in a procedure used to validate them. Experimental findings confirmed by several investigators should be taken very seriously. How else could limitations of theories be explored? A theory attempting to overcome the coulomb barrier objections to cold fusion has been described at the conference by an MIT professor, Peter Hagelstein.

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108) Another experiment for students ?

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1) The cold fusion experiment for students, described in item 103, focuses on excess heat. Let me describe an experiment for students which some of you might be able to perform. It was inspired by the talk of Dr. Richard Oriani at the 10th International Conference on Cold Fusion, by a conversation with Dr. John Fisher (also at ICCF10), and by an email message received from Oriani after the conference (see below). This experiment is designed to reveal alpha particles resulting from nuclear reactions presumably taking place during the electrolysis. An ordinary tap water, rather than expensive heavy water, was used to prepare the electrolyte.

2) Alpha particles were detected in plastic chips similar to nuclear emulsions (but much less expensive and much easier to use). The detector is called CR-39; it is a sheet of transparent plastic. Alpha particles, for example, from radon, create latent tracks which can be made visible (through a microscope) by developing the chips in hot KOH. The CR-39 detectors are commercially available from several suppliers. One place to obtain them is:

Track Analysis Systems Ltd
H H Wills Physics Lab
Tyndall Avenue
Bristol
BS8 1TL
U.K.
Tel (0)117 9260353
Fax (0)117 9251723
web www.tasl.co.uk
e-mail apf@tasl.co.uk

Here is what this supplier wrote to me in January 2003. “Thank you for your inquiry. We do not have any agents in the US -- we sell directly to customers. The prices depend entirely on what you require. If you can say the sizes and quantities you are interested in, I will be pleased to make a specific quote. For example: pieces 25x25 mm, with an engraved (incrementing) number, cost 1.75 US dollars each. We can quote for sizes from 10x10 mm up to 275x280 mm. Our material is carefully produced to high quality standards, and each batch of production is tested for background and track response. For the US, we have a minimum order quantity of 100 US dollars.”

3) To gain experience I exposed a CR-39 chip to a ^{241}Am source of alpha particle (removed from a fire alarm detector). After an exposure of 30 seconds the chip was kept in hot KOH (70 degrees C) for six hours. Then it was washed in water and placed under an ordinary microscope (magnification 40 or 100). Pits due to alpha particles were visible in the exposed area while the surrounding areas had much lower track densities. These detectors are often used to measure concentrations of radon in different locations. Dr. Oriani placed the chips into the electrolyte of a cold fusion cell and observed an excessive number of tracks. **Here is what I wrote to him after the conference:** “I am a physics teacher who is trying to develop a convincing cold fusion demo for students. Your CR-39 experiment is a good candidate. I talked about this with John Fisher and he suggested that I contact you about details.

1) What was the concentration of Li_2SO_4 in water?

2) How many volts and how many amps?
3) How large were the areas of your Ni and Pt electrodes?

Suppose I do an experiment under about the same conditions as you did and that no extraordinary event (which you described) occurs. I want to bring the number of alpha pits to at least 10 per field of view (magnification 100).

4) How long should a CR-39 be kept in the tube (days, weeks, months, ...) ?

5) Is it better to keep CR-39 in the liquid or above the liquid?

Here is Dr. Oriani's reply: " ... It's a pleasure to answer your questions because I am delighted that you will try to convince your students that so-called cold fusion is a real phenomenon. Answering your specific questions:

1) The concentration that I used is 2.3 g of Li_2SO_4 in 100 cc of H_2O, but you can deviate from that to some extent.

2) I usually electrolyzed for 3 days starting at about 100 mA, the next day at about 200 mA, and the third day at about 300 mA, letting the voltage be what it needs to be to achieve those currents.

3) The cathodes were bounded by an O-ring whose diameter is 1.4 cm, and the anode is a flat Pt spiral of about 1 cm diameter. The CR-39 detector chips are suspended within the electrolyte preferably above the anode, or in the gas in a heated section of the electrolysis cell kept at about 65 C and separated from the surface of the electrolyte by a nickel disc of diameter slightly less than the id of the electrolysis cell. Its purpose is to mitigate the impact of electrolyte mist upon the chips.

4) The detector chips should be in the cell for the entire duration of the electrolysis. I suggest that you place chips both in the liquid and above the nickel disc in the vapor.

5) You must remember that radon in the air and cosmic rays will produce tracks on the chips during handling and photography or viewing under the microscope. Hence you must examine the chips by etching in 6.5 N KOH at 65 C for about 10 hours and recording the tracks visible before electrolysis, then do the electrolysis and repeat the etching and examination of the chips at exactly the same areas that you examined and recorded prior to electrolysis. [I suppose the photos taken before the electrolysis were compared with photos taken after the electrolysis.]

6) Of course you also need to carry out controls, doing exactly the same operations to the control chips as for those exposed in the cell, but the control chips are not exposed to electrolysis. For further details I refer you to our paper published in Jap. J. Appl. Phys. [in 2002] and available at www.lenr-canr.org I hope that you succeed. Feel free to write again if something is not clear."

In the correspondence that followed Dr. Oriani provided me with data shown below. Referring to the table he wrote: "The numbers are new tracks per cm^2 of chip area. That is, I count the tracks that appear after electrolysis and the second etch and subtract from that number the tracks that appeared after the first etch before electrolysis and divide that difference by the area on which the tracks were counted. [The results are shown in column 2]. Exactly the same area was counted after each etching. The same procedure was applied to the control chips; they were immersed in solution of the same concentration as were the actives, in fact from the same batch, the sole difference being that they were not exposed to electrolysis. [The results are shown in column 1]. If you want the absolute number of new tracks, multiply the tabulated numbers by the area of a chip, roughly 0.64 cm^2. Note that the data entries were artificially organized in order of decreasing magnitude. All experiments were done with solution from one large batch."

Oriani and Fisher
published in July 2002

Controls Actives
==========

541 3760
260 2756
260 2375
221 1733
204 1138
195 962
177 962
165 897
143 757
143 676
118 578
108 543
95 318
95 315
75 260
59 225

As indicated by Oriani, the lines in the above table have been sorted according to new track densities; a monotonic decrease of densities in 16 consecutive experiments would be very unlikely. The second column shows that a single 3-days exposure produced very different new track densities when experiments were repeated. Commenting on this Dr. Oriani wrote: "The overlap of the two distributions is owing to two facts: not every electrolysis [experiment] produces a nuclear reaction (I do not know the reason), and the reaction may have taken place away from the chip. This second reason is a strong possibility because I often see one side of a chip with a large number of tracks whereas the other side has only a number characteristic of the controls. I appreciate that for a demonstration you would like an experiment that always produces the expected result. However, we are not yet able to have that assurance in this field. Because of the two reasons discussed above we must rely on a statistical analysis of a large number of experiments unless, as you say, one gets lucky and sees a 'shower' as I showed in my lecture." Yes, reproducibility is a big issue, as in most cold fusion experiments.

I do not know how to explain large changes (order of magnitude) in new track densities from control samples. It is significant that the method used allowed to discriminate against tracks which were formed in CR-39 before experiments. Would the use of distilled water be preferable? It depends on the role of impurities in cold fusion phenomena. Several scientists speaking at the conference (ICCF10) think that successes and failures of cold fusion phenomena depend on uncontrollable impurities, often below the ppm level. The results presented by R. Oriani and J. Fisher are remarkable because new track densities in control samples are always much lower than corresponding densities in active samples. At least this aspect seems to be highly reproducible.

Knowing that I plan to share his data with other teachers Dr. Oriani wrote: "you have my permission to share and operate on these data in any way you please." In a message commenting on the draft of this document he added: " I hope that your web site causes someone to replicate the experiment." I also hope that this will happen. It would be a great student-oriented project. The task is well defined: to confirm or not to confirm results published in a refereed journal in Japan. I think that the journal would publish a serious study of that kind. Too bad that I did not meet Dr. Oriani when I was learning how to use CR-39 detectors last spring (for an experiment which turned out not to be necessary). I was on sabbatical at that time; now I am too busy teaching. Perhaps I will have more time in the Spring semester, or after I retire in May of 2004.

POST SCRIPTUM:
The CR-39 detectors can be cut into small pieces, for example 1 by 1 cm, by using regular scissors. They can also be labeled by using a sharp needle. No darkness is required, as with photographic emulsions. A team of Russian scientists (A. Lipson, now working in the University of Illinois, and A. Roussetskii, from Lebediev Institute in Moscow) managed to distinguish alpha particles from protons by using CR-39 detectors in combination with very thin filters. But such task is much more difficult than with nuclear emulsions. The unique advantage of CR-39, as illustrated by R. Oriani and J. Fisher, is that a chip can be "developed" twice.

Suppose the first development produced 37 tracks near a recognizable scratch on the detector's surface. This is the background due to previous long time exposure to radon, cosmic rays, etc. The chip is then used in an experiment and developed again. The previously seen tracks are still visible but an additional set of new tracks appears, for example, 10. Then we know that new tracks are real; they were created when the experiment was performed. This approach is much better that using two different chips, that is: getting 47 from one chip (background plus signal), getting 37 from another (background only) and subtracting these two numbers to get 10. The net result obtained in that way can not be distinguished from statistical fluctuations.

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109) Cold Fusion: Fire from Water

Ludwik Kowalski (September 8, 2003)
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A week after returning from the 10th International Conference on Cold Fusion I was trying to explain the essence of the cold fusion phenomenon to my sister-in-law, Helen Knott. She wanted a very simple explanation; the video cassette I was showing her was "not good enough," she said. In my opinion that cassette ("Cold Fusion: Fire from Water" authored by E. Mallove and J. Rothwell), available at

http://www.infinite-energy.com

is very good for people who have studied some physics and chemistry; I was not aware that a person without such education might not be able to follow the narrative. This brief summary is dedicated to Helen. It is an introduction that I will make before showing the cassette to a nonscientific audience. I plan to do this at Montclair State University, probably in October or November. Am I right to assume that at least 99% of the general population knows nothing about the cold fusion controversy?

Burning of wood, coal and oil consist of chemical reactions. Each gram of fuel yields a certain amount of thermal energy. But stars, like our sun, have no such chemical fuels; their thermal energy is generated from nuclear reactions, as in a hydrogen bomb. The fuel used in these thermonuclear reactions is hydrogen. The reaction is called "fusion" because two tiny atomic nuclei must fuse into one larger nucleus before the energy is released. The idea of a slow non-explosive "nuclear burning" is not new; scientists started investigating this possibility in early 1950s. For a small fraction of a second they raise the temperature of hydrogen to millions of degrees hoping to get more energy out of a system than the amount of energy needed to operate it.

Unfortunately, progress was much slower than anticipated. Tens of billions of dollars were spent each year but the amount of thermal energy generated in huge fusion furnaces continues to be a small fraction of the energy used to operate them. It is important to emphasize that a great deal of understanding resulted from decades of these investigations. We know that the possibility of building thermonuclear fusion devices producing more energy than is necessary to operate them is real but decades of additional research will be needed to achieve this goal. That is why the entire scientific world was galvanized when two scientists from the University of Utah,
Fleischmann and Pons, made an announcement (in 1989) that they had a device producing more energy than what was needed to operate it. The device was as small as a can of beer and could be operated from a car battery. The cost of materials needed to build a demonstration device was less than $1000.

Even today, nobody understands what was going on, but the amount of excess heat generated was much larger than what could possibly be attributed to chemical reactions going on inside. It was thus hypothesized that nuclear reactions of some kind were taking place in the relatively cold environment. Utah scientists, and others who were able to reproduce the effect, did not say that the reactions were the same as those going on in hot fusion furnaces. But they were criticized as if that was the essence of their discovery. The opponents coined the term cold fusion and convincingly argued that experimental facts, known in 1989, were not consistent with the cold fusion mechanism. Furthermore, many scientists failed to observe excess heat in their laboratories.

On that basis the extraordinary claim of Utah scientists was officially declared to be invalid (by a panel of experts appointed to investigate the phenomenon). Subsequently, the US Department of Energy, and other government agencies, decided not to support cold fusion research. Furthermore, editors of major scientific journals decided not to

publish articles devoted to such research. Despite these unprecedented decisions at least one hundred researchers, in several countries, continued to investigate the puzzling phenomenon. Most of them are highly qualified to conduct such research; their expertise was recognized long before the discovery of cold fusion. Fleischmann, for example, was a widely respected electrochemist and a professor at the University of Southampton. He was a Fellow of the Royal Society in England and the author of more than 200 scientific publications. In 1979 he won the medal for Electrochemistry and thermodynamics. Six years later he was awarded the Palladium Medal by the U.S. Electrochemical society. Between 1970 and 1972 he was the president of the International Society of Electrochemistry.

Unlike supporters of other controversial claims, such as UFOs, healing with copper brasletes, perpetual motion machines, etc., nearly all cold fusion investigators are Ph.D. scientists, often retired, who have been associated with prestigious universities and research laboratories. Yet they have been “excommunicated” from the rest of the scientific community as practitioners of “pathological science.” In my opinion, there were no need to condemn a field in less that a year after the first discovery was announced. The field should have been allowed to develop, like any other thread of scientific investigations, in order to decide, scientifically rather than administratively, about the validity of its claims. Some claim that the official “blacklisting” was motivated by political, rather by scientific, reasons. I am not qualified to validate this claim. In my opinion, the arguments used by critics in 1989 are at odds with many recent findings, and a second national evaluation of the entire field, by an appointed panel of experts, is needed.

I also think that the main emphasis should be on scientific aspects of cold fusion; it is premature to argue about practical applications at this time. Promising too much, and too early, was one of the mistakes of those who supported cold fusion when it was originally announced. The present situation is highly abnormal. This can be best illustrated by the titles of two recently published books, that of Robert Park and that of Charles Beaudette. The title of the first book, published in 2000, is “Voodoo Science; the Road from Foolishness to Fraud;“ the name of the second book, published in 2002, is “Excess Heat. Why Cold Fusion Prevailed?” No, cold fusion did not prevail, it is still treated as it were pseudo science. To convince me that cold fusion is real a person should allow me to perform an experiment in which nuclear events, such as progressive accumulation of helium, or tritium, is demonstrated. Such observations have been reported by many investigators but, in the current abnormal situation, I want to see things for myself. I would also be convinced if I could see that isotopic compositions of various chemical elements change in a cold fusion apparatus.

I do believe, that sooner or later, perhaps in fifty years, the cold fusion puzzle will be resolved, one way or another. This optimism is justified because “the scientific process is self-corrective. This unique attribute sets science apart from most other activities. The scientific process may on some occasions move slowly, sometimes even along a circuitous path. The significant characteristic of the scientific method, however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error.” This statement was made by John Huizenga, the author of a highly critical book about cold fusion, published in 1991. The title of that important book was "Cold Fusion: The Scientific Fiasco of the Century."

The cold fusion episode will never be forgotten; perhaps it will be viewed as the beginning of a very significant new period in the history of science. Suppose that the claim for nuclear reactions occurring in so-called “cold fusion” experiments are definitely validated by mainstream scientists. In that case the problem of blacklisting of a large group of recognized scientists by the so-called “scientific establishment “ will be debated. Something will be done to make sure that a similar event does not happen again. On the other hand, the definite rejection of cold fusion claims will result in focusing on another issue. How come that so many highly qualified scientists, in so many countries, were able to deceive themselves, for decades, by phenomena that were not real? How do we know that phenomena of similar massive self-deception do not exist in other areas of science? Something will be done to improve scientific methodology, to make sure that a similar long-term blindness does not occur again. Of course, such issues belong to the realm of social interactions among scientists. That is why the “cold fusion episode” deserves to become a topic of general interest.

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111) A political campaign using C.F. ?

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Today I received an interesting e-mail message from Dr. Eugene Mallove, editor of Infinite Energy Magazine and author of a well known book “Fire from Ice” (John Wiley and Sons, Inc., New York, 1991). The message (see below) is a call for using the cold fusion issue in the upcoming presidential campaign. I do not think this is appropriate. Cold fusion research is still in its infancy and nobody claims that practical industrial applications are on the horizon. Cold fusion is no closer to practical applications than hot fusion was half a century ago. The path from recognizing the possibility of an application to a commercial success is highly unpredictable. Promising electric energy “too cheap to meter” was one of the mistakes of hot fusion promoters. That was in the late 1950s; and they were in a much more favorable situation than cold fusion researchers are today.

Like Eugene, whom I had pleasure to meet at the 10th International Conference on Cold Fusion, I think that a panel of experts should be appointed, for example, by the National Academy of Sciences, to investigate the validity of cold fusion claims formulated since the initial 1989 report. Physics teachers, like myself, need official evaluations in order to know how to answer questions about cold fusion. My recent letter to the editor of The Physics Teacher (published in June, 2003) is an appeal for a formal investigation of new cold fusion findings.

But I would be against politicizing this issue in a presidential campaign. Why should cold fusion, and not, for example, solar energy, be singled out? The first goal of the cold fusion community (if it were organized) would be to develop a reasonably reproducible device demonstrating nuclear phenomena at ordinary temperatures, for example, emission of protons, tritons or alpha particles, production of chemical elements with abnormal isotopic ratios, accumulation of helium (correlated with excess heat generated at the rate of 23.6 MeV per atom), etc. Such devices have been built in several countries, including the US, but they are not available to most of us. The second goal of an organized cold fusion community would be to develop a theory of cold fusion phenomena.

Attempts to build useful devices are not likely to succeed at the present level of our understanding of cold fusion. That is why it is too early to promise anything practical, especially in a political campaign. Yes, I am aware that the rapidly aging cold fusion community is highly dispersed and leaderless (see the footnote). Each scientist, or group of scientists, works as if cold fusion were just another officially recognized field of investigation. And each believes that his work will finally convince all honest skeptics. The total number of active cold fusion scientists, worldwide, is probably about the same as in the Manhattan Project. But in the last 13 years they have not been able to convince the rest of the scientific community that their claims are valid.

Performing beautiful experiments is not enough; something else is needed to convince the entire scientific community that cold fusion phenomena are either real or false. Eugene is right; a second national review of the cold fusion field could clarify the current, very unfortunate, situation. Ten years of dedicated research, done by hundreds of top scientists, worldwide, should not be ignored. It should be evaluated by an appointed panel of experts. Why should their “labor of love,” thousands of man-years of sophisticated research, be wasted? The panel should be created before the scientists, and the instruments they constructed, become too old to be useful. The question is how to make this happen?

The initiative, in my opinion, should come from honest mainstream scientists, teachers and engineers. They, not the general population, should be approached by cold fusion scientists and asked to examine recent findings. Professional conferences and journals provide an ideal ground for this kind of initiative. I am aware that some editors and conference organizers think that cold fusion is voodoo science and that society should be protected from it as from a dangerous

disease. They place cold fusion researches in the same category as fraudulent market manipulators, believers in UFO and practitioners of copper bracelet therapy. A way of bypassing dogmatic officials must be found. My own interest in recent cold fusion findings was triggered, one year ago, at a technical conference at which cold fusion researchers were allowed to make presentations. I do not know what else can be done; a political campaign suggested by Eugene is not likely to result in the second national review of the cold fusion field. Here is what Eugene wrote to me, and probably to many other conference participants:

"This will be my question to ALL presidential candidates that I happen to encounter during the New Hampshire Presidential Primary season, which is already well underway in September 2003:

****

Candidate X: I'm Dr. Eugene Mallove of Infinite Energy Magazine in Concord.

I'd like to ask what YOUR position is on an issue that transcends and yet encompasses ALL the other issues that you and other candidates talk about — peace, freedom, the economy, healthcare, the environment, and - of course - energy independence:

During your campaign will you ask for and DEMAND a review by the US Department of Energy or by the National Academy of Sciences of the now overwhelming body of scientific evidence that supports cold fusion/low-energy nuclear reactions energy? This is clean, abundant energy from water, a scientific discovery made in the United States and announced in 1989 — and then crushed, as it was being confirmed, by arrogant vested academic and bureaucratic interests?
****

A Yes or No answer will do!

If anyone wishes to post this question elsewhere, or to ask it himself or herself of any candidate -- in ANY primary state, I would be thrilled. Please let me know if any responses are received from any of the presidential candidates or their senior advisors. We will post responses on our web site and elsewhere."

Ludwik's footnote:

1) The only "leader," as far as I know, is a person chosen to be the chairman of the next cold fusion conference. The responsibilities of that person are limited to conference-related tasks.

2) There is also a web site (www.lenr-canr.org) maintained by a dedicated individual, Jed Rothwell. Its function is to inform rather than to direct scientists. That web site became an excellent source of downloadable cold fusion papers.

3) A pier-reviewed electronic journal devoted to cold fusion research has just been created. Will the editor of that journal, professor P. Hegalstein from MIT, become a leader (as Oppenheimer or Grove were for the Manhattan Project)? I do not think so.

4) Those who reject cold fusion claims, on the other hand, are highly organized. That group consists not only of an army of active research scientists, for example, those working in national or industrial laboratories, but also of official managers of money available via institutions, such as NSF, DOE, APT, NAS, etc.

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111) Photos of Fleischmann and Jones, August 2003

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This item contains pictures of Martin Fleischmann and Steven Jones. I took these photos during the 10th International Conference on Cold Fusion (Cambridge, Mass., August 2003). And I was not able to resist temptation of showing myself in the company of at least one of these two scientists.

Martin Fleischmann with a glass containg cold fusing ice.

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Steven Jones and myself.

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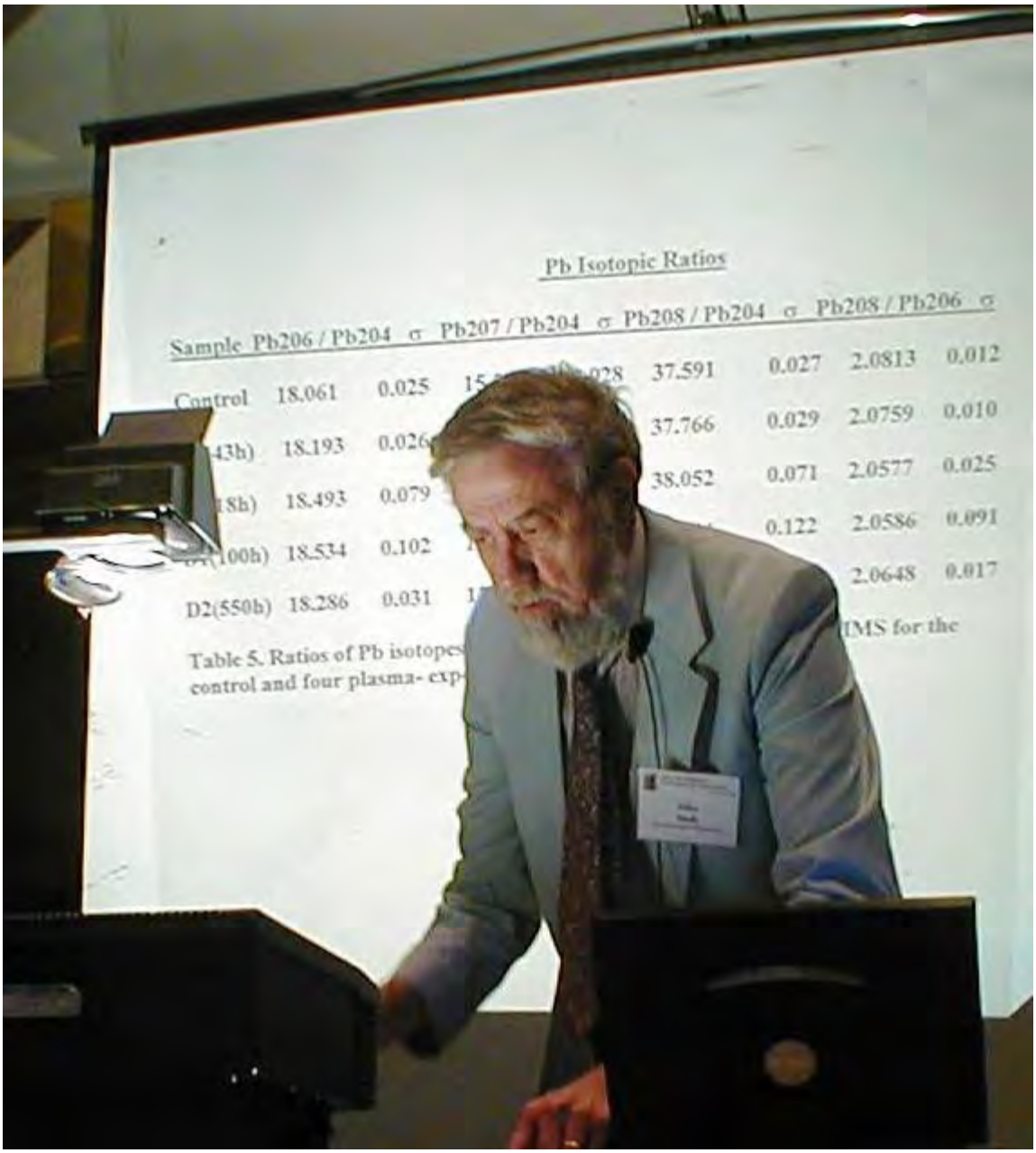

And this John Dash, a physics teacher from Portland State University at the conference presentation. John also brought a group of students to demonstrate their own cold fusion projects (see items #103 and #98). I suppose he is the first teacher in the world to direct a student project devoted to cold fusion research.

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A high school student showing a simple excess heat setup.

NOTES:

1) Prior to 1989 Fleischmann (with Pons, who is no longer active in the area of cold fusion) and Jones were using similar electrolytic cells to study completely different phenomena. Their laboratories were only about 20 miles apart but the two Utah scientists did not know of each other's work. Fleischmann was a chemist, Jones a physicist; each was known as a top scientist in his field. They intended to cooperate, to study the phenomena systematically, and eventually publish the result of findings. That would have been the normal way of conducting science.

2) But, due to unusual circumstances, the cooperation did not materialize. After 1989 the scientists became victims of bureaucratic manipulation and their work the subject of an unprecedented public debate. The scientific community became polarized; some rejected cold fusion findings on the basis of presumed errors, incompetence or fraud; others took the data seriously and continued investigating. Most scientists and teachers (among those who are still interested in the controversy) wait for an official verdict from a team of appointed experts. They are not equipped to conduct sophisticated cold fusion experiments. It turns out that most questions asked by early critics have been answered by those who continued conducting research. But are the answers satisfactory? The appointed panel of unbiased experts should answer such questions. In my opinion our National Academy of Science, or National Science Foundation, should take the initiative. The focus should be on scientific claims, not on practical aspects of cold fusion. It is premature to speculate about practical applications at this time.

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My oral contribution at the 10th International Conference on Cold Fusion was made in the form of Power Point slides. The content of these slides has already been posted as item #80 on this web site. Several days ago the manager of the LENR-CANR web site, Jed Rothwell, suggested that I write a paper, based on slides, for the conference proceedings. What follows is the paper I wrote today; it will probably be downloadable from the Library of the LENR-CANR site < http://www.lenr-canr.org >. My poster contribution to the conference is also going to be downloadable from that site.

112) The dilemma of a physics teacher

Ludwik Kowalski
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This presentation is dedicated to an unknown high school chemistry student who sent me an e-mail message last spring. She wrote:

"Help! My name is Maggie Johnson and I am a sophomore at Saratoga High School. In my chemistry class, I am doing a project on Cold Fusion. I was looking on the Internet for websites on Cold Fusion, and I came across links to your Cold Fusion items. I was wondering if you could give me some advice or information?"

1) A year ago I would have replied that cold fusion is pseudoscience. **But I am no longer comfortable with this kind of reply.** Why am I not comfortable? My first opinion was based on Huizenga's famous ERAB report. I knew the author personally and I respected him. His criticism of cold fusion was convincing because it was based on the idea that cold fusion is a thermonuclear reaction between two colliding hydrogen ions. Experimental data certainly do not support such an idea. But who made this claim? Only the popular press and those who criticized cold fusion. Fleischmann and Pons, who discovered excess heat, speculated that some kind of nuclear processes might be responsible for what they observed. The very name "cold fusion" was highly unfortunate because it misled many scientists, including myself. Who invented this term? Was it introduced deliberately (as a straw man) to discredit the nascent field of research? I have no answers to such questions. But I suspect that this misleading term will survive the controversy; that is why I use it here.

2) Two other factors helped to discredit the cold fusion field in many minds: the claim that experiments in this area are extremely simple, and that practical applications are going to be possible very soon. Again, I do not know who the authors of such claims were. Those who criticize cold fusion today, Park in the US and Kruglyakov in Russia, essentially repeat Huizenga's arguments. **What was convincing in 1989 is no longer convincing today.** Why do they ignore the generation of helium? Why do they ignore more sophisticated calorimetry? Why do they ignore unnatural isotopic ratios? Why are they not at this conference listening to presentation of new data and defending their own ideas? That is another set of questions that I am not able to answer. Ignoring experimental data is not an acceptable method of addressing a scientific controversy.

I am still not convinced that cold fusion is real. But I no longer say that cold fusion is voodoo science. I do read papers downloadable over the library of the Internet web site < http://www.lenr-canr.org > and I share what I understand with other physics teachers < at http://csam.montclair.edu/~kowalski/cf/ >. Feel free to explore my short essays and to share them with others. **Why am I still puzzled?** Because everything I know about nuclear science goes against the idea that

nuclear reactions can be induced by chemical processes at ordinary temperatures. I wish I had a chance to personally participate in experiments generating extraordinary results. But, like most teachers, I have no access to a sophisticated laboratory which would be needed to verify accumulation of helium and heavier reaction products. I read about such phenomena and I am impressed. But I would be more comfortable if the reported results were examined and officially confirmed by an appointed panel of experts.

3) I am also puzzled by the fact that **hundreds of sophisticated research scientists exploring cold fusion over the past 13 years have not yet developed a reliable demo for teachers**; windows of opportunity did exist in several countries. Teachers need experiments that can be performed with simple instruments available in schools, such as Geiger counters and gamma ray spectrometers. Excess heat generated at a rate of about one watt is not convincing unless one is able to deal quantitatively with all possible chemical reactions taking place in the apparatus. Reproducible generation of excess heat at the level of twenty watts, or higher, for a long period of time, would be much more convincing to a physics teacher, especially if it could be correlated with emission of nuclear particles or gamma rays. Even a 70% reproducible demo would be useful; teachers know that some experiments, for example in electrostatics, do not succeed when humidity is too high. Cold fusion seems to depend on factors which have not yet been identified. Abnormal isotopic ratios, reported by many independent researchers, are extremely convincing but a typical teacher can not verify such data.

4) That is why I would be more comfortable if the reported results were examined by an officially appointed panel of experts. **A second national evaluation of the entire cold fusion field is definitely needed to clarify the situation.** I am also disturbed by the fact that some cold fusion data are reported in journals which also publish papers dealing with hydrinos, antigravity, rotational fields, quantum healing devices, etc. etc. I know that leading journals do not accept articles devoted to cold fusion and that many scientists share their observations via channels available to them. It is better to use these channels, they think, than not to publish. That is natural. But it would be better if claims based on speculative extrapolations from exotic theories (and perhaps on wishful thinking) were not mixed in with cold fusion claims which are basically experimental. Therefore I strongly support the announced initiative of starting an electronic peer-reviewed journal devoted exclusively to cold fusion topics. Voodoo science does exist and cold fusion researchers should separate themselves from it.

5) I am optimistic that the cold fusion controversy will be resolved, one way or another.
The optimism is based on the following quotation from what John Huizenga, the author of the ERAB report, wrote in 1989. "**The scientific process is self-corrective**. This unique attribute sets science apart from most other activities. The scientific process may on some occasions move slowly, sometimes even along a circuitous path. The significant characteristic of the scientific method, however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error." And here is another quote from the panel of appointed scientists responsible for the first national investigation of cold fusion. **"**The Panel recommends against the establishment of special programs or research centers to develop cold fusion. However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system." **Sympathetic attitude toward unresolved issues** is worth emphasizing.

6) What will be the verdict of history? Sooner or later, perhaps in 50 years, the cold fusion puzzle will be resolved (like the "puzzle of cybernetics," or the "puzzle of genetics," both in USSR). Only two outcomes are possible: (a) CF phenomena will finally be confirmed or (b) CF phenomena will not be confirmed. In each case one will have to deal with important social issues. Suppose that CF is confirmed. Then one would have to explain causes of a long-lasting conflict between scientists and administration. Suppose that CF is not confirmed. Then one would have to explain a phenomenon of massive self-deception involving hundreds of top scientists in many countries. In either case you will be recognized as participants of an important and **unique event in the history of science**.

Keep working to clarify the most intriguing scientific and social puzzle of the 20th century. I am certainly not the only physics teacher waiting for a consensus on cold fusion. Keep submitting good papers to traditional refereed journals, such as Physical Review, etc. **Do not be discouraged by frequently unjustified rejections** of your papers. Document such rejections and make them known to mainstream scientists. Deplorable confrontations with overly bureaucratic editors should also be exposed. Take advantage of the new electronic journal devoted to cold fusion. Dissociate yourself

from voodoo scientists and openly criticize them. Keep bringing cold fusion topics to scientific conferences devoted to areas overlapping with your activities. My own interest in cold fusion was reawakened at such a conference one year ago. Try to seek contacts with students, and with the general public. But focus on puzzling scientific results; it is too early to speculate about practical applications.

7) Let me finish by describing **my personal encounter with the process of rejection**. About half a year ago I wrote a letter to the editor of Physics Today. In that letter I described my own dilemma in dealing with cold fusion and asked for help. Why was my short letter rejected? Why was I not allowed to see what the referees wrote about it? Ironically, that letter was triggered by the article entitled "New APS Ethics Guidelines Address Research, Misconduct and Professional Responsibilities."
That article by Jim Dawson was published in the January 2003 issue of Physics Today. I welcomed the new guidelines and asked how a physics teacher can make sense of "cold fusion?" Was the research conducted in that area, in the last ten years, a "departure from the expected norms of scientific conduct" or did it "lead other scientists along fruitless paths?" I see no evidence that the data were "fabricated." As a physics teacher I am confused by the situation. Some say it was "a fiasco" while others say it was an "important discovery." How should teachers address this topic in the context of "public affairs between science and society," or in the context of discussing "institutional support for new ideas and innovations?"

After waiting several months I sent an e-mail message asking about the status of my letter. On Thursday, June 12, 2003, I received a reply from Dr. Marty Hanna, Letters Editor at Physics Today. He wrote: "So far, **I have a split decision on the possible publication of your letter.** I expect soon to have a tie-breaking input from a third reviewer. I will let you know as soon as I have a firm decision. Thank you for your patience." On Thursday, July 3 Dr. Hanna wrote " We have completed our review of your letter commenting on the APS ethics guidelines story in our January 2003 issue. Our decision, after some valuable discussion, is not to publish your letter. Thank you for writing and for your interest in Physics Today."

Unhappy about this I wrote "I would very much appreciate if you could send me the reports of the referees evaluating my letter to the editor." The immediate reply was "Please let me explain. I know that scientists who submit articles to peer-reviewed scientific journals expect reviewers to give them a critique of their letters. Physics Today is not, in the strictest sense, a peer-reviewed scientific journal; it is, instead, a **special-interest magazine** for physicists. Generally, my reviewers are staff writers and editors (all physicists) who may give me little more than 'thumbs up' or 'thumbs down' on a letter. As a rule, we do not give out the specific comments of the reviewers, because we consider them to be internal business. Thank you for your inquiry. I hope my explanation has helped."

The input from three qualified referees would be very useful to me. I am a member of APS and I expected a better treatment from my journal. This is not a healthy situation. Here is how it was described by Dr. E. Storms: "Serious scientists rejected 'cold fusion' in the past for good reason. These reasons no longer apply. **If science cannot correct a past rejection, then what good is the scientific method?"** Physics Today should promote exchanges of information among recognized physicists from different disciplines. It is an ideal place to ask the question formulated in my letter. Why did Dr. Hanna reject the letter after one of the referees recommended it? Which special interest is he protecting by not publishing the letter? What is wrong with asking for another evaluation of new cold fusion claims by an appointed panel?

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113) Recent Papers of Steve Jones et al.

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Many interesting papers were presented at the 10th international Conference on Cold Fusion (in Cambridge, August, 2003); some of them are already downloadable from the < http://www.LENR-CANR.ORG > web site. (click of ICCF10 PROCEEDINGS and scroll down to the list of papers). One of the most convincing evidence that the unexplained nuclear processes are real was presented by Jones et al. I am referring to his papers on the emission of neutrons and on the emission of charged particles.

1) The paper on neutrons reminded me of the old argument against cold fusion, absence of neutrons commensurable with the reported amount of excess heat. Suppose that excess heat, generated at the rate of 10 W, is due to well known thermonuclear fusion of deuterons. How many neutrons would be emitted in each second? To answer this question one must know that the amount of energy released from each thermal fusion event is about 3.5 MeV and that, on the average, one out of two events is associated with the release of a neutron. The other half are events associated with the release of protons. Converting 10W into $6.25*10^{13}$ MeV/s one finds $1.8*10^{13}$ fusion events per seconds and nearly 10^{13} neutrons per second.

This result is by many orders of magnitude higher that what has been observed. The unavoidable conclusion, reached as early as 1989, was that thermonuclear fusion can not possibly by a dominant mechanism generating excess heat in cold fusion experiments. Nuclear reactions taking place in cold fusion remain unexplained (production of heat without emitting a lot of neutrons) but one thing is clear, they are very different from well known thermo-nuclear reactions. Who invented the terms "cold fusion" and “hot fusion?” These unfortunate terms suggest the idea that cold fusion reactions are the same as hot fusion reactions. As far as I know, nobody has ever made such claim, except journalists and some book writers. Did they deliberately create a “straw man” to justify criticism of cold fusion? It is easy to criticize people for claims they do not make. The real claim of scientists studying cold fusion is that some, previously unknown, nuclear processes occur at metals loaded with hydrogen isotopes.

Therefore the issue of neutron commensurability, the central argument of those who criticize cold fusion, is totally irrelevant. Even a very small number of neutrons, emitted from metals loaded with hydrogen, is highly significant in the context of showing that something totally unexpected is taking place. That is why I was very impressed by the evidence, presented by Steve Jones et al., that neutrons have been detected. Their experiment was complicated by the fact that the emission rates are very small. The efficiency of detection of neutrons was maximized by using a setup of sixteen detectors in close proximity to metallic foils loaded with deuterium. The arrangement was able to detect approximately one out of ten neutrons, on the average. Additional complications resulted from presence of cosmic ray neutrons. That background was minimized by performing experiments underground (at the depth of 100 meters), by surrounding detectors with layers of additional absorbers and by using an electronic method of partial rejection of neutrons coming from the outside of the experimental setup. In a typical experiment, lasting several hours, the counting rate was about 8 per hour while the background was close to 2 counts per hour. Correcting such raw data for efficiency, and subtracting the background, the counting rate could be as high as 60 neutrons per hour.

Counting nuclear particles at a rate which is only four times higher than the background would not prevent one from measuring the net counting rate very accurately if sufficiently long periods of time, for example, several days or weeks, were available. Unfortunately such luxury is not yet available when particles are emitted from metallic foils loaded with deuterium. Conditions favoring nuclear processes have not yet been identified but, according to experimental data, they

often disappear after a couple of hours or so. Two methods of creating favorable conditions were used by Jones and his collaborators. The first method consisted of placing hot titanium foils into deuterium gas, the second consisted of treating foils with a weak D_2O solution of the unusual sulfuric acid, D_2SO_4. Not every treatment resulted in creating a nuclear active environment, the rate of success, as far as the emission of neutrons is concerned, was about 40%. In my opinion, this fact should not be used as an argument against cold fusion. Further progress can be very fast when a team of highly trained scientists is already able to observe a new phenomenon in one out of two or three experiments. Something significant is still not under their control and additional research is necessary.

2) The second paper presented by Jones and his coworkers described experiments with charged particles. In one experiment such particles, identified as 2.6 MeV protons, were counted at the rate of 2,171 ± 93 counts/hour. This was 400 times higher than the background and the repeatability was as high as 70%. Low energy protons, as described in item #28 (at my cold fusion web site), have already been reported by Lipson and his coworkers. The method of detection used by Lipson was based CR-39 track detectors while the method used by Jones was based on scintillation and silicon detectors. What can be more trustworthy than observation of protons by two teams of highly qualified scientists working in different laboratories and using different experimental techniques? Why are these experimental data ignored by those who keep repeating that cold fusion is voodoo science? The arguments used by them are based on what was known 13 years ago, not on knowledge accumulated in the last ten years.

The authors claim the "repeatability" exceeding 70%. I suppose that it means that nuclear particles are not always emitted from thin titanium foils loaded with deuterium. Why is it so? Because something is still not under control of experimentalists. But being successful 70% of time is very significant, considering the absence of a theory. Keep in mind that Jones's papers are downloadable from the above web site; my purpose is to summarize them, and to comment.

3) The third paper of Jones and Ellsworth, downloadable from the LERN_CANR.ORG web site, is very different from the first two. I would call it a vision paper; it focuses on old speculations of great importance to planetary science and on anticipated research in that area. Here is how the essential hypothesis was formulated by the authors. *"Natural geo-fusion in the earth occurs in or near the core of the earth, in the hot, hydrogen-bearing metals and minerals which are subjected to extreme off-equilibrium conditions deep in the earth. This hypothesis can be tested by measuring tritium and helium-3 in magmatic fluids from hot-spot volcanoes which tap magmas from plumes arising from the core-mantle boundary. In particular, magmatic waters of Kilauea, Loihi, and Icelandic volcanoes are predicted to contain significant tritium. We predict that tritium is also present in Jupiter, originating from 'cold' fusion in or near its metallic hydrogen core."*

In the second part of their short paper, the authors speculate that cold fusion might play an important role in nucleosynthesis of elements. Nuclear transmutations, reported by many cold fusion scientists, give credence to such speculations. The article ends with a list of sophisticated analytical tools available to the Department of Physics and Astronomy of Brigham Young University; Steven Jones is a professor in that department. I suppose these tools will soon be used to expand current investigations of new nuclear phenomena. Let me end with another quote from this article; it illustrates a healthy attitude of cold fusion scientists toward research.. *"Neither cold fusion nor cold nucleosynthesis is understood at present, nor are the results yet widely accepted by the scientific community. But as we continue to explore together, cooperative experiments at several laboratories giving positive results cannot be ignored much longer."* I hope the authors are right and that the entire field will soon be recognized as a valid area of useful research.

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114) Another Debate Among Teachers?

Ludwik Kowalski (October 31, 2003)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

This morning (10/31/2003) I posted a message about cold fusion on the Phys-L discussion list. The immediate reply from Teacher 2, who often speaks for AAPT (American Association of Physics Teachers), made me think that other messages might follow. But they did not. It shows that many of us think that Phys-L is not an appropriate place to continue arguing about claims made by cold fusion researchers. There are two good reasons for this: (a) most of us are not trained is electrochemistry, highly accurate calorimetry, sophisticated nuclear detectors, mass spectrometry, surface chemistry, etc., b) it is a political issue often driven by emotions rather than by rules of scientific methodology.

Instead of responding on Phys-L list I will try to do this here. I will start by quoting both messages (hiding the identity authors) and by asking for comments to be sent to me in private. Your comments might enrich this item. Please write to me about evolving attitudes toward cold fusion. Samples of representative “voices from teachers” are worth collecting and preserving; future historians of science might benefit from their existence. Next year I plan to attend the 11th International Cold Fusion conference (in Marseilles, France) and this item might become an interesting poster presentation. I would also like to know how many people found my cold fusion web site worth visiting. It was created for you, and for your students. Should I continue using it to share what I learn and think about cold fusion?

Teacher #1 (myself):
My short summary of three recent papers of Steven Jones has been posted as item #113 at:

http://csam.montclair.edu/~kowalski/cf/

Steven Jones was one of three scientists whose work resulted in the "cold fusion" controversy, 13 years ago. Unlike Fleischmann and Pons, he is a physicist. Pons left the field while Fleischmann and Jones are still very active. Which Nobel Prize might they share, in physics or in chemistry? Physics. Is this possible? I think so.

But at this stage I would not recommend them (if I were entitled to suggest nominees). Instead I would argue that the second national evaluation of the entire "cold fusion" field is needed in view of recent findings. We, teachers, could play a role in asking for the second evaluation, as exemplified by my letter to the editor of our journal. Click below to see that letter.

http://ojps.aip.org/journals/doc/PHTEAH-home/letters/jun2003.pdf

My hope was that others will support the appeal (in responding to what I wrote) and that this will be noticed by NSF, NAS and AIP leaders. A letter to the editor, inspired by Jones' papers, for example, can have a significant impact. [Other research papers, presented at the Cold Fusion conference last August, are also downloadable from the same web site as Jones's papers. To get them go to http://www.lenr-canr.org , click on the ICCF10 and scroll down to the list of downloadable papers.]

Something will happen if many physics teachers write that they are confused by the present situation. We (and our students) have access to many good papers but our establishment keeps repeating old arguments that the entire field is "pseudo-science." Somebody should appoint a panel of experts to evaluate findings reported in the last ten years.

A letter to the editor of our journal, The Physics Teacher, can be submitted via e-mail. The editor is Dr. Karl. C. Mamola. The address is: < tpt@appstate.edu >

Teacher #2:
Ludwik, I disagree. TPT is not a research physics journal, and letters to the editor of TPT will not advance your agenda of reopening the issue of cold fusion. Such letters will actually alienate TPT readers like myself to your cause. Replicable research published in peer-reviewed physics research journals is required. Claims of conspiracy without results further discredits your cause.

Teacher #3: (in a message e-mailed to me in private):
In general I am supportive of cold fusion. However, I do not discuss the issue with others since I have found that most people, both pro and con, have more religious views on the subject than scientific. Anyway, that has been my experience, and that is my preference.

Teacher #1:
I am responding to Teacher 2 here (not posting it on Phys-L) because nobody else responded publicly. That is right; cold fusion papers should be published in peer-reviewed papers. TPT is not an appropriate place to argue either for or against the validity of claims made by experts. But it is an appropriate place, I think, to ask for help; most of us are not equipped for specialized research. Papers from high caliber scientists are available to us, and to our students; somebody has to help us to evaluate them objectively. I no longer know what to say when a student asks about cold fusion. A position paper published by an appointed panel of experts would help us to deal with a controversial scientific topic. What is wrong with expressing this opinion in TPT? By the way, my earlier attempt to ask for help on pages of Physics Today (in a letter to the editor) was rejected.

We need to know why claims made by cold fusion researchers should be labeled as not scientific; that seems to be the official policy of AIP. Do our leaders think that recent cold fusion claims result from a large-scale self-delusion involving hundreds of Ph.D. scientists? Do they think that it is a matter of international conspiracy to deceive? Do they think that fraudulent data are used to promote hidden agendas? If they do then they should try to convince us that such accusations are valid. They should support the initiative to appoint a new panel of experts. That panel, like the one appointed 13 years ago, would evaluate recent data and prepare us for dealing with recent claims. Who is against this and why?

By the way, what did teacher 2 mean by “claims of conspiracy without results?” And why did he say that I have an “agenda of reopening the issue of cold fusion?” I think that the 1989 ERAB report was valid; the evidence for unusual nuclear processes did not exist when that report was released. I see no need to argue about this. What I want to see is a critical examination of claims based on experimental data gathered in the last ten years. Need for such evaluation has been anticipated in the first report. How else should interpret the following quotes?: At this time “the Panel recommends against the establishment of special programs or research centers to develop cold fusion. However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system.” What is wrong with addressing the unresolved issues on the basis of new findings. And here is another quote. “The significant characteristic of the scientific method, however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error.” Who wrote this? John Huizenga, the chairman of the 1989 ERAB. I had a privilege of knowing John personally, and to learn from what he wrote about mechanisms of nuclear reactions.

Teacher 1 (myself):
Yesterday (November 7, 2003) I wrote:

> Such comments might help me to turn the item #114 into a compilation of
> interesting quotes. Please write about your evolving attitudes toward cold
> fusion. Samples of representative "voices from teachers" are worth collecting;
> future historians of science might benefit from their existence. Next year I plan
> to attend the 11th International Cold Fusion conference (in Marseilles, France)
> and item #114 might become an interesting poster presentation.

1) If you want your authorship to be known then please indicate so in your message to me. Otherwise you will be Teacher 5, Teacher 6, etc.; as in the poster I showed at the 10th conference. That poster, by the way, will appear in the book form (usual conference proceedings) at some times next year. They are working on it.

2) Feel free to focus on any aspect of cold fusion, either scientific or social. But try to be brief; emphasize what you think is original, new or highly significant.

3) Connections with teaching (questions students ask, projects they work on, comment they make, etc.) would be highly valuable. Also short descriptions of discussions you had with other teachers or parents (in the last ten years or so), etc.

4) No matter what the final verdict (perhaps in 50 yeas or so) cold fusion will be viewed as a strange event in the history of science, something comparable with what happened in Europe at Galileo's time. Help to document this event in the way that might be of some interest to future generations.

P.S.
5) Let me share an episode with you. The 10th conference had an open evening for the public. Not too many people came. One high school student asked: "what and where should I study if I want to become a cold fusion scientist?" In trying to answer I said something like this. "Do not focus on this right now. Learn physics, chemistry and mathematics; this will be useful to you no matter what you decide in four or five years. Cold fusion does not exist as a study track. Perhaps it is in the same stage at which aviation was 100 years ago. Or it may turn out to be a false alarm. Nothing would prevent you from being a cold fusion scientist if you decide to do so after establishing yourself in another technical or scientific field. Keep your eyes open and learn things which prepare you to be good in any field.

6) And here is something else. A conference organizer told me that he personally invited Robert Park, the APS spokesman and the author of the 2000 "Voodoo Science"
book, to come to the conference. The author had a "time conflict" excuse. A golden opportunity of learning about what is going on, or arguing that it does not make any sense, was missed. How does this differ from cases in which people refused to look at Galileo's telescope?

7) The conference was at a short walking distance from MIT. All professors (including experts in thermonuclear technology) were invited but they did not show up. What does this illustrate? But the conference chairman, Peter Hagelstein, was the MIT professor. He has been doing cold fusion research since 1989, mostly on the theoretical side.

Teacher 4:
Ludwik, I've read through about four of the links, including "what physics teachers think of CF" and the very first one, "Introducing cold fusion to students". I have to say it seems like a topic that really puts the Scientific method to the test, or perhaps the scientists! It seems like the debate revolves around whether or not the experimental discovery is worth considering, or if it was itself a fraud... I don't have time to read them all, but it's a very interesting archive. The clickable list of items is a bit daunting. You might wish to organize your webpage into a table, with links in each cell organized by sub topic, like "What is cold fusion?", "My articles regarding the subject", etc. It would make for an easier reading experience. ;)

Teacher 5:
I found your site last night and find it quite interesting. I am especially interested in the bits and pieces you have about the use of nano-particles. I read the comments made by Charles Kelly and have a few questions. First, I would consider myself to be a skeptic of "cold" fusion in that I am not convinced that it is real. However, at the same time, I have never been convinced that it has been disproved either. . . .

Teacher 6:
Recognizing the complexity of physical phenomena on solid surfaces Wolfgang Pauli once said that “surfaces are the invention of the devil.” Would he also say that “cold fusion is a diabolic science?” Cold fusion researchers are excommunicated as practitioners of forbidden cults. Terms like “pseudoscience” and “voodoo science” refer to heretical activities. The establishment, like the Inquisition, tries to protect true science from such influences. Fortunately for CF

researchers, the methods used against heretics today are not as brutal as they were five centuries ago.

Pauli is famous, among other things, for the discovery of a mystery particle called a neutrino. His investigation, as in the case of cold fusion, started when calorimetric measurements revealed an abnormality; heat generated in beta decay was produced at a rate much lower than expected. Instead of accepting the idea that energy is perhaps not conserved in beta decay, as suggested by others, Pauli postulated, on a purely theoretical basis, that beta decay is a three-body process. The third particle involved in the decay, the neutrino, is a carrier of missing energy. Experimental validation of this postulate occurred ten years later. In cold fusion the rate at which heat is generated was found to be higher than expected. What kind of processes are responsible for this? That is a big question. Scientists trying to answer it are not treated fairly, in my humble opinion.

Teacher 7:

Teacher 8:

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115) They need your support

Ludwik Kowalski (November 17, 2003)
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Today I received an e-mail message from Eugene Mallove. He is the author of a book about cold fusion, the chief editor of Infinite Energy magazine and the director of The New Energy Foundation. Dr. Mallove, whom I met at a recent cold fusion conference, is not an active research scientist. But he holds advanced degrees in astronautical engineering and environmental science. Eugene was a Chief Science Writer for the MIT News office when the discovery of cold fusion was announced in 1989. He is a fervent supporter of many energy-related causes. Personally I am not convinced that all these causes are equally valid. The practicality of cold fusion is possible but it is probably too early to anticipate benefits. The main emphasis, at this time, should be on fundamental research. The New Energy Foundation does support research projects, for example, a cold fusion investigation conducted by several high school students this year. The Infinite Energy magazine serves a very useful purpose summarizing research in many energy-related areas.

Mallove's e-mail message to me:

Ludwik,
I hope this note finds you well. Herewith, in the form of an MS Word attachment, is a distillation of the last 15 years of my experience in new energy -- with Internet site referencing. I hope that you appreciate its story and tone. There are some juicy quotes from several Nobel Laureates! I would very much appreciate your circulating this Appeal to those who might help our cause. The Nobel Laureate community and others should know about this! The field is greatly in need. We are at a critical turning point...

We recently received $100,000 from the New York Community Trust -- half to be awarded to grant applicants in new energy, and the other for support of Infinite Energy. The next issue of Infinite Energy (#52) has gone to press, many bombshell articles in it, as usual. You'll particularly like a table top experiment in thermodynamics, trivial to reproduce, that will make your head spin... Great for Physics Teachers -- a real monkey wrench into their self-satisfied complacency! Get a copy. All best,
Gene Mallove

Mallove's Universal Appeal:

New Energy Foundation, Inc.
(A nonprofit 501(c)(3) corporation)
P.O. Box 2816, Concord, NH 03302-2816
Phone: 603 485-4700 Fax: 603-485-4710
www.infinite-energy.com

Universal Appeal for Support
for New Energy Science and Technology

by Dr. Eugene F. Mallove
President, New Energy Foundation, Inc.
Editor-in-Chief, *Infinite Energy* Magazine

TO ALL PEOPLE OF THE WORLD who have open-minded curiosity, good will, good judgment, and imagination. To Scientists and Engineers, Philanthropists, Environmentalists, Energy Developers, High Technology Investors, Healthcare Professionals, Journalists, Artists, Writers, Business People, Entertainers, and Political Leaders. Whether you are Conservative, Liberal, Democrat, Republican, Libertarian, or Anarchist, and whether you may be Agnostic, Buddhist, Christian, Jewish, Hindu, Muslim, Atheist, or some other category of spirituality, this message is directed to all people of good will like you …

Dear Friend:

Here are some thoughts by wise thinkers -- background for this urgent appeal for your consideration and support of research and development of radically new forms of energy. These are energy sources that have the potential to turn the present world order upside down and bring about a bright new day for civilization:

"The exception tests the rule." Or, put another way. "The exception proves that the rule is wrong." That is the principle of science. If there is an exception to any rule, and if it can be proved by observation, that rule is wrong.
Richard P. Feynman (1963), Nobel Laureate in Physics (1965)

The progress of physics is unsystematic…The result is that physics sometimes passes on to new territory before sufficiently consolidating territory already entered; it assumes sometimes too easily that results are secure and bases further advance on them, thereby laying itself open to further possible retreat. This is easy to understand in a subject in which development of the great fundamental concepts is often slow; a new generation appears before the concept has been really salted down, and assumes in the uncritical enthusiasm of youth that everything taught in school is gospel truth and forgets the doubts and tentative gropings of the great founders in its eagerness to make applications of the concepts and pass on to the next triumph…But each new young physicist…is in danger of forgetting all the past rumination and present uncertainty, and of starting with an uncritical acceptance of the concepts in the stage of development in which he finds them.
Percy W. Bridgman (1961), Nobel Laureate in Physics (1946)

American Nobel Laureate in Physics (1988) Leon M. Lederman is no proponent of research into *radical* forms of new energy; one might accurately call him a "pathological skeptic" based on at least one opinion he has voiced (see *The God Particle*, 1993, p.122). Nonetheless, he somehow *senses* that a physics revolution may be upon us. He said recently, "You can smell discovery in the air…The sense of imminent revolution is very strong." (*New York Times*, November 11, 2003, p.D12). He is much more accurate than he can imagine, but not at all for reasons that he would readily accept! Perhaps he may be thinking of esoteric academic physics subjects such "string theory" or "cosmic dark energy," but certainly not practical technologies based on radical new physics. Having the intellectual problems identified by physics Nobel Laureate P. W. Bridgman in the quotation above, Lederman has *not* been looking at a large body of research that *will indeed* revolutionize the foundations of physics and give us command of fantastic new forms of energy. Too bad for Lederman; and too bad for us all that he has not been paying attention. We could use the support of people like Lederman…if they would only come to their senses, that is, examine open-mindedly the validity of *experimental data* that challenges their cherished *theories*.

In an article in *Science*, November 1, 2002, eighteen experts reported that they examined all the *conventionally understood alternatives* to fossil fuels and found them all to have "severe deficiencies" in their ability to deal with environmental problems while also being adequate to growing planetary energy needs. Physics Professor Martin Hoffert, leader of that research group, told the press that the United States would have to undertake an urgent energy research crash program, like the Manhattan atomic bomb project or the Apollo lunar missions. According to the *New York Times* (November 4, 2003, D1), Hoffert stated that we would need "Maybe six or seven of them [massive projects] operating simultaneously…We should be prepared to invest several hundred billion dollars in the next 10 to 15 years." Well, I have news for these experts: The solutions to our energy problems are very close at hand, and they *do* require

initial research and funding, but not the billions of dollars that such Establishment "experts" are accustomed to from government largesse. Rather, all that is needed perhaps are only several tens of millions of dollars to create robust prototype electric power generators based on new energy physics discoveries *that have already been made*. That is what this Appeal for Support is all about: to raise consciousness and funding for these radical alternative new energy sources.

Question: Do you believe that it is possible that modern science has overlooked or ignored major scientific discoveries, which -- if developed into technologies— would revolutionize almost every aspect of civilization? **It has!**

I will not catalogue the many horrors and troubles of this world that could be reduced or eliminated with an abundant, safe, and clean, radically new form of energy, if it were to be embodied in widely used technologies. You know these troubles already. But I do want to tell you about a significant path toward solving many of these problems, which we can all begin to take *now*, but about which you may have heard very little. You may have thought that no such path could exist. Let me assure you that it does and that thousands of researchers are already on it. They have traveled this unbeaten path to a new era for far too long without adequate support. I should know, I happen to be one of them. Yes, we have not reached our goals, but thanks to meticulous scientific research, huge sacrifices, and tireless work against great opposition, these objectives are now much closer to being realized. The basic scientific direction of the path forward has already been mapped out. **We need your support to go further on the path and reach our common destination: A world of abundant, clean, and safe energy from sources that have no centralized geopolitical control.**

Please attend to this appeal. I am most certainly *not* asking you to accept my claims at face value. But you must read, consider, study or review the compendious referenced material, investigate it, and then, I hope, you will be moved to *take action*. If you still have questions about these claims that need answering, I and my colleagues are available to answer them with facts, not hand-waving.

Who am I to ask *anything* of you on behalf of others, whether your attention for these brief moments, or for your financial and moral support? I am a scientist and an engineer with two engineering degrees from MIT (1969, 1970) and a doctorate from the Harvard University School of Public Health (1975). I have worked all my adult life as a dedicated scientist, despite my engineer's stripes. I have always sought to learn how the cosmos really works, and I find this process to be an exciting, difficult, and unending adventure, despite those who so erroneously claim that we are approaching "The End of Science" or a "Final Theory of Everything." Apart from my work in government-funded research at MIT and Harvard and later in corporate settings, I have also broadened my horizons by writing about science as an author and a journalist. Articles by me and about me have appeared in such venues as *MIT Technology Review*, *The Washington Post* Sunday "Outlook" section, the *New York Times*, *Popular Science*, *Analog*, *TWA Ambassador* in-flight magazine, *Wired*, and *New Hampshire Magazine*. I have appeared on many national radio programs, and for a time in the mid-1980s I was proud to have been a regular science and technology broadcaster for The Voice of America.

I am telling you something about me, not to elevate myself, but to convey to you something of my experience, sincerity, and integrity. I have written three acclaimed science books for the general public: *The Quickening Universe: Cosmic Evolution and Human Destiny* (1987, St. Martin's Press), *The Starflight Handbook: A Pioneer's Guide to Interstellar Travel* (1989, John Wiley & Sons, with co-author Dr. Gregory Matloff), and *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (1991, John Wiley & Sons). The late Nobel Laureate in physics (1965) Julian Schwinger endorsed my book *Fire from Ice* with these words: "Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly held preconceptions, he bares the truth implicit in a provocative variety of experiments." (He shared the 1965 Nobel Prize with Richard P. Feynman and Sin Itiro Tomanaga.) I am most proud of this latter book, because it began a jarring quest that led to finding out not only dramatic new truths about new accessible forms of energy in nature, but more important for me and you, the following most astonishing truth about modern "official" science: *Official science is not really intent on truly expanding scientific knowledge, in particular when some very, very fundamental scientific dogmas and theories are put at risk.*

Here is how one famous nuclear science professor at my alma mater MIT reacted to my request to him in 1991 to study the summary reports from two pioneering Ph.D. scientists, who had compiled seminal reviews about frontier experiments in low-energy nuclear reactions (a.k.a. "cold fusion"). One of the reviewing scientists was 34-year veteran researcher at our Los Alamos National Laboratory (LANL) and the other was a leader of research at India's Bhabha

Atomic Research Center (BARC):

"I have had fifty years of experience in nuclear physics and I know what's possible and what's not!…I will not look at any more evidence! It's all *junk*!" -- *MIT Prof. Herman Feshbach, May 1991, on the telephone to Dr. Mallove*

I hope you recognize that the late Professor Feshbach's most unfortunate and ill-considered reaction was fundamentally unscientific. It reminds me of the Church leaders at the time of Galileo, who refused to look through Galileo's telescope at the Moon or at Jupiter, because they "knew" that nothing new could be seen! Yes, many modern scientists are filled with catastrophic hubris; they have become in many ways mere "technicians of science," and guardians of what amounts to a pernicious "Holy Writ." *Don't bother me with the experimental evidence, my theory can tell me what is possible and what is not!*

If by chance you are one of those who believe that "all is well in the house of science" and that "official science" can be counted on to behave itself and always seek the truth --even in matters of central, overarching importance to the well-being of humankind -- you are sorely mistaken, and I could prove that to you with compendious documentation. (If you want to read what happened at just one institution, MIT, when a paradigm shift threatened established *hot fusion* research programs and "vested intellectual interests" such as those Prof. Feshbach so vehemently defended, read my 55-page report about this monumental tragedy at www.infinite-energy.com.) But as a first step, you should reflect on the broader history of science, which is so fraught with revolutionary leaps and paradigm shifts. These have often been made against great opposition -- with revolutionary data staring an older, unaccepting generation of scientists right in the face! Read this Appeal carefully and then reconsider your opinion about who is telling the truth and who is defending falsehood about revolutionary new prospects for science and civilization.

For almost nine years I have been the editor of *Infinite Energy*, the magazine of new energy science and technology. Though it is now small in circulation, *Infinite E*nergy is received worldwide in some forty countries. And, *Infinite Energy* is distributed to newsstands across the United States and Canada. My friend and colleague, Sir Arthur C. Clarke, has supported with words and resources some of our efforts on behalf of new energy. The research that *Infinite Energy* covers suggests that there are at least three major categories of radically new sources of energy that civilization is on the verge of being able to tap and reduce to practical technologies. These are the completely new forms of energy for which this Appeal for Support is being issued. *New Energy* is the term that we apply to new sources of energy that are currently not recognized as feasible by the "scientific establishment," but for which overwhelming and compelling evidence exists, we suggest, in at least these major categories:

Category 1. New hydrogen physics (a.k.a. "cold fusion," more generally Low-Energy Nuclear Reactions or LENR, "hydrino" physics, and other water-based energy sources. Copious technical and other information about this research may be found on these two diverse websites: www.lenr-canr.org and www.blacklightpower.com as well as our own site, www.infinite-energy.com. The upshot of this energy-from-water field is that within ordinary water there is a heretofore unimaginably large energy reservoir that may be as great as 300 gallons of gasoline energy equivalent within each gallon of plain water! This energy would be non-polluting, would have no hazardous radiation, and would, in effect, have a zero fuel cost. Only one cubic kilometer of ocean water would provide energy equivalent to all the known oil reserves on Earth. In responding to a special plea by Sir Arthur C. Clarke, the White House requested from me a technically-based Memorandum on this topic in February 2000. This 8,500-word Memorandum, "The Strange Birth of the Water Fuel Age," was submitted to the Clinton Administration and later to the Bush Administration. It is now posted on www.infinite-energy.com. It asks for a review of the substantial evidence -- in particular the copious evidence developed over the past 14 years in U.S. Federal laboratories -- for this category of anomalous new physics energy. Unfortunately, apart from polite "Thank You" notes, no discernable action has been taken by either administration. The 10th International Conference on Cold Fusion (ICCF10) was held near and at MIT in August 2003. Actual public demonstrations of excess energy production in electrolytic cells occurred at MIT's Department of Electrical Engineering and Computer Science. *Wall Street Journal* science journalist Sharon Begley attended ICCF10 and wrote a fine column in the September 5, 2003 issue of *WSJ*, "Cold Fusion Isn't Dead, It's Withering From Scientific Neglect." Among other surprising technical developments at ICCF10 was the presentation by a well-funded Israeli corporation, Energetics Technologies, which appears to have made enormous strides in overcoming some of the problems with the low-energy nuclear reactions phenomenon. Isn't it time that the experimental data from this significant field of scientific work is reviewed by an unbiased panel, unlike the rush-to-judgment hostile group in 1989, which inexcusably botched that

investigation? Why aren't the many politicians who have been informed about this taking action? Are they perhaps fearful of the all-to-common "sneer review" from the Scientific Establishment?

Category 2. Vacuum energy, Zero Point Energy or "ZPE" for short, aether energy, or space energy. These are descriptions of vast energy sources from the vacuum state. Information about this most radical and paradigm-shattering physics and technology research can be found on websites: www.aetherometry.com, www.energyscience.co.uk, and www.aethera.org. In the mid-1990s, Dr. Paulo and Alexandra Correa in the Toronto area obtained three US patents on an astonishing technological device, the so-called Pulsed Abnormal Glow Discharge (PAGDTM) reactor. In its several embodiments, it already produces kilowatt-level electrical, thermal, and mechanical output power. A Quicktime video of one such device, working in 2003, may be viewed at www.aetherometry.com/cat-abrimedia.html. Successful testing of the PAGD by outside parties, including Israel Aircraft Industries (IAI) and Ontario Hydro, regrettably did not lead to commercial arrangements to further the development of this scientific wonder, which has been meticulously documented in the three United States-granted Correa patents. (Uri Soudak, former Chief Technology Officer of IAI, is still involved with the project here in the U.S.) The Correas and Dr. Harold Aspden, IBM's former chief of patent operations in Europe (from 1963 to 1983), have provided convincing theoretical explanations, based on concrete experiments with a variety of fundamental phenomena, all of which illuminate how this unsuspected vacuum state energy can be extracted by the PAGD reactor. The advent (possibly *in only 2-3 years)* of self-sustaining electrical power-generating units in the multi-kilowatt power range appears to be only a matter of gathering a relatively small amount of engineering/scientific development funding, in the low several tens of million dollars range.

Category 3. Environmental energy, *i.e.* energy from sensible thermal energy (in particular, energy of molecular motion), through significant extensions to the Second Law of Thermodynamics. The Proceedings of an important scientific conference dealing with this subject gives great insight into this work: *Quantum Limits to the Second Law: First International Conference on Quantum Limits to the Second Law* (San Diego, CA, July 28-31, 2002), Professor Daniel P. Sheehan, Editor, American Institute of Physics, Conference Proceedings, #643, 2002. A strong consensus of a significant number of the scientist attendees, as reported by the author, is that it will be possible to make utilitarian machines that convert the thermal energy in the environment to useful work, without a lower temperature reservoir to dump waste heat. This would be in direct contravention of the supposedly sacrosanct Second Law of Thermodynamics. These devices would be nearly perfect "free energy" machines. Accurate simulations of such devices have been carried out and the results published in peer-reviewed journals. Some of the authors predict that such prototype devices could be reduced to small prototype units within five years.

The foregoing brief descriptions of the three categories of New Energy identified so far is only the tip of the iceberg of the verifiable and testable information that is available on these energy sources. **It is amenable to critical and precise scientific review.** Of course, if the Scientific Establishment trusts only in its textbook theories and if disbelieving people of good will who have the means to move this work forward choose "not to look through the telescope," the consequences will be that these wondrous technologies will not be developed as rapidly as they could have been otherwise -- or they may not be developed at all! This has been and will be a monumental tragedy for virtually every category of human experience, all of which would be transformed by these now apparently "unwanted" discoveries.

I could write much more in this memorandum about the corrupt machinations within the supposedly well-ordered and ethical house of science, actions that have kept the information that *Infinite Energy* publishes from where it should be: prominently considered in such publications as *Science* and *Nature*. Don't worry, many, many *peer-reviewed* technical publications have indeed courageously published pioneering technical papers about new energy, but the prominent mainstream publications that set the boundaries of the public scientific discourse -- journals such as *Science* and *Nature* -- *reject without review* any and all papers that challenge the foundational paradigms of physics, chemistry, and biology. You may find that difficult to believe, as I would have a mere fifteen years ago when I wrote *Fire from Ice*, but it is a sad and demonstrable truth. Let us not dwell on that, however, but rather move forward together with an end-run around this grotesque, anti-scientific obstruction.

Infinite Energy Magazine has been published bi-monthly since March 1995 and I have been its Editor-in-Chief and Publisher since that time. It is a technical magazine with editorial outreach to the general public as well. Many of its articles are very accessible to laypeople and non-specialists. You may download *for free* some 117 pages of representative sample articles, which we have gathered together for you at www.infinite-energy.com. Other key articles

are posted for free downloading on our website on a continuing basis. To maintain the highest editorial standards, *Infinite Energy* is written and edited by scientists, engineers, and expert journalists. It is aimed at pioneering scientists, engineers, business people, environmentalists, philanthropists, and investors who are concerned about an exciting R&D area that we believe will change the world dramatically.

New Energy Foundation, Inc. (NEF) is an IRS-approved 501(c)(3) public charity corporation, based in New Hampshire; it has a five-member board of respected citizens. (Prior to July 2003, *Infinite Energy* had operated under a for-profit corporation.) NEF also has a research grant-awarding function, which was initiated in 2003. NEF dispenses to outside researchers and developers carefully targeted research and development funding grants from its reserves of charitable contributions. These funds are beginning to grow, but are nowhere near the level they need to be.

The current subscription price and newsstand price of *Infinite Energy* provides less than 30% of what it costs to carry on a publication of this quality at the frontiers of knowledge -- and for which no significant advertising base yet exists. And this frontier knowledge is neglected (and not infrequently mocked) by most of the scientific and media establishments. Therefore, charitable contributions are needed to carry on this important information networking function. Here is the other basic motivation for NEF: It has been far too difficult (so far) to persuade venture capital to invest in new energy technology that is not quite ready yet for "prime time," so the vicious Catch 22 ("We won't invest because it is not successful already.") must be broken. We appeal to the humanitarian and charitable instincts of those in a position to invest charitably in and/or to spread the word about the most fundamental aspect of our future: The triumph of truth over falsehood on the frontiers of science -- in which the new energy field, in our view, will be the first paradigm-shattering example.

What we have today in the fiery menace of hydrocarbon fuels and its associated geopolitical nightmare is very ugly indeed. There is almost no area of human activity that would not be dramatically affected by the advent of new energy technology -- especially matters of war or peace and health and the environment. Therefore, if your review of the referenced material convinces you that this is a reality and not "pathological science," as the unrepentant critics -- who *have not studied the scientific findings objectively or at all -- would* have you believe, we hope that you will view your tax-deductible support of the New Energy Foundation as a significant investment in your future, for your loved ones and for civilization at large. Just try to imagine our world twenty or fifty years hence *without* the advent of a dramatic source of new energy such as low-energy nuclear reactions, aether energy (or Zero Point Energy/space energy/vacuum energy, if you prefer), or some other very powerful new physics energy source. It is not a pleasant picture.

What about solar power, wind power, or hydrogen fuel cells, you ask? Those are fine, and *Infinite Energy* devotes some smaller space to writing about these. But a future of abundant, clean energy has almost no chance of emerging from the well-intentioned, beneficial, but extremely limited world of wind-power, photovoltaics, hydropower, and other conventional renewables. And the so-called controlled *hot fusion* tokamak reactor program, which is lavishly funded with *billions* of dollars by governments to the exclusion of *workable* new energy science and technology, will never bring about an era of clean abundant energy from the heavy hydrogen in water. Conventional hydrogen fuel cells, which are widely discussed by the news media today, rely on the conventionally understood energy from hydrogen when it combines with oxygen to form water. *This is thousands to millions of times less powerful per gram of hydrogen than already demonstrated new energy sources!* Furthermore, the hydrogen for conventional fuel cells must come from some other energy source that must be used to break down abundant *water* to get hydrogen fuel (if we reject the other hydrogen source: *hydrocarbon fuel*). But in all *conventional* hydrogen fuel processes using water as the starting material, this requires more energy than one gets back when the hydrogen is consumed. So ordinary "hydrogen power" is a misnomer at best -- it is no solution at all to the world's real energy needs. Hydrogen, conventionally employed, is *an energy storage medium* period. New Energy Foundation supports radically new forms of energy, not the relatively weak examples of alternative energy within conventional renewables. We acknowledge, of course, that there are now no robust new energy devices on the market -- not yet. But when adequate, well-targeted research funding is applied, a revolution in energy technology will occur that will dwarf the personal computer revolution in intensity. It will have much in common with that revolution too, since power sources will be highly distributed. The very troublesome and erratic power grid is doomed to obsolescence.

At this time, New Energy Foundation is in need of financial support from a broader community than heretofore. NEF disseminates information about potentially world-changing technologies -- about the science, technology, patents,

investment, and politics thereof; we measure and investigate new claims about new energy devices to determine whether they are sound. This latter can be tough, because there is no question that there is much bogus "free energy noise" that obscures the good research. Most important, we are now processing grant applications by scientists and inventors from around the world, so that the most promising work -- now highly under-funded, due to the very heretical nature of this work -- gets the financial support that it so much deserves. We are very demanding about these grants; we insist that the research must be headed in the direction of developing publishable scientific results and/or actual commercially useful technologies that operate on new scientific energy principles

Please help us today, either with your financial contribution -- of any size -- or by passing along this letter and our message to those who may be better able to help NEF. Whatever you or they can afford, no matter how small an amount, will be deeply appreciated -- and will be acknowledged in the pages of *Infinite Energy* (unless you or others tell us that anonymity is requested). Some day we will live in a world in which the discoveries of New Energy science will be taken for granted. No one will be able to deny the devices, processes, and science, whose validation we are struggling so hard to achieve. In some sense, we will then have succeeded in our mission and thus will have "put ourselves out of business." Those scientific publications and general media, which should have been dealing fairly with this topic all along, will then be forced to write about it and recant past inexcusable excessive skepticism. Billions of dollars in R&D money will then flow from corporations and individuals, *as should have been happening already based on what scientists have already discovered!* The huge funding for infrastructure conversion to New Energy will flow naturally from private sources, as it has in the rise of the personal computer and Internet industry. Nothing would make me happier than to have that day come. But until then, we very much need increased financial support.

We would like to reach soon our target of at least $500,000 per year in approved research funding for New Energy. That may not seem like a lot of money to do significant research, but let me assure you that even this amount -- wisely distributed to the best researchers -- could soon begin to have a dramatic catalytic effect. New energy researchers are accustomed to low budgets and are fantastically creative, unlike the wasteful government energy research programs that have demonstrably failed already. It will not be easy to obtain even this level of modest research funding -- and, of course, several millions of dollars per year would accomplish much more, but the sooner well-targeted funding reaches under-funded researchers, the more likely we are to accelerate the inevitable New Energy Revolution. Yes, we understand that there is room in parallel for corporate start-ups, and we definitely encourage that to take place. But some of the charitable grant money can help the struggling inventors and scientists to do sufficient research, so that their work can be of greater interest to corporate start-up models.

I think you would agree with me that in these often very dark times the world would benefit immensely from a realistic *hope -- followed* by on-market technology -- that a new era of abundant, clean energy resources will be dawning. Please do your best to help us make that happen. Study the hard-won information that we have brought to your attention, if you do not yet accept what I have tried to convey to you. When you have become convinced, if you are not already, please act! You may donate charitably to the efforts of New Energy researchers at www.infinite-energy.com. Please also help us to bring this critical issue to others who may be able to help. Why not satisfy your curiosity and also help New Energy Foundation by subscribing to *Infinite Energy*. Thank you in advance for joining with us now or in the very near future.

Sincerely,
Dr. Eugene F. Mallove
President, New Energy Foundation, Inc.
Editor-in-Chief, *Infinite Energy* Magazine

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116) A negative evaluation of cold fusion claims

Ludwik Kowalski (12/9/2003)
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Several days ago I received an e-mail message from Dr. Kirk Shanahan. Last summer he sent me a paper on calorimetry that was published three years ago. That paper (1), like several others, helped me realize that calorimetry can be very tricky. That is why I think that the most convincing arguments, either for or against cold fusion, are those based on products of nuclear reactions, not on the generation of excess heat. Excess heat, even when it is real, can be caused by non-nuclear processes. Production of neutrons, protons and heavier particles, on the other hand, can be due to nuclear transformations only. Thus, experimental findings of S. Jones et all (2), or A. Lipson et al (3), for example, are more convincing (about something unusual going on) than calorimetric arguments presented, for example, by E. Storms and M. McKubre.

But cold fusion was discovered via calorimetry and “excess heat” is still a major argument of those who expect CF to become a path toward new energy resources. That is why technical issues of calorimetry, raised by Dr. Shanahan, should not be ignored. I hope his paper (summarized below) will contribute to improvements of measurements, especially when excess heat is a small fraction of total heat generated in an experiment. Exchanges of e-mail messages between me and Dr. Shanahan, are worth preserving. That is the motivation for this item.

Kirk:
In looking over your web page(s) on cold fusion, I find no reference to my explanation of the effect. Why is that when I have sent you the paper? Did I miss it?

Ludwik:
My recollection is that your paper is mostly about calorimetry, and possible systematic errors, rather than about cold fusion. It is one of many papers that I saw but not wrote about, mostly because I did not feel competent in that area.

Kirk:
You are correct. It is about a systematic error in calorimetry, but the point is that that is why people see excess heat in cold fusion experiments. In a nutshell, the paper makes the point that a simple calibration constant shift is fully capable of explaining how Ed Storms got 'cold fusion' from a platinum electrode. (Note that Pt does NOT hydride, thus Ed's getting CF from it clearly indicates that hydriding is not the primary controlling factor in CF. This means that concerns about bulk loading of Pd are not primary.) I have looked back through all the cold fusion literature and find the same problems present throughout, right back to F&P's 1990 paper. With a few exceptions then, I claim all reports of excess heat are mismeasurements. I would say that is a pretty powerful statement that you should consider if you want to claim you know the field.

Ludwik:
Please send me a brief summary at a level appropriate for physics teachers.

Kirk:
OK... Whenever an experiment is calibrated, a descriptive equation with calibration coefficients is established. That equation is then used to 'translate' subsequent raw experimental data into results. However, the underlying assumption which must be true for this procedure to be correct is that the calibration equation must be applicable during the experimental time frame. My paper analyzes the expected results when this assumption is violated for a simple linear calibration equation in the specific case of excess power measurements. However, the problem is generic. Whenever an inappropriate calibration equation is employed, inappropriate results are obtained. Furthermore, there is no reason to expect any form of the calibration equation will avoid this problem. Simply put, if your experimental system is changing, it can't be calibrated.

In the paper, a set of data supplied by Dr. Edmund Storms which was used to suggest that Pt can produce 'cold fusion' is reanalyzed in light of the calibration constant shift problem. It is shown that _assuming_ no excess power produces run-specific calibration constants that statistically bracket the constants expected for a pure water value by about +/- 3%. In other words, it is implicitly suggested that the 'cold fusion' calorimeter is unstable to that level.

Thus, there is now a new proposition on the table regarding claims to have observed excess power from heavy water electrolysis cells. Namely, there seems to be some real physical/chemical process acting in the cells that causes an instability, reflected primarily through shifts in the calibration constants. In good science, when an apparently valid counterproposal is offered to explain experimental results, scientists are required to address the counterproposal, theoretically and experimentally if required.

Dr. Edmund Storms and I have had extensive communications where he has tried to rebut my explanation, but in my estimation he has never succeeded in debunking my proposal. Instead he proceeded to conduct further experimentation without figuring in the problem, and produced another set of data (presented at ICCF10) that still shows the same basic problem, albeit now with a quadratic calibration and resultant quadratic excess power curves. He has also shown that a highly localized application of heat via a laser to the cathode maximizes the error, which supports my basic physical/chemical mechanism for how a calibration constant shift might occur. I have commented extensively on this in the Usenet newsgroup sci.physics.fusion [s.p.f.], and many of Dr. Storms comments were posted there with his permission as well.

See messages:

<db28db991beae7a3563e830842cd9c29.38703@mygate.mailgate.org>
<13959d99bc79e6e0c054b4a3c6ed470c.38703@mygate.mailgate.org>

for a quick example of where I was discussing the issues with Jed Rothwell. You should do a Google search on my name as author in s.p.f. There are over 300 hits there, not counting all the replies, going back to 1995.

Ludwik:
I would also be interested in your overall views on cold fusion, in light of what you know now.

Kirk:
You can get lots of details on all these from my s.p.f. posting. In short:

- no excess heat, it's a calibration constant problem

- no transmutation, is failure to interpret data from surface analysis techniques adequately, combined with failure to recognize active leaching/deposition processes
- no He products - it's bad mass spectrometry (see papers by W. Brian Clarke, et al)
- no radiation detection by film - it's hypering and heat
- not enough tritium results available to decide, but interferences and contaminations are suspected (by me)

Ludwik:
Where are you now and what is your current level of commitment to cold fusion?

Kirk:
I work in the Hydrogen Technology Section of the Savannah River Technology Center of the Savannah River Site. . . I am a physical chemist who studies aging effects of tritium on metal hydrides and polymers, and who supports the tritium isotope separations processes as needed. Those processes use metal hydrides, including Pd, Ti, La-Ni-Al alloys, and many others. When researchers claim they have induced 'cold fusion' in such hydrides, they directly threaten my and my coworker's health and safety by implying that we could be unknowingly being irradiated and subjecting ourselves to a potential explosion/ radioactive material contamination from a leak arising from an unexpected heat excursion in our closed vessels that contain radioactive and flammable tritium gas.

I have been involved in hydrides since 1995, and I also began studying the cold fusion area at that time. I have concluded that it is in all probabilities not real. I have most definitely concluded that the scientists conducting CF research are sub-par, typically ignoring any information that is inconsistent with their predetermined conclusions, and generally not conducting adequate work to support their claims. On the other hand, many vocal skeptics of the cold fusion claims are likewise guilty. I try not to be, and I feel I can adequately defend my position, and have done so on Usenet and elsewhere.

OK, I've tried to hit you hard here, because I see that you are 'connected' with the CFers. I am hoping you are a good scientist and teacher, and will take the time to assess my claims, which includes discussing any issues you see with me and others. You will find the CF community considers me a 'crackpot', but I assert to you that that is an unjustified attempt to marginalize what I say. 'Crackpots' don't present well-thought-out, technically defensible proposals. I hope that you will incorporate my thinking into what you teach your students, because it seems quite a shame to miss the primary lesson of cold fusion, that it is very easy to let your desires and wants override good science.

Ludwik:
Thanks Kirk: I will think about your long e-mail message received this morning (12/9/03). It helped me to understand your paper better, I think. You provided more food for thought that can possibly be squeezed into one short item.

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APPENDIX

In reading statements about excess heat I saw descriptions of cases in which excess heat was much larger than Pin. Such cases would be highly convincing that excess heat is real; no refinements described by Kirk would be necessary to justify this. What I have in mind is the episode described in Muzino's book (5), or the following piece of information posted by Eugene Mallove.

"INFINITE ENERGY Magazine of Concord, New Hampshire will sponsor an all-day Cold Fusion/New Energy Symposium on January 20, 1996 at the Cambridge Marriott Hotel in Cambridge, Massachusetts. ..... One of the many high points of the gathering will be presentation of the latest findings on the Clean Energy Technologies, Inc. (CETI) Patterson Power Cell, which recently achieved record excess power production levels for a cold fusion process -- in one test greater than 1,300 watts thermal output for about 1.4 watts DC input electrical power." I met the inventor of this cell in August 2003 and asked about the event. He smiled and did not want to talk about it. Muzino wrote that he also could not reproduce the episode described in the book. The overall situation seems to be as follows:

a) Reasonably reproducible excess heat power, but very small in comparison with Pin, has been reported by many qualified researchers. But many of them do not offer enough information to rule out possibilities of experimental errors, or to rule out a possibilities that excess heat is real but not nuclear. That seems to the main point of Kirk's messages. He keeps emphasizing that "almost all reports of observed excess heat" suffer from errors in calibration.

b) Totally unconfirmed stories of very high excess heat power, even after Pin became zero, have been told. But such stories can not be taken seriously. It is OK to be motivated by anecdotal evidence but it is not OK to accept it without serious investigations.

Shanahan is not the only qualified scientist claiming that a nuclear origin of excess heat has never been demonstrated. According to Steven Jones (4): "It is high time to strongly question claims of cold fusion based on crude techniques and to demand tests at a rigorous scientific-proof level. ..... Different detectors must give signals which agree qualitatively. . . . Otherwise, the researcher may well be chasing noise, and probably making noise as well. (Hyping questionable results in the media seems to be a characteristic practice of those claiming excess heat, which we never did.) I have not seen any compelling evidence for any "cold fusion" effects, to date."

It is clear to me that the term "cold fusion" is used by Jones as a reference to any nuclear phenomenon able to produce a measurable amount of excess heat. Low level nuclear phenomena discovered by Jones (see item 113 on my list) do not generate measurable excess heat; he would probably not call them "cold fusion." Most scientists, however, would disagree; they would say that Jones' research belongs to the field of cold fusion. Fortunately, this kind of disagreement is not important.

Let me end with an additional quote from (4); it shows a dilemma faced by all cold fusion researchers. "What should a scientist do with an anomalous experimental result (one which flies in the face of prevailing theories), which is statistically significant (at the five sigma + level), yet which cannot be repeated at will? This is the problem which plagued us early on in our non-plasma or "cold" fusion experiments which began in spring 1986 at Brigham Young University, 2.5 years before we heard of the ostensibly-related work of Drs. Pons and Fleischmann. It is a question which haunts us still. I invite the reader to seriously consider the question posed above: what would you do with such data?

If you walk away from an anomalous result, you could miss something important. Indeed, is it scientifically honest to ignore such data? One can argue that scientific instruments often show "glitches," and this is probably just one of these. Quite possibly. But what if the "glitch" is not only statistically significant, but also has interesting characteristics, such as the correct energy (2.5 MeV) for neutrons from deuteron-deuteron fusion, and the expected signal-width? What if other data runs which are individually significant only at the 2-sigma level also contribute this peak? Well, what would you do? Keep trying to find the trigger mechanism, if any? How many months would you be willing to spend/waste? Would you continue if all grants dried up for this project and if virtually everyone thought the effect was non-existent and laughable?. ...."

I think that above question can be asked about any cold fusion area, including excess heat. Perhaps conditions favoring high level of excess heat will soon be identified and practical applications will become reality. For the time being, however, cold fusion researchers should study what seems to be reproducible (even if it is not yet 100% reproducible) and learn more, like in any other area. Constructive criticism of calorimetric methodology, offered by Dr. Shanahan, seems to be valid to me; I would like to know what research scientists think about it.

References:

1) Kirk Shanahan, "A systematic error in mass flow calorimetry demonstrated," Thermochimica Acta 387 (2002) pages 95–100. This article is also downloadable from http://ww.lenr-canr.org (scroll down to the LIBRARY button to access a long list of downloadable documents. Files are organized alphabetically, by the first author.)
2) See my summaries in item 113.
3) See my summary in item # 28
4) S.E. Jones, "Chasing Anomalous Signals; The Cold Fusion Question." Accountability in Research, 2000, 8: p 55. This article is also downloadable from http://ww.lenr-canr.org (scroll down to the LIBRARY button to access a long list of downloadable documents. Files are organized

alphabetically, by the first author.)
5) H. Mizuno, “Nuclear Transmutation: The Reality of Cold Fusion,” Infinite Energy Press, Concord, New Hampshire, 1998.

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117) Exposing false claims

Ludwik Kowalski (12/11/2003)
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After posting the previous unit I felt guilty for writing about something that I did not fully understand. The main idea of Dr. Kirk Shanahan's paper is simple enough but details are not. The paper is not written for people like me; it is written for cold fusion calorimetrists. This explains why I ignored the paper when I first saw it in August. In item #116, I mostly repeated statements made by Kirk without understanding many details described in the paper. Other non-specialists might find themselves in the same situation. What follows is an essay summarizing my understanding of Kirk's views. The essay is a dialog between a student, a teacher and an engineer. The content is inspired by numerous e-mail messages received from Kirk in the last ten days; the form is inspired by Galileo Galilei.

The main point of this pedagogical exercise is to address the subject without using electrochemical terminology and concepts. I want to focus on well know aspects of error analysis, not on details which are likely to emerge in real research situations. Individual contributions are numbered to facilitate further discussion.

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1) Student:
My friend, an electrical engineer, discovered an unusual resistor. It behaves normally when the current is low (below 100 mA); the electric power, Pin, and the heat generation rate, Pout, are identical, as they should be. But at a larger current the Pout exceed the Pin. The engineer thinks that he has discovered a new phenomenon; some unknown exothermic process is triggered by the current in the material from which the resistor is made. Is this possible?

2) Teacher:
We do not know everything about nature; new discoveries are likely to be made anywhere and at any time. Your friend may indeed be making an important discovery but I am not willing to accept his claim on faith.

3) Student:
My friend has a calorimeter and other instruments. He said he is willing to make a demonstration. Would you be interested in seeing it?

4) Teacher:
Yes, I would.

5) Engineer (one week later):
Thanks for allowing me to use the laboratory. Everything is ready. Should we go and start collecting data?

6) Teacher:
I would prefer you to first tell us exactly what you want to do. Where is your "magic resistor?"

7) Engineer (reaching in his pocket):
Here it is. The length of this green cylinder is 7 cm and its diameter is 2 cm. Two wires are soldered to the terminals. I can lower this cylinder into water and pass an electric current through it. A thin insulating coating keeps water away from the electric circuit. I will connect this resistor to a d.c. power supply and show that V/I is essentially constant (Ohm's law), at least up to I=0.1 A. The R will be close to 160 ohms.

Next I will immerse the resistor in water and pass a current I=250 mA through it. The input power, Pin, will be calculated as V*I, where V is the measured voltage across the resistor.

8) Teacher:
How accurate are your voltmeter and ammeter?

9) Engineer:
According to manufacturer's specifications the accuracy of each instrument is 0.5%. This means that the accuracy of Pin will be 1%.

10) Teacher:
And what kind of instruments will be used to measure the Pout?

11) Engineer:
For this I have built a flow calorimeter. It is a well isolated container whose capacity is close to one liter. Water enters through the pipe at the bottom (passing through a commercial flowmeter) and exits through the pipe near the top. I will set the flow rate to about 50 cubic centimeters per minute. The temperature of entering water, T1, and the temperature of exiting water, T2, will be measured.

12) Teacher:
How accurate are the thermometers and the flowmeter?

13) Engineer:
Temperatures can be measured with an accuracy of plus or minus 0.1C. The factory-specified accuracy of the flowmeter is 1%. The container is thermally insulated; this means that practically all heat (at least 99.9%) is removed from it by water.

14) Teacher:
Your goal is to determine Pout. How will this be accomplished?

15) Engineer:
I will calculate it from the following equation: Pout=S*F*(T2-T1), where S is the known specific heat of water (4186 J/kg*C), F is the flow rate (in kg/s) and (T2-T1) is the measured difference of temperatures.

16) Teacher:
That makes sense. Will the (T2-T1) remain constant?

17) Engineer:
At the beginning T2 will be the same as T1. Then T2 will start increasing slowly while T1 will remain constant (room temperature). After about two hours T2 will stop changing and (T2-T1) will remain constant. This will indicate that in each minute heat generated and heat removed (by circulating water) are equal.

18) Teacher:
How accurate will be your determination of Pout?

18) Engineer:
The difference of temperatures will be about 10 C. Thus the accuracy of the (T2-T1) term will be slightly less than 1%. To be on the safe side I will assume it is 1%. The accuracy at which F will be measured, as I said before, will be 1%. In

other words, Pout will be known at the accuracy level of 2%.

19) Teacher (addressing students):
Let me make a comment. Our guest is using the word "accuracy;" rather than the word "precision." This is correct. The word “precision” should be used when we are referring to random errors while the word "accuracy" should be used when we are referring to systematic errors. Suppose Pout turns out to be 40 W. Then, knowing that the accuracy is 2%, we would be able to say that the "true value" of Pout can be anywhere between 39.2 and 40.8 watts. Unlike random errors, which may also be present, systematic errors can not be reduced by performing the same experiment many times to obtain the average value.

20) Engineer:
Yes, I learned about this recently. In some textbooks the terms “accuracy” and “precision” are still used interchangeably. It is easy to be confused. What else would you like to know about my apparatus?

21) Teacher:
I think we should continue discussing your data. The two numbers you provided (2% accuracy for Pout and 1% accuracy for Pin) are highly important. That is why we focused on them first. How large were the actual values of Pout and Pin?

22) Engineer:
They turned out to be 37.4 W and 36.4 W, respectively. To me it means that the difference between these two numbers, 1 W, represents the "excess power." It is the rate at which thermal energy is generated inside the calorimeter. I think that this happens through a new nuclear process. What else can it be?

23) Teacher:
I can think of many non-nuclear processes able to generate heat at the rate of 1W.
Before addressing the issue of causes, however, I would like to ask a different question. Is it possible that the difference of 1 W is nothing but an experimental error due to the limited accuracy of measurements? We know that the true value of Pout can be anywhere between 36.6 and 38.2 W. That is based on your 2% accuracy. Likewise, the true value of Pin can be anywhere between 36.0 and 36.8 W; this is based on your 1% accuracy. Thus, the difference between Pout and Pin, can be as small as -0.1 W or as large as 2.1 W. Do you understand my reservations?

24) Engineer:
I understand you very well. Instead of accepting my hypothesis (that the excess heat is real) you prefer to accept your own hypothesis (that the excess is an illusion due to ever-present systematic errors). Why should your hypothesis be taken more seriously than my hypothesis?

25) Teacher:
Because we are not in the same situation. You are claiming that something extraordinary is taking place; I am only a defender of status quo. If you are right then the Nobel Prize might be awarded to you; nothing of that kind exists to recognize my modest contribution. The burden of proof is yours, not mine. That is how science works. In our legal system the accused is presumed innocent until proven guilty; in science a radically new idea is presumed wrong until proven correct.

26) Student:
That’s what we learned in another course. A statistician starts with a “null hypothesis” stating just the opposite to what is being claimed. Then he tries to do everything reasonable to justify that hypothesis. Failure to justify the opposite constitutes the basis for acceptance.

27) Teacher:
Yes, such strategy can be used in natural sciences. But keep in mind that statistical methods have been developed to deal with random errors while we are discussing systematic errors. Statistical errors can be reduced by repeating an experiment many times, random errors can not be reduced in that way. Calibration errors are usually categorized as systematic errors. A faulty thermometer, for example, might show that the temperature of boiling water, at standard

pressure, is 110 C instead of 100 C. The strange effect (10 C of "excess temperature") will not go away when the average temperature is calculated after performing 1000 measurements.

28) Student:
But what if the effect is real? Assuming that the 10C of excess temperature is due to an error of calibration one may miss a chance of discovering something important.

29) Teacher:
Yes, such possibility does exist; the observed effect can be due to a hidden variable of some kind, for example, a cosmic event in another galaxy, or wishful thinking of a magician. Before accepting something unusual, however, a scientist should convince himself that the instrument is not faulty, for example, by measuring the temperature with other thermometers. First he would assume that the instrument is faulty then he would reject this hypothesis by showing that the instrument is not faulty. The name of the game is checking and double-checking before accepting something unusual.

30) Engineer:
So what would it take to convince you that my hypothesis is correct?

31) Teacher:
It is not just me; by announcing a radically new idea you are challenging the entire scientific community. You must do two things to be taken seriously. First repeat the experiment using more accurate instruments. What you have built would be fine if the difference between Pout and Pin were larger, for example, 10 W or more. But it is not accurate enough for your 1 W difference.

32) Engineer:
And what is the second thing that I must do for my hypothesis to be acceptable?

33) Teacher:
Once you convince yourself that the observed effect can not possibly be due to systematic errors you must clearly describe the experiment. The description, in the form of a paper, or in the form of a conference presentation, should be sufficiently detailed to allow replications of your work. You are likely to be taken seriously after your results are independently confirmed by others. The exact error analysis should be an essential part of your publication.

34) Student:
I think that claiming a discovery is more fun than defending the status quo.

35) Teacher:
That might be true. But making scientific contributions is a serious matter. Those who are engaged in it must respect established rules of acceptance. Following these rules is very different from what poets or composers do to be successful. Scientists must be objective; they should resist the temptation to have fun by making false claims. They should be guided by experimental facts and reliable theories, not by wishful thinking.

36) Engineer:
I appreciate your comments and I will try to improve the accuracy. I will borrow a more expensive flowmeter; this alone will allow me to lower the level of uncertainty by about 0.8%. I will also reduce the flow rate; this will increase (T2-T1) from about 10 degrees to about 50 degrees.

37) Student:
How will this help you?

38) Engineer:
The relative error will not change for T1 but it will be reduced for T2. The percentage error, as you probably know, is the ratio of the absolute error (0.1 C) over the value of T2. That value will be higher when the flow is reduced.

39) Teacher:
How much can you gain from making (T2-T1) larger?

40) Engineer:
Perhaps another 0.5%. In fact, I can also use more sensitive thermometers. More accurate voltmeters and ammeters are also available. Reducing the overall instrumental uncertainty from 3% to 0.3% is possible, at least in principle. Is it worth trying?

41) Teacher:
Only you can answer this question. It depends on the amount of time and money at your disposal. Personally I would first think about the so-called procedural errors. They are often responsible for unjustified claims.

42) Student:
What are procedural errors?

43) Teacher:
Procedural errors can be anything from "inexcusable blunders," such as confusing 30 with 300 (using a wrong scale) to unexpected effects of hidden variables. Let me tell you a little story. A group of students was measuring magnetic field along the axis of a wire loop positioned in the middle of a table. In doing this they discovered an unexpected asymmetry; the magnetic field on one side of the loop was different from the field on the other side. The effect was very strong and results were reproducible. The lab assistant was puzzled. It took him a while to find out that the wooden table, on which the instruments were mounted, had iron nails below the plastic cover. Procedural errors, as you can see, are systematic errors caused by hidden variables. Even experienced researchers can be victimized by such effects.

44) Engineer:
Are you telling us that experimental results can never be trusted?

45) Teacher:
Yes, a single publication is never 100% certain. But it becomes more and more trustworthy when findings are confirmed by others. In the final analysis, one may say, scientific research is a matter of consensus. I am not trying to discourage you; I am trying to make you aware of difficulties faced by scientists. Demonstrating that an effect exceeds known instrumental uncertainties is only the first step. What follows is much more difficult. The researcher should identify as many suspected hidden variables as possible and show that they are not responsible for the "new effect." The burden of proof is on the discoverer. You must convince others that the effect is not due, for example, to viscosity of flowing water, or to chemical reactions. One way to accomplish this could be to show that a common 160 ohms resistor does not generate excess heat under identical conditions.

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P.S. COMMENT:

1) Dr. Kirk Shanahan saw my essay and suggested that another participant, QC scientist, be allowed to participate in the debate. That "quality consciences" investigator would focus on several nontrivial aspects of scientific research. My preference is to have three participants and to keep things as simple as possible. But Kirk's suggestion to focus on hidden variables could not be ignored.

2)Kirk wrote :"Your dialog does focus on the simple concept of propagation of errors, but the point of 'QC Scientist' is that that is not what is relevant in the cold fusion excess heat case, based on my studies. He attempts to get the 3 people to realize there is more to life than just the random/systematic debate."

3) At one point I asked Kirk: "did I summarize your understanding of experimental errors correctly?" And the answer was: "You present the 'old school' paradigm. My efforts in this arena arise from the 'new school', so, you did fine on the

old school stuff, but unfortunately that's not what is relevant.” I learned a lot from Kirk’s messages and I hope he will write a good summary of ‘new school’s ideas in the next unit.

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A BRIEF INTRODUCTION BY LUDWIK KOWALSKI

The previous unit (#117) was revised to incorporate valuable suggestions made by Dr. Kirk Shanahan. The most important omission of the first draft was that procedural errors were not mentioned. After posting the revised essay I asked Kirk to write closing comments on what we were discusseing for nearly two weeks. Kirk thinks that procedural errors (both systematic and random) are likely to be responsible for the apparent excess heat in all cold fusion experiments. This is very serious criticism. Unfortunately, I am in no position to comment on electrochemical errors in different kinds of calorimeters. The purpose of my essay, triggered by Kirk's published paper (see item #116), was to review the most basic concepts of traditional error analysis. As far as know, the purpose of error analysis has always been to establish the level of confidence in experimental data.

118) Error analysis; new versus old?

Kirk Shanahan (12/19/2003)

Some Comments on the 'New School' Approach to Error

In Unit 117, Dr. Kowalski appended a couple of my comments to him regarding a 'new' way of thinking about error. His units focus on the classical approach using the concepts of systematic and random error. While the scientist does need to know about the extremes of a purely systematic error and a purely random error, reality is never so cooperative as to give us the extremes. A systematic error is generally considered to be an error that produces an accuracy change. Specifically that means it is noise-free, and causes your result to be shifted away from the true value. On the other hand, a random error is usually considered to not shift the result, but to just induce noise on top of the result, which is nominally at the true value. But when the scientist walks up to her real experiment, and looks at her real results, she almost never sees anything this pure. The 'new school' of error thought focuses on the real situations, and is most often observed being applied in real world situations like process control and quality control troubleshooting.

Why should we be talking about error anyway? Because it's there, like the mountain/mountain climber case? The real reason a 'new schooler' (NSer) talks about error is because practical experience indicates it is very costly. It results in bad product being manufactured that has to be discarded or reworked, which costs money. Or it results in some cases in accidents, where people can and do get hurt. So to the NSer, anyone who doesn't define their error is economically and/or scientifically unethical. (By the way, if you're a scientist, what is your product? In the most general sense, it is information.)

So what is the fundamental difference between a NSer and an OSer (old schooler)? It seems to boil down to the attitude on what to do with error. The OSer seems to want to quantify it, and classify it (as random or systematic), but then he wants to forget it. The subtle problem with the OS approach is that it allows one to think you are done when you have quantified and classified. Isn't that what scientists do, quantify and classify their results? What else is there?

The NSer likewise quantifies error (which include propagating random error through equations by the way), but then takes a different turn. She then seeks to u*nderstand*
the error. To do that, she employs more advanced techniques than is 'usual', i.e. OS. She uses factor analysis and contour

plots and statistically designed experiments and correlation plots and... In other words, she *studies* the error. You can appreciate this tends to require a lot more work. And because of that there is a prior step I skipped, namely an economic evaluation. She asks, “OK, now that I have *this* level of error, do I need to get better (lower the total error) and can I afford to do it?” So it is the economic stakes, the presumed cost/benefit ratio, that really keys in the NSer's additional work. In the cold fusion case, the potential benefits are so high that considerable effort to understand the situation is warranted. (By the way, in this situation one 'benefit' is cost avoidance, such as avoiding injury to people. The 'cost' is the actual economic cost of doing the work proposed.)

Let's talk briefly about some rules of thumb aimed at helping one make that kind of decision. In analytical chemistry (which usually underlies most scientific research and quality manufacturing) there are some reasonably clear breakpoints in method development. It is relatively easy for an analytical chemist to 'whip out' a method with a 10% error (and by that I am being deliberately imprecise, the 10% could be either 1, 2, or 3 standard deviations, but if it is 1, the following discussion will also deal with 1, likewise if it's 3). With an approximately equal work investment, the chemist can drop the error to 2-5%. Now, *maybe* with another equal time investment, he can drop it to 1%, but it is almost as likely that it will take considerably more It is nearly impossible to get much better than a 1% technique in analytical chemistry. (Remember I am talking rules of thumb here, there are always exceptions and modifications to rules of thumb.) With purely *physics* measurements you can often do a little better, specifically it's the measurement of composition that is so difficult.

So how does this apply to the cold fusion (CF) field, specifically to calorimetry? Well, when Dr. Edmund Storms presented his paper on CF from platinum electrodes, as part of that work he showed results from two kinds of cell calibrations that were approximately 3% different. Dr. Storms is a good scientist, reasonably careful and precise, and so it is entirely expected that his presented method falls in the '2-5%' bracket. That is the 'normal' bracket one ends up in when you do good quality work on your own. It takes cooperative efforts most usually to get better than that. However, his results (and others) show an anomalous result. This is where the OS/NS difference becomes radically apparent.

In the OS approach, once the error is quantified and classified, you are done. You have basically concluded there is nothing else you can do, and that is just what the method does. So when the CF calorimeters produced an apparent excess heat signal, the simple explanation of 'instrument error' had already been eliminated. Therefore (and this is correct if there is no calorimeter error) the signal must be real, and it is large enough that it can't be a typical chemistry-based heat production, it has to be nuclear.

But the NSer says; “And where is your detailed error analysis that justifies the error-free assumption?” In other words, why should anyone accept the radical new idea of a novel nuclear process? The more common result is that there IS an error present, heretofore unknown and unnoticed. This is the 'establishment' point of view, and it was developed through long-term experience. The CFer however, wants us to agree with him that his analysis of the error situation is correct.

Since the OSer hasn't done the *study* of the error, he is unable to answer the NSer's question. So the NSer starts to work. The technical basis to the NS approach at this point is the concept that every theory (in this case I am speaking of the set of mathematical equations that are used to predict what the experiment does) is oversimplified and every experiment has thousands of hidden variables that may or may not be active at any given instant. Sounds impossible to deal with doesn't it? Thousands of variables available, and all theories inadequate to explain them. Wow, what can a researcher do? The OSer throws up his hands and says “Nothing! That's what the error IS!” And he is right on the latter point. But in fact, there are approaches that have been developed to start to address this problem, and thereby to reduce error. One key tactic from the NSer toolbox is to take a long, hard look at the implicit assumptions of the method. Back it up right to the start and ask, “Was it right to apply this that way?” or “Was it correct to assume that?” And, in order to be efficient and economical, the NSer focuses initially on any obvious difficulties (these are often called 'special causes'). In the CF case, the 3% disagreement in calibration equations was a tip-off to me.

So I did the normal NSer thing, I asked what effect the 3% error would have. Now the implicit assumption used by all CF calorimetrists to date is that their calorimeter is stable over the span of their experiments. I decided to quickly test that, so I did some algebra and derived an expression for excess power signal in the case where the 'wrong' calibration coefficients were used. What I found was that the 'theory' I developed looked A LOT like the real results. Following up further led me to a whole picture of how the apparent excess heat signal could arise from mundane chemistry and

physics, no nuclear stuff required, and then I publicized my findings. The key idea then is that by working out the impact of an observed error, I was led to a mundane explanation of how a calorimeter could indicate an enormous new heat source had suddenly appeared.

Now the next thing to happen should be to consider how we can run the experiments to differentiate whether or not my proposal is really correct. It is an unfortunate fact that whenever a hidden variable is discovered, the prior experimentation was not designed to address, i.e. record or control, that variable. Thus the prior work tends to be of no value in differentiating. It can however be used to show that the prosaic explanation works in most cases. Note that word 'most'. Remember that in a given experiment, there are 'thousands' of hidden variables theoretically available. It shouldn't be surprising then that any new factor will not fit *every* case.

So to summarize, the NS approach to error is to study it and understand it. This is versus the OS approach to quantify, classify, and forget it. The NSer cares little if the error is classified, because that tends to be an after-the-fact activity. The NSer's goal is to understand the errors and their impact, and she goes to much greater lengths to define what possible hidden variables are present.

In the CF arena, the calibration constant shift 'error' seems to be both systematic and random in nature. It is systematic in the sense that for a given datum, interpreting the datum with a shifted constant will produce a fixed offset from the true value, clearly a systematic error signature. But the actual shifts observed vary, so in that respect there is an aspect of randomness. (Actually if you examine Dr. Storms' paper carefully, you will see some clear time-dependent trends, so the shift is not strictly random.) So which is it, random or systematic? But more importantly, does that really matter? Isn't it more important to decide if the apparent excess heat signal is real or mot?

Disclaimer: The opinions expressed above are expressly those of Dr. K. Shanahan. A discerning reader should expect to find considerable objections to these opinions. Otherwise what is the point of comparing and contrasting the 'old' vs. the 'new' schools?

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Ludwik:
In one of my messages I asked Kirk for a reference on “the new school” ideas about errors analysis. He mentioned Deming. I asked “who is Deming?” The reply (referring to the following URL: http://www.ricoh-usa.com/about/awards/deming_the_man.asp) was:

Kirk:
Deming represents the other half of the textbooks on 'errors' that you seemingly haven't read, which isn't surprising. Most University training doesn't get into these issues at all, let alone deeply enough. It's the industrial world that lives and dies by error reduction and tighter control. A 'special cause' or 'special causal factor' is an variance-producing causal factor that typically produces 'flyer' data, and tends to be easy to identify and fix. Also, the factor tends not to be always active like typical 'random' variables. Thus, special causes tend to convert an underlying random error distribution into a non-gaussian form. Once 'special causes' have been eliminated from the error pool, the residual tends to be more nearly randomly distributed. In other words, in the process of tweaking up a chemical process or analytical method, the special causes are the first targets. The Deming Award is Japan's highest award for quality.

Deming was born on October 14, 1900 in Sioux City Iowa. After studying engineering at the University of Wyoming, he earned a Masters Degree in mathematics and physics from the University of Colorado, and was awarded a doctorate in physics from Yale in 1928. Deming developed his philosophy of using statistics to control manufacturing while working at AT&T's Hawthorne manufacturing plant in Chicago. After World War II, as the Census Bureau's chief mathematician, Deming developed sampling techniques that drastically narrowed the margin of error. He first went to Japan in 1947 to help with the country's census. The Japan Union of Scientists and Engineers (JUSE) became keenly interested in Deming's statistical methods. At the time, the Japanese product reputation was terrible. Within four years of employing Deming's methods, Japanese industry had made a complete turnaround, and was well on the road to

producing the outstanding products available today. In this country, several major organizations, including Ford, Pontiac and the U.S. Navy have thrived by implementing Deming's methods. Deming lived and worked out of a modest Washington, D.C. home. So obsessed was Deming with eliminating waste, one of his daughters recalled he dated his eggs with a felt tip pen so that the oldest would be eaten first. He conducted his intensive four-day business seminars right up until less than two weeks before his death in December 1993, at the age of 93.

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119) Errors in unison ?

Ludwik Kowalski (12/20/2003)
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After reading final comments made by Dr. Kirk Shanahan (see Unit #118) I composed another essay, similar to that of Unit #116. The previous essay focused on systematic errors, the one shown below focuses on random errors. The main point is to illustrate Kirk's view that these two kinds of errors can not always be separated. As before, the content is inspired by numerous e-mail messages received from Kirk in the last two weeks; the form is inspired by Galileo Galilei.

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1) Student:
My friend, an chemical engineer, discovered an unusual radioactive rock. The radioactivity of that rock increases when the temperature goes up. Is this possible?

2) Teacher:
We do not know everything about nature; new discoveries are likely to be made anywhere and at any time. But I would be highly surprised if his claim turned out to be validated.

3) Student:
Why would you be surprised?

4)Teacher:
Because a large number of great scientists, including Marie Curie, looked for such phenomena, when radioactivity was discovered, and their results were always negative. That is one reason. The second reason is that such discovery would be in conflict with what we already know. A change in temperature, even as large as several thousand degrees, has no effect on what is going on in atomic nuclei. And radioactivity, as you know, is a nuclear phenomenon.

5) Student:
But didn't you tell us that unexpected discoveries are always possible? My friend is willing to make a demonstration. Would you be interested in seeing it?

6) Teacher:
Yes, I would. And I will try to open-minded. Keep in mind, however, that the burden of proof is on him. Most likely we will find an error in his presentation. But who knows, perhaps the claim of your friend is valid. If it is then everything we know about nuclear phenomena would have to be reexamined. Let us hear him.

7) Engineer (one week later):
Thanks for allowing me to use the laboratory. Everything is ready. Should we go and start collecting data?

8) Teacher:
I would prefer you to first tell us exactly what you want to do. Where is your “magic rock?”

9) Engineer (reaching in his pocket):
Here it is. Looks like a common stone but I will show you that it is radioactive.

10) Teacher:
What kind of counter are you going to use?

11) Engineer:
A Geiger counter, it is not very different from the one I saw in your lab. I will place this rock below the counter, set the timer, and start counting, for example, for several minutes or several hours.

12) Teacher:
Good. Why would you need to count for several hours instead of several minutes?

13) Engineer:
I count longer when I want better precision. Radioactivity is random, if if the number of counts is N then the standard deviation is the square root of N. The percentage error decreases when N becomes larger. Longer counting times result in larger N. The counting rate, equal to N divided by time, is proportional to the source activity.

14) Teacher:
That is right. Let me elaborate on this. Suppose we are trying to determine the activity C of a source with a counter when the background is not negligible. The source activity, if any, is expected to be A - B, where A is the number of counts recorded when the source is present and B is the background (when the sample is removed). If A is not very different from B, then even small random fluctuations in A and B will have strong effect on C=A-B. The standard deviation of the mean C (s3), the standard deviation of the mean A (s1) and the standard deviation of the mean B (s2) are related as:

s1^2 = s2^2 + s3^2

Suppose a single set of two measurements yields: A=100 and B=25. In that case s1=10, s2=5 and s3=sqr(s1^2+s2^2)=11.2. We would only be able to say that C is 75 plus or minus 11 (15% error). To increase the level of precision we could increase the counting time, for example, by the factor of one hundred. With A=10000 and B=2500 we would be able to say that C is 7500 plus or minus 112. (1.5% error). The longer we count the more reliable the result. Ernest Rutherford, who discovered atomic nuclei, is often quoted to say: “If experiment requires statistical analysis, then one should do a better experiment. ” He was probably referring to Geiger counter experiments in which N were too small.

15) Student:
Does it mean that any level of precision is possible?

16) Teacher:
Yes, but only “in principle.” This is an important point; I suspect that we will have to return to it later. Meanwhile let me say that your use of the term “precision” is appropriate. This term should not be confused with the term “accuracy.” Today we are talking about random errors and we identify precision with the standard deviation of the mean, s. Mathematicians do tell us the s approaches zero when N approaches infinity.

17) Engineer:
But real counters will break, sooner or later. Is that was the reason for adding the “in principal?”

18) Teacher:
Yes, this was one of the reasons. I think we are ready to hear about the actual data you collected so far. How large was your “signal”, N1, when the rock was below the counter?

19) Engineer:
It was 18043 in two hours.

20) Teacher:
How large was your background, N2, when the rock was taken away?

21) Engineer:
It was 2180 in one hour.

22) Teacher (addressing students):
In terms of counts per minute we can say that A was 150 (plus or minus 0.7%) and B was 36 (plus or minus 2%). There is no doubt that this rock is radioactive; the difference between the signal and the noise (background) is much larger than random errors. I think it is time to see the real experiment.

23) Engineer (turning toward the apparatus):
To make sure that the geometry is the same as in my earlier experiment I will place the rock on this stand. I am setting the timer for six hours. Now I am pressing the start button. Do you hear the clicks? We are not going to sit here for six hours; the counting will stop automatically and we will continue tomorrow.

24) Teacher:
May I suggest that we stop the counting in an hour. This may already reveal something significant. Meanwhile let me explain what I would like to verify. The best way to do this is to use our own counter. Let me turn it on and place our radioactive cobalt source below it. As you can see, it counts rapidly. What I want to do is to see how the counting rate depends on the high voltage applied to the Geiger tube. Please write down these results; each refers to one minute of counting.

voltage -->	700	800	1000	1100	1200	1300	1400
counts -->	0	37	372	400	458	540	974

Next plot the number of counts against the voltage. What you see is called the “plateau curve” for our counter. It shows that the dependence of the counting rate on voltage is not very strong between about 1000 and 1300 volts. That is why we normally use the counter at 1200 volts. Ideally the curve should be flat , in that way changes in voltage would not affect the counting rate. But no counter is ideal, some counters are better than others in that respect. They also differ in terms of stability of the voltage. Ideally a voltage should remain constant but in reality it changes unpredictably.

25) Engineer:
My counter stopped counting. It displays 11707. In terms of counts per minute it is 195 (plus or minus 0.92%).

26) Teacher:
This is this not consistent with 150 (plus or minus 0.7%) that you reported before? The difference between 195 and 150 is highly significant. Would you say that bringing the rock into this room changed its radioactivity?

27) Engineer:
I think that the background in this room is higher than in my basement.

25) Teacher(after the background was measured):
As you can see the background in this room is nearly one half of it was in your basement. What else might be responsible for the discrepancy?

26) Engineer:
I do not know. What do you think?

27) Teacher:
One possibility is that the plateau curve of your counter is not much better than the curve of our counter and that the

voltage applied is not stable. Suppose the voltage drifted from 1200 to 1400 volts. Have you tested stability of your power supply?

28) Engineer:
No I haven't. I simply assumed that the voltage does not drift too much.

29) Teacher:
That is a good assumption when things seem to be normal. But you are claiming something totally new and unexpected. You should check and double-check everything. One thing I would recommend, besides measuring voltage continuously, is to determine the plateau curve before and after each serious measurement.

30) Engineer:
Thanks for constructive criticism; I agree with you. I will start another sequence of experiments.

31) Teacher:
That is good; let us know what happens. Let me use this occasion to comment on the term "in principle," used above. Mathematically it is correct to say that random errors can be reduced by as much as we want. It is only a matter of making a larger and larger number of measurements and calculating averages. This,however, is true only when systematic and procedural errors are absent. But hidden variables, such as the unknown voltage instability, should always be suspected. In your case trying to improve precision by increasing the numbers of counts would be counterproductive. It does not make sense to be preoccupied with tiny cracks in one window when another window, in the same room, is widely open. Likewise, using highly accurate ammeters or voltmeters is likely to be counterproductive in an environment dominated by random errors and by effects of hidden variables. In other words, as often emphasized by Kirk Shanahan, the issues of accuracy (systematic and procedural errors) and the issues of precision (random errors) can not always be separated.

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120) Deuterium gas in Palladium

Ludwik Kowalski (12/22/2003)
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A paper published by Chinese Sciences (Xing Zhong Li et al, in J. Phys, D: Appl.Phys. 36 p 3095 to 3097, 2003) describes an experiment in which excess heat was generated when deuterium was diffusing through palladium. I heard about similar result from an American cold fusion researcher, Glen Schmidt from Albuquerque. I visited Glen's metallurgical laboratory in October 2003 and spent several hours with him. His apparatus was very impressive. My plans of working with Glen did not materialize but he sent me several messages describing the results. My question, at the the time was: "can the excess heat be due to the Joule-Thomson effect?" Professor Li also asked this question at the end of the article. And he answered it negatively, at least for his high pressure and high temperature system.

The Chinese measurements were performed by a calorimeter able to detect heat generation rates as low as one microwatt. It is hard to make sense of what is displayed in Figure 2 without knowing the details. Why was the heating power nearly zero when the temperature was higher than 150 degrees? The heating rate, at the level of about 1 mW, was sustained for about 1.5 hours; it became zero when the temperature of the vessel decreased to 140 degrees. The issue of possible systematic, random and procedural errors was not addressed in the short article. Influenced by Kirk Shanahan (see units #116 to #119) I was looking for a discussion of errors. Why not a single word was written about the calorimeter calibration? I also noticed that the phrase "cold fusion" was not mentioned neither in the article nor in the list of references. I suppose that this was done to make sure that the article is not rejected. The main point of the publication was the correlation between the flow rate of deuterium and the excess power attributed to an unspecified exothermic process. The title of the article, "Correlation between abnormal deuterium flux and heat flow in a D/Pd system," emphasizes this point.

The library at http://www.lenr-canr.org contains 22 papers authored by Dr. Li, including the paper he presented at the 10th International Cold Fusion Conference. Dr. Li is a veteran of cold fusion research. His conference paper begins with this summary: "Great progress has been made after 14 year of experiments with the gas-loading D/Pd system. 6 watts of "excess heat" were generated in a gas-loaded D/Pd system for 9 hours continuously. This experiment has been repeated 6 times already in various configurations. The "excess power" density in the Pd disk is more than 100 W per cubic centimeter, which is about the power density in a fuel rod of a thermal neutron fission reactor."

The best argument, as far as I can determine, that the observed effect is real is a demonstration that heat generated in palladium is much higher than heat generated in copper, under identical conditions. Unlike palladium, copper does not allow hydrogen to diffuse through it and no excess heat is generated in copper. This observation should rule out many parasitic effects, such calibration shift etc. At the end of his paper Dr. Li thanks Glenn Schmidt for fruitful cooperation. Another very convincing fact (see figure 6) was that the excess heat generation rate was comparable to the rate at which heat was supplied to keep the membrane hot. In other words the difference between the Pout and Pin was no longer a very small fraction of Pin, as in electrochemical experiments. In the middle of the run, for example, Pin was 25 W while the excess power was 7 W. In the last hour of the run the percentage of the excess heating power over the Pin was close to 50%. This is much higher than 3%, reported by E. Storms, and criticized by Kirk Shanahan (see my units #116, and #118).

At the very beginning of his cold fusion conference presentation Dr. Li said that gas loading of Pd has four advantages: "safety, sensitivity, low cost, and a higher operating temperature." Four advantages with respect of what? Most likely with respect to electrochemical loading practiced by most cold fusion researchers. I do not know why the higher

percentage of excess heat is not listed as one of the advantages. And once again I see that the phrase “cold fusion” is not even mentioned in the body of his paper. Dr. Li is certainly familiar with Karabut’s work in which deuterium gas was used induce a large number of effects, including excess heat generated at the level of 10%. Why is Karabut’s work not even mentioned? Why nothing is said about attempts to observed at least some of the effects reported by Karabut? It is hard to believe that attempts to observe such effects were not made by Dr. Li, or by some of his associates. Why don’t they say anything about this? I am sorry that I did not ask this question at the conference.

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121) From Wall Street Journal

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While searching the Internet for something else I found an article about the current cold fusion situation. Written by **S. Begley** (shortly after the last cold fusion conference ended), the article was published in the Wall Street Journal (Sept. 5, 2003, page B1). The title was: "**Cold Fusion Isn't Dead, It's Just Withering from Scientific Neglect**." I think that the article is worth showing on this web site. Sharon Begley wrote:

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"Well, were here, " said physicist Peter Hagelstein to the 150 scientists at the 10th International Conference on Cold Fusion in Cambridge, Mass., last week. "Many people in the scientific community feel we should be shot." That, actually would be a big step up for the beleaguered community of cold fusioneers. It has been 14 years since two little-known electrochemists announced, at the infamous news conference on March 23, 1989, what sounded like the biggest physics breakthrough since Enrico Fermi produced a nuclear chain reaction on a squash court in Chicago. Using a tabletop setup, Stanley Pons and Martin Fleischmann, of the University of Utah, said they had induced deuterium nuclei to fuse inside metal electrodes, producing measurable quantities of heat. (Deuterium, aka heavy hydrogen, has one proton and one neutron in its nucleus.)

Although nuclear fusion is supposed to be impossible at temperatures much below those in the sun or a hydrogen bomb, the Utah duo said they had managed the feat at room temperature. That was the opening bell for one of the craziest periods in science. Cold fusion, if real, promised to solve the world's energy problems forever. (There is enough deuterium in sea water to provide electricity for millennia). Scientists around the world dropped what they were doing to try to replicate the astounding claim. Some did, most didn't. When a US Department of Energy investigation concluded in November 1989 that cold fusion was a mirage born of bungled measurements and wishful thinking, the field become a pariah.

Yet the cold fusioneers persist. In paper after paper last week, scientists reported that when a metal, usually palladium, absorbs huge amounts of deuterium into its atomic lattice, the result is more heat than plain old electrochemistry can explain, as well as particles thought to be by-products of nuclear fusion. Some of the most extensive work has been at the Naval REsearch Laboratory, whose scientists found both excess heat and a telltale sign of fusion, particles of helium-4, in dozens of experiments. And Michael McKubre of SRI International, Menlo Park, Calif., is still, after hundreds of thousands of experiment-hours and $4 million, getting more heat from his cold-fusion cells than can be explained conventionally.

Some of the most intriguing research is by physicist Steven Jones of Brigham Young University, Provo, Utah. Several years before Prof. Pons and Prof. Fleischmann, he reported low-temperature nuclear fusion, but virtually no excess heat. That made his cold fusion a big fizzle as an energy source, but much more acceptable to science. ":The question I get more than any other is 'Are you still doing this?," says Prof. Jones. "The answer is yes, and what we are seeing is very difficult to explain outside of cold fusion. The repeatability of these experiments now approaches 80%." Although he still detects no excess heat, the telltale signs of nuclear fusion "make us conclude that we are seeing new physics."

Although the persistence of the cold fusioneers makes skeptics shake their heads, proponents see it differently. "If there

were no effects and it were just experimental error," says Prof. Hagelstein, associate professor of electrical engineering and computer science at the Massachusetts Institute of Technology, "we should have figured out that by now I don't think there is any doubt about the existence of nuclear anomalies. Excess heat might be real, too." Right about here, I would cite physicists explaining why Prof. Hagelstein is wrong. But I can't. Almost no scientist outside the ostracized community listens to its claims anymore, much less critiques them. It has been years since a major physics journal published a paper on cold fusion. Prof. Hagelstein invited some of the original critics to last week's meeting; none showed.

Cold fusion today is a prime example of pathological science, but not because its adherents are delusional. Yes, it's disconcerting that many of the experiments inexplicably and unpredictably stop (and start) producing heat. But the real pathology is the breakdown of the normal channels of scientific communication, with no scientists outside the tight-knit cold-fusion tribe bothering to scrutinize its claims. "If you 'know' that cold fusion is impossible, then you don't have to pay attention to these results," says Prof. Hagelstein, an award-winning DOE physicist before being ostracized for his work in the theory of cold fusion. "The initial criticism was that people needed to do the [heat measurements] right, but now that some groups have spent millions of dollars doing just that, the critics still won't read the papers."

I, for one, would love to hear smart physicists explain why the excess heat from the deuterium-filled palladium reflects not nuclear fusion bu t the release of mechanical energy - sort of like letting go of a stretched spring. I'd love to see a smart critique of a 2002 paper by Japanese scientists, published in a Japanese physics journal that few American scientists see, describing (shades of medieval alchemists) the transmutation of elements through cold fusion. What these claims need is critical scrutiny by skeptics. That's how science normally functions. But in cold fusion, it isn't. And that's the worst pathology of all.

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122) New cold fusion data from Russia

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Last summer Karabut wrote to me that visa difficulties prevented him from participation in the 10th cold fusion conference. But his three papers were submitted and I downloaded them from the http://www.lenr-canr.org web site. I will refer (1) to these papers as KK1 (Karabut and Kolomeychenko), K2 (Karabut) and K3 (Karabut). The papers report experimental results obtained by using the glow discharge chamber already described in my Unit #13. But conditions of the discharge, and auxiliary equipment, were different in each experiment.

The KK3 paper focuses on emission of soft X-rays (1.3 to 1.8 keV) emitted from various cathodes (from Al to Pb) of the glow discharge tube (deuterium and hydrogen gas at the pressure of 10 torrs). Two kinds of X-rays were identified: coherent (laser-like) and not coherent (diffused). The authors observe that "the emission of the X-ray laser beams occurred" both during the discharge (current pulses of 100 mA) and after the current was off. They add that "the obtained results were the direct experimental proof of existing excited metastable energy levels with energy of 1.2 - 5.0 keV in the solid state cathode samples." The authors report that in some cases the intensity of emission was as high as 0.01 Gy/s (1 rad/s).

The K2 paper reports on X-rays generated in experiments in which the current pulses were as high as 500 mA. Four different gasses (H2, D2, Kr and Xe) were used as well as cathodes made from various materials (from Al to W and Pt). Some ionizing radiation, according to Karabut, was able to penetrate the walls of the chamber (5 mm of steel). He refers to it as "secondary radiation." The abstract of the paper ends with the following sentence: "It was shown that the secondary radiation consisted of fast electrons." How are these findings related to cold fusion? I do not know how to answer this question. Is it possible that laser-like X-rays are emitted in all cold fusion experiments but nobody noticed them before? I remember Alexander telling me about laser-like X-rays in Albuquerque (in October of 2002). But I was not ready to ask questions and was very skeptical.

The K3 paper also refers to emission of X-rays but the main emphasis is generation of excess heat and detection of transmutation products. The abstract of that paper ends with the following sentence: "The possible mechanism of producing the excess heat power and products of nuclear transmutation reactions in the solid medium with excited energy levels was considered." The most significant aspect of K3 paper, as far as I am concerned, is confirmation of experimental findings that were reported in the previous cold fusion conference (see item 13 on my web site). In one case the excess power was 8 W while the input power was 14 W. In most cases, however, the excess power was about 3 W while the input power was about 9 W. These results were obtained by using the flow calorimeter. In reading K3, Kirk Shanahan (see item #115 and #118), would probably say that not enough information was provided to validate the claimed excess heat.

The section devoted to products of transmutation reactions (where the term "impurities" is used by Karabut instead of the term "trace elements") seems to be a repetiton of what has been reported by him (see item #13 on my web site). Karabut lists 20 nuclides (from ^{7}Li to ^{115}In) which are produced at the rate of up to 10^{13} atoms per second.

Titles of three articles:

KK1- A.B. Karabut and S.A. Kolomeychenko: "Experimental Research into Characteristics of X-ray Emission from Solid-state Cathode Medium of High-current Glow Discharge."

K2 -- A.B. Karabut, “Experimental Research Into Secondary Penetrating Radiation When Interacting X-ray Beams of Solid Laser with Various Materials Targets.”Emission from Solid-state Cathode Medium Of High-current Glow Discharge.”

K3 -- A.B. Karabut, “ Production Of excess Heat, Impurity Elements And Unnatural Isotopic ratios Formed At Excited Long-Lived Atomic Levels With Energy Of More Than 1 keV In A Solid Cathode Medium During High Current Glow Discharge.”

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123 This is real alchemy

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Attempts to change one chemical element into another are usually referred to as alchemy. Such transformations are said to be impossible unless nuclear reactions are involved. A nuclear fusion of two deuterons ($^2H + ^2H$), for example, nearly always results in production of either ^3He or ^3H. Very rarely, once in a million fusion events, the reaction results in the production of common helium, ^4He. Turning atoms of one element into atoms of another element, such as ^2H-->^3He, by means of nuclear reactions, is usually called transmutation. Atomic nuclei repel each other and for that reason they do not fuse spontaneously at temperatures below tens of millions of degrees.

Accumulation of ^4He, and other atoms, in the electrodes of cold fusion cells, were first reported in 1991 by J. Bockris (1). Here is an interesting quote from this fascinating reference. At first, " the proposition that one could carry out nuclear reactions with metals in solids in the cold ('Alchemy') would have received unhesitating rejection and ridicule. Now, eight years later, not only are scientific papers describing such phenomena available from many groups; but scientific meetings in Russia, formerly entitled Cold Fusion, have been changed to meetings on 'Transmutation'. The American Nuclear Society has hosted sessions on Low Temperature Nuclear Reactions for three years in succession."

Progressive accumulation of ^4He was soon confirmed by other investigators (2,3) but confirmation of production of heavier elements had to wait a little longer. Facing the irreproducibility of results Bockris wrote (4): "And indeed, in very small quantities, around 100 parts per million, we got four consecutive experiments which did give -- seemed to give, to all intents and purposes -- the transmutation of small amounts of lead and mercury into gold, ruthenium, osmium, etcetera. However, when we went on with this, later, we couldn't reproduce it, so we had to withdraw. But I'm fully convinced that in the four experiments that we made we were producing these small amounts. Since then, numerous people have done similar things, but usually with other metals."

Results from extensive quantitative studies of heavy products, summarized in (5), are consistent with what has been reported by other investigators (6,7,8). The most recent report in this area was presented by Iwamura, from Advanced Technology Research Center, Mitsubishi Heavy Industries, Ltd., in Japan. Addressing the 10th international CF conference (August 2003) Iwamura described a fascinating setup (9) in which cesium is turned into praseodymium and strontium is turned into molybdenum. The paper describing these experiments (10) has already been published in the prestigious Japanese Journal of Applied Physics (JJAP). Was I the only one whose first reaction, during Iwamura's conference presentation, was to think about pseudo-scientists?

Imagine a 0.1 mm membrane, mostly Pd, separating a container filled with deuterium gas from a vacuum chamber. The gas diffuses slowly through the membrane into the vacuum; there is nothing unusual in this. The surface at which the gas enters the membrane is covered with a material to be transmuted (deposited either electrolytically or by the ion injection method). Using highly sophisticated analytical tools the researchers were able to show that the amount of targeted material, such as purified Cs or Sr, decreases, while the amount of new material, such as Pr and Mo, increases at the same rate. Comparing this situation with a typical nuclear reaction setup (a target and a beam from an accelerator) the authors write: "Analysis of the depth profile of Pr indicated that a very thin surface region up to 100 angstroms was the active transmutation zone. Many experimental results showed that the quantity of Pr was proportional to the deuterium flux through the Pd complex. The cross section of transmutation of Cs into Pr can be roughly estimated at 1 barn if we consider the deuterium flux as an ultra-low-energy deuteron beam."

The view of the membrane in Figure 1 (of their paper), shows that it is essentially a 0.1 mm Pd foil coated with several alternating thin layers of CaO and Pd. The first layer seen by the D atoms, as they enter the membrane, consists of Pd; it is 400 A thin. Cs or Sr were deposited on that layer. As indicated in another figure, it took nearly 100 hours to turn all atoms of Cs (about $1.3*10^{15}$) into atoms of Pr; transformation of an equal amount of Sr into Mo took about 300 hours. The Cs --> Pr experiment was repeated two times while the Sr --> Mo experiment was performed three times; the results were reasonably reproducible. I find it highly significant that the isotopic composition of Mo, produced from Sr, is drastically different from that found in nature. This seems to rule out the possibility of contamination (redistribution of impurities).

Low energy transmutations in condensed matter, reported by Iwamura, have recently been confirmed by scientists from Osaka University (11). Here is a quote from their brief description: "As a result, we confirmed that the nuclear transmutation reaction, from ^{133}Cs to ^{141}Pr, occurred. This transmutation suggests that the mass number and the atomic number increase by 8 and 4, respectively. . . The model of multi-body resonance fusion of deuterons, proposed by A. Takahashi, can explain this mass-8-and-charge-4-increased transmutation, as follows:

(Primary reaction) : $8D \rightarrow {}^{16}O^* \rightarrow {}^{8}Be^* + {}^{8}Be^* + 95.2$ (MeV)

(Secondary reaction) : $^{133}Cs + {}^{8}Be$ (47.6 MeV) $\rightarrow {}^{141}Pr^*$ (50.47 MeV)

or $^{8}Be^* \rightarrow {}^{4}He + {}^{4}He$

If the phenomena occur according to this model then ^{4}He should also be produced. So we are trying to detect ^{4}He."

It is interesting that radioactive byproducts of presumed nuclear reactions are not mentioned in (5) or in (9). Most radioactive byproducts would be much easier to identify, in small quantities, than their stable counterparts. Their absence seems to indicate that nuclear reactions in condensed matter (presumably responsible for the reported alchemy events), are totally different from common nuclear reactions. This has already been recognized by those who investigated generation of helium. They reported that helium generated via cold fusion is mainly ^{4}He; the ^{3}H and ^{3}He atoms are produced much less frequently. The situation is dramatically different from what happens in thermonuclear reactions taking place in gasses. In these reactions the probability of the $^{2}D+{}^{2}D \rightarrow {}^{4}He$ (releasing about 24 MeV of energy) is 10^{-6} while the probabilities of reactions producing ^{3}H and ^{3}He (releasing about 3 MeV of energy) are roughly 0.5 each. How can this difference be explained? That is one of the many theoretical questions still to be answered. At present the main issue is experimental rather than theoretical. Do occasional nuclear reactions happen spontaneously in condensed matter at ordinary temperatures or not? That question, formulated thirteen years ago, must be re-addressed in the context of new information.

Appendix:
After posting the above I wanted to see if comments about the work of Japanese scientists can be found on the Internet. And I discovered, with pleasure, that the upcoming meeting of American Physical Society (APS March, 2004) has a session devoted to "dd-Fusion-Iwamura Connection." Fortunately, the free speech policy is in effect, as far as APS conferences are concerned. I wish the policy would apply to publications in leading scientific journals; publications should not be rejected on the basis of their associations with a particular field. (Is it true that editors of APS journals tend to select reviewers of cold fusion papers from pools of scientists who are known to be prejudiced against the field?)

What follows are three abstracts of papers to be presented at the meeting. I do not understand them fully because I am not a theoretical physicist. The abstracts show that some theoretical physicists are trying to understand cold fusion. Several additional CF-related abstracts can be seen at:

http://www.eps.org/aps/meet/MAR03/baps/abs/S9530.html

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1) Talbot Chubb *(Research Systems, Inc. , 5023 N. 38th St., Arlington, VA 22207)*

My conjecture: LENR dd fusion occurs in PdD_x when a subset of the interstitial deuterons occupy tetrahedral sites in a PdD_x crystallite. The tetrahedral deuterons(d's), which occupy shallow potential wells, behave as a superfluid, similar to ultracold Na atoms in shallow-well optical traps, as modeled by Jaksch et al.(D. Jaksch, et al, Phys. Rev. Lett., 81, 3108 (1998).) The tetrahedral d's form a deuteron (d) subsystem, which is neutralized by an electron subsystem containing an equal number of electrons. In the superfluid all the properties of each quasiparticle d are partitioned among N_{site} equivalent sites. The partitioning of the d point charge reduces the Coulomb self-repulsion within each quasiparticle pair, which causes wave function overlap at large N_{site} , allowing d-d fusion. Similarly, partitioning of the point charge of each single quasiparticle d reduces the Coulomb repulsion between it and an obstructing impurity atom, which causes wave function overlap between quasiparticle and atom at large N_{site}, allowing transmutation of the impurity atom. The Iwamura reaction (Y. Iwamura, et al, Japan J. of Appl. Physics, 41A, 4642 (2002).) is $4\,^2D^+_{bloch} + 4e^-_{bloch} + {}^{131}Cs \rightarrow {}^{141}Pr$, with the reaction energy incoherently transferred to the lattice.

The entire paper (the “ChubbTAtheddcolf.pdf” file) can be downloaded from the Internet (see reference 1 above).

2) Scott Chubb *(Research Systems, Inc. , 9822 Pebble Weigh Ct., Burke, VA 22015-3378)*

Three, Key, Unanswered Questions posed by LENR's are: 1. How do we explain the lack of high energy particles (HEP's)? 2. Can we understand and prioritize the way coupling can occur between nuclear- and atomic- lengthscales, and 3. What are the roles of Surface-Like (SL), as opposed to Bulk-Like (BL), processes in triggering nuclear phenomena. One important source of confusion associated with each of these questions is the common perception that the quantum mechanical phases of different particles are not correlated with each other. When the momenta p of interacting particles is large, and reactions occur rapidly (between HEP's, for example), this is a valid assumption. But when the relative difference in p becomes vanishingly small, between one charge, and many others, as a result of implicit electromagnetic coupling, each charge can share a common phase, relative to the others, modulo 2n\pi, where n is an integer, even when outside forces are introduced. The associated forms of broken gauge symmetry, distinguish BL from SL phenomena, at room temperature, also explain super- and normal- conductivity in solids, and can be used to address the Three, Key, Unanswered Questions posed by LENR's.

3) Peter L. Hagelstein *(Research Laboratory of Electronics, Massachusetts Institute of Technology, Cambridge, MA 02139)*

We have proposed that phonon exchange can occur in the presence of a highly excited optical phonon mode during a dd-fusion reaction. We have also suggested (P. L. Hagelstein, Bull. APS 45, 235 (2000)) at new second-order site-other-site reactions can occur when the energy of a fusion reaction is transferred elsewhere. Fast particle ejecta from the experiments of Chambers(G. P. Chambers, et al, J. Fusion Energy, Vol. 9, p. 281 (1990).) and of Cecil (F. E. Cecil, et al, AIP Conf. Proc. Vol. 228, p. 383 (1990).) appear to be consistent with such a mechanism, in which a dd-fusion reaction at one site is coupled to a disintegration at another site. The dominant process of this type is the null reaction in which dd-fusion is coupled to He-4 dissociation. This process can lead to compact dd-states(P. L. Hagelstein, Bull. APS 2001), and is consistent with the Kasagi experiment(J. Kasagi et al, J. Phys. Soc. Japan 64, 777 (1995).). We find that compact states near resonance with the molecular D2 states changes the radial wavefunction at small r.

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In browsing the Internet I also found an earlier paper of Iwamura at all. Presented at the 7th International Cold Fusion Conference (Canada, 1998), it shows the history of the project. The authors wrote:

“Abstract
A new type of experimental apparatus is developed to induce nuclear reactions by continuous diffusion of deuterium. Ti atoms, which cannot be explained by contamination, were detected on the surface where deuterium atoms passed through on Pd cathodes after electrolysis. A multi-layer cathode (Pd/CaO/Pd) is introduced based on an EINR (Electron Induced Nuclear Reaction) model. Excess heat generations and x-ray emissions were observed for all the cases we tried

by the multi-layer cathodes. 57Fe/56Fe ratio of Fe atoms detected on the multi-layer cathodes is anomalously larger than natural
$^{57}Fe/^{56}Fe$.

1. Introduction
Beginning in 1993, we have researched "cold fusion" phenomena to investigate it as a potential new energy source. At first, we performed gas-loading experiments and suggested that the high diffusion velocity of deuterium, in addition to a high D/Pd ratio, is an important factor for causing nuclear reactions in solids (1)-(3) The authors analyzed electrolyzed Pd samples by a variety of methods (4), and we conjectured that impurities in Pd play essential roles to induce nuclear reactions. The foregoing ideas result in the assumption that necessary conditions to induce nuclear reactions in solids are as follows:

(i) high D/Pd
(ii) enough diffusion flux of deuterium
(iii) the existence of a third element except Pd and deuterium.

A new type of experimental apparatus was developed to induce continuous diffusion under high D/Pd conditions, in which the conditions (i) and (ii) were satisfied. (5) A multi-layer cathode composed of a Pd sheet, Pd and CaO complex layer, and Pd thin layer is developed to meet the condition (iii). Ca is introduced into Pd cathode based on an Electron-Induced Nuclear Reaction (EINR) mode (6). In this paper, experimental results using the continuous diffusion apparatus with both a normal Pd sample and the multi-layer cathodes is described**."**

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5) G. Miley, et al., 2000. "Advances in Thin-Film Electrode Experiments;" Eighth International Conference on Cold Fusion. Lerici, Italy. This paper is downloadable from the library at http://www.lenr-canr.org
6) T. Mizuno, "Nuclear Transmutations: The Reality of Cold Fusion," Oak Grow Press, Concord, NH, 1998.
7) A. B. Karabut et al.; " "Nuclear product ratio for glow discharge in deuterium;" Phys. Let. A, 170, p 265, 1992.
8) T. Ohmori et al. (1996), "Iron Formation in Gold and Palladium Cathodes," J. New Energy, vol 1, no 1, pp 15-22.
9) Y. Iwamura et al. "Energy Nuclear Transmutation In Condensed Matter Induced By D2 Gas Permeation Through Pd Complexes: Correlation Between Deuterium Flux And Nuclear Products" This paper is available over the Internet (see ref 1 above).
10) Y. Iwamura et al. "Elemental Analysis of Pd Complexes: Effects of D2 gas permeation. Jpn. J. Appl. Phys. 41 (2002), pp. 4642-4648.
11) T. Higashiyama et al. "Low Energy Nuclear Transmutation In Condensed Matter Induced By D2 Gas Permeation Through Pd Complexes: Correlation Between Deuterium Flux And Nuclear Products." This paper is available over the Internet (see ref 1 above).

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The draft of the article, whose title is shown below, was posted here on February 27, 2004. The "clickable list of items" (my index.html file) referred to it as:

125) An article I want to publish (WORK IN PROGRESS)

I was still improving the draft. Then I decided to publish the article. But journals usually insist on receiving manuscripts which have not already been publish elswhere. For that reason I decided not to make my article accessible over the Internet. Attempts to publish failed and today (6/28/04) the article is going to be posted as item #152 on my web site. The item has has only three sentences below the title; it is a record for my chronologically evolving sequence. Today I will start writing the histiory of rejections. It will be a separate item showing my correspondence with editors of several journals. Each of them decided not to publish my article without telling me what is wrong with it. I think the article is an unbiased review of the main cold fusion claims. Why was it rejected? Why was it not sent to be peer reviewed?

Recent cold fusion claims: are they valid?

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What is Cold Fusion?
Cold fusion (CF) is a mixture of several claims that may or may not be related. Some of them belong to the realm of basic science while others belong to the area of patents. And some seem to be science fiction. . . .

(To see the rest go to item #152)

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126) Reactions or contamination, that is the question.

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In an earlier unit I provided a brief description of sophisticated tools often used by those who demonstrate cold fusion alchemy. Except for a mass spectrometer, used to demonstrate changes in isotopic compositions, my familiarity with these tools is very limited. They are mostly used by chemists and material scientists. In this unit I want to describe another method, called "nuclear activation analysis" (NAA), with which I am very familiar. I will do this before describing an interesting paper of an American researcher, T. O. Passell. The author concludes, by using the NAA method, that the isotopic composition of palladium powder, after it was used to generate a lot of excess heat, is not the same as it was before the powder was exposed to deuterium. He also argues that nuclear reactions other than D+D fusion might be significant contributors to excess heat.

Nuclear activation analysis
Suppose you want to verify a claim that a very small amount of gold, for example, 0.0001% of the total mass of a 5 grams coin consists of gold. The NAA method seems to be ideal to show that 5 micrograms of gold (plus or minus 0.1 or so, depending on specific limitations) is indeed present. As a preliminary step you exposed a pure gold sample to a flux of neutrons for certain amount of time. The sample becomes gamma-radioactive and you use a gamma ray spectrometer to see the characteristic peak. That is your preliminary calibration. Knowing the number of counts under the peak and the mass of the pure gold sample you can calculate what to expect, under identical irradiation conditions, from the same amount of gold in the coin. If the characteristic obtained from the coin is, for example, ten times weaker than from the calibration sample then you know that the coin contains only 0.5 micrograms of gold.

I am skipping many details that become important when the issues of the accuracy (absolute error) and precision (random error) is addressed. The important point is that, gold is a monoisotopic element and the amount of ^{197}Au is essentially the same as the total amount of that substance. Many chemical elements are mixtures of several isotopes; the relative amount of each isotope in a mixture is a well known number. That number does not change by more than a fraction of a percent when samples of different origin are compared. The isotopic composition of Pd, for example, no matter where it was found on earth, is approximately as follows:

^{102}Pd 0.96%
^{104}Pd 10.97%
^{105}Pd 22.23%
^{106}Pd 27.33%
^{108}Pd 26.71%
^{110}Pd 11.81%

Deviations from the above average percentages do exist but they are, most often, at the level of very small fractions of one percent. It turns out that some isotopes are much easier to identify, by the NAA method, than others. Unlike traditional mass spectroscopy, the NAA method is not universal, it is suitable for some isotopes and much less suitable for others. In many cases the NAA is much more sensitive (allows to determin smaller quantities) than mass spectroscopy. The interesting thing about isotopic compositions is that they are essentially the same in all minerals containing that metal, for example, in rocks from Australia, Brazil, China or Denmark. It is also the same in palladium extracted from see water or found lunar rocks. In natural palladium the number of atoms of ^{108}Pd over the number of

atoms of ^{110}Pd, for example, is always 0.442. But that ration is very different in palladium extracted, for example, from spent fuel of a nuclear reactor. Passell analyzed palladium after it was used in a cold fusion experiment. The atomic ratio, ^{108}Pd/^{110}Pd, in that palladium, turned out to be 0.477 instead of 0.442. That is a very convincing indication that some kind of nuclear reactions did occur during the experiment.

Passell's two reports

Thomas O. Passell is research-oriented scientist (Ph.D. from the University of California at Berkeley) whose most recent report has been presented at the 10th International Cold Fusion Conference, (August, 2003). That report can be downloaded from <http://www.lenr-canr.org/iccf10/iccf10.htm> His older report (8th International Conference on Cold Fusion, Italy, 2000).is also worth reading; it can be downloaded from the library at <http://www.lenr-canr.org>.

In the older report one reads that "samples of particulate Pd weighing from 5 to 16 milligrams each were sent to the University of Texas for NAA.(7). All elements susceptible to NAA at a flux of 10^{12} n/cm^2-sec under an irradiation of 3 hours were detected by means of their emitted gamma rays as detected in an intrinsic germanium crystal operated at 77°K." So much for the general methodology. The neutron flux and the irradiation time are variables which could optimized for specific isotopes.

The purpose was to study chemical composition of trace elements, not to study their isotopic compositions, as in the most recent report. This is reflected in the title of the older report: "Trace elements added to palladium by exposure to gaseous deuterium." Here is how the motivation was described by the authors of this paper. "To confirm or refute the suspicion of a possible nuclear reaction producing the excess heat, trace element changes were measured in particulate Pd exposed to very high deuterium pressures generated in the hollow core of cylindrical Pd cathodes in a series of experiments by Arata and Zhang.(l) Independently in a completely different type of experiment, Mo, Cai, Wang, Wang, and Li reported observations of increased zinc for Pd exposed to hydrogen and deuterium gas.(6) In addition they reported observing an altered ratio of Zn-64 to Zn-68 relative to that of naturally occurring zinc. They speculated that the extra zinc observed may have been produced by some nuclear process induced by the hydrogen or deuterium on the Pd metal surface, since it was unlikely that contamination by handling or experimental processes would have given zinc with an altered relative abundance of Zn-64 and Zn-68."

The authors obtained three samples of palladium powder used by Arata and Zhang to generate excess heat [see item #23 on my CF site] plus one sample removed from the powder before it was inserted into the cold fusion apparatus. The chemical compositions of the first three samples were then compared with the composition of the fourth (virgin) sample. Increases in the amount of some elements, including iridium, gold and zinc, have been observed. Before jumping to a conclusion that the elements are produced through nuclear reactions, the authors write: "The conventional explanation for such increases is the cathodic deposition of electrolyte impurities on the cathode surface. However, all these samples were protected from the electrolyte inside the gas-pressure-tight hollow core of the cylindrical cathode. The only remaining possibility of contamination would be in the process of cathode preparation. .....It is possible that the high temperatures experienced by the welded region could have vaporized various impurities in the Pd and the vapors carried down for deposition on the Pd powder. While this scenario is plausible for the relatively volatile zinc, it is less likely for gold and iridium. For example, zinc has a boiling point of 907°C, whereas iridium and gold have boiling points of 4800°C and 2600°C respectively."

Due to experimental difficulties, the authors failed to confirm that the isotopic composition of zinc in the first three samples is different from that in the last sample. They were able to measure the amount of ^{64}Zn only; the amount of ^{68}Zn could not be determined. In the discussion of the results the authors speculate that the only exothermic "nuclear reaction capable of producing Zn-64 is the fission of palladium isotopes or proton capture in impurity copper….. " After providing an argument against the second possibility they write: "We are thus left with the possibility of Pd fission, a process giving about 20 to 30 Mev per fission (10-15 Mev per fission product atom)..... The proof of a nuclear source for the increased zinc (and possibly other multi-isotope elements), will be resolved by NAA or other methods that show the isotopic abundance ratios to be significantly different from those found naturally. Otherwise, a contamination source is indicated. The small increases in iridium and gold in the active samples relative to the virgin sample find no ready explanation."

I suppose that Dr. R. Park, an expert in surface science, read the report of Arata and Zhang before writing the "woodoo science" book (published in 2000). How does he distinguish real scientists from charlatans? But let me move to the most recent paper (2003) of Passell. I know that organizers of the 10th International Cold Fusion Conference, at which that paper was presented, personally invited Dr. Park to attend. Why did he, the official spokesman for APS (American Physical Society), decline the offer? Questions of that kind are always in my mind when I read good cold fusion papers. But that is not science. I do not know whether or not cold fusion claims are valid. But I am convinced that many cold fusion researcher are not charlatans and that their recent findings should be evaluated by appointed experts. Most of us are not equipped to conduct cold fusion experiments; that is why an authoritative evaluation would benefit all science teachers.

The title of the new paper of Passell is "$^{110}Pd/^{108}Pd$ Ratios and Trace Element Changes in Particulate Palladium Exposed to Deuterium Gas." The authors found that in palladium exposed to the deuterium gas (to produce excess heat in the cold fusion experiment of Ararta and Zhang) the $^{110}Pd/^{108}Pd$ ratio was 0.477 instead on 0.422. The 8% change in the isotopic composition of Pd is very significant. Unfortunately, a possible confirmation or refutation of changes in the isotopic composition of a trace element silver is only mentioned as work in progress. Interesting information on changes in the amounts of ^{109}Ag, ^{59}Co, ^{64}Zn, ^{191}Ir and ^{197}Au is also provided by the author. Here are the last two sentences from his last paper. "Probably the greatest revelation in this work is the possibility that trace elements may be significant participants in nuclear reactions in solids such as Pd so that focusing entirely on D+D fusion is not necessarily the only path forward in understanding these phenomena. Of course it is possible that some of the variations in impurity levels is simply random differences within a non-homogeneous batch of powdered Pd. Iridium and gold may be in this category, since no easy explanation for their changes have been found."

Comments

1) The NAA method is highly reliable; it has been used for decades in many applications. But it is not a universal method, like traditional mass spectroscopy. It is highly suitable for some isotopes and not suitable for others, even when only one chemical element is involved.

2) I suppose that NAA, when applicable, is much more sensitive than mass spectroscopy. That is why it should be used in the analysis of trace elements. But for the analysis of a main component in a sample, such as palladium, mass spectroscopy would be be more desirable that NAA. Why was the indirect NAA method chosen instead of the universal method of traditional mass spectroscopy? Probably because NAA is Passell's specialty.

3) Isotopic ratios of NAA are less informative than actual amounts of substances expressed in arbitrary units. We do not know if a change in the $^{110}Pd/^{108}Pd$ is due to an increase of ^{108}Pd or in a decrease in the ^{110}Pd. Traditional mass spectrometry would clarify this issue.

4) By using the AAA method Passell observed a large increase in the amount of ^{64}Zn. Such increase should not be identified with the increase of the total amount of zinc because 64Zn is only one of several stable isotopes. The identification would only be justifiable if the isotopic composition of Zn was the same in all four samples.

5) Traditional mass spectroscopy, of very high resolution, was an essential part of the methodology of Arata and Zhang. They used it to distinguish ^4He from D_2. I am surprised by the absence of references to studies conducted by traditional mass spectroscopy. Does it mean that nobody tried to compare isotopic compositions of Pd (before and after it was used in cold fusion experiments) by traditional mass spectrometry? It is hard for me to imagine that this was not done; an examination of a bulk element, in this case palladium, is usually easier than an examination of trace elements in it.

6) A panel of experts (appointed, for example, by the National Academy of Science, to investigate cold fusion claims) would have to decide on which experiments to focus. My recommendation would be

a) Arata and Zhang excess heat experiment with the follow up mass spectroscopic analysis of the bulk Pd and trace elements. Also a detailed chemical analysis of the so-called "impurities."

b) Jones's experiments on neutrons and charged particles. (10th International Cold Fusion Conference, August, 2003).
c) Karabut's experiment in which generation of excess heat was accompanied by generation of radioactive reaction products.
d) Iwamura experiment (10th International Cold Fusion Conference, August, 2003).

Fortunately, the authors of papers in which many essential CF experiments are described are still alive. The authors should be invited to repeat the experiments in front of critical experts appointed by the Academy. How else can the unhealthy situation (that resulted from the press release in March of 1989) be resolved?

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127) Water remembers: Homeopathy does seem to be pseudoscientific

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On 1/30/04 I watched a TV fragment on homeopathy. It was a 20/20 “Give Me a Break” report by John Stossel on ABC . Those who defended homeopathy claimed that “water remembers.” The idea is to prepare a water solution of something that, in a strong dose, would produce sickness, or some other undesirable effect. A much weaker solution of the same thing causes the opposite effect, they said. For example, a person who can not fall sleep, after taking too much coffee, would fall asleep rapidly after drinking a highly diluted coffee. The claim is that a medicine can be effective even when it is diluted to a point at which the probability of finding a single molecule becomes practically zero. How can I accept this?

I have no doubt that some homemade remedies can be as effective as prescription drugs. Or that the “truly holistic” approaches are often desirable. But I can not accept the “water remembers” idea, as presented in the ABC program. Homeopathy became popular after a German doctor, Samuel Hahnemann, started promoting it two centuries ago. He was guided by the so-called law of similarities: '”That which makes you sick in large doses shall heal you in small doses.” That philosophical principle is likely to be consistent with some experimental facts (for example, vaccination) and be contradicted by others. It is not a law of nature confirmed by all available experimental facts. In my opinion those who defend the “water remembers” idea are pseudoscientists.

The only statement about homeopathic medication that I am willing to accept is that it cannot cause side effects or addiction, especially when the dilution process is carried out to the level of “nearly nothing is left.” One might say “water has an excellent memory for good things but no memory for bad things.” Hmm, what is the mechanism by which water distinguishes between what is and what is not good for a patient? Here is a brief description about how the remedies are made; I fetched it from the following Internet site:

http://www.webhealth.co.uk/Complementary_Health/Complementary_Therapies/
Complementary_Therapy_Overview/complementary_therapy_overview12.html

“ The remedies which homeopaths use come from many different sources. Most are derived from plants, but minerals, metals and some poisons which have been used medicinally for generations are also used. After initial preparation of the raw material the remedies are made by serial dilution and succession (vigorous shaking) in a solution of alcohol and water. This is done a few (three to four) times or up to many thousands of times. The liquid dilution is then used itself as a remedy or soaked into tablets or granules for convenience. The diluted remedies are described as being 'potentized', in recognition of the dynamic healing power they can stimulate.”

And here is an advertizing of “energized water;” it seems to be nothing but commercial deception and pseudoscience. “An Energy Mug to energize your water in 1-2 minutes.....Highly charged water inside the hollow of the mug transfers positive information to your drinks (water, juice, coffee, colas), giving them a positive spin and a sweeter, softer, less acidic taste. This information stays with the drink even when poured into another glass. (Do Not X-Ray Or Microwave).” What does it mean to “energize water?” What is “positive information?” In which form does is “stay with water?” What is the purpose of the last recommendation?

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Since this is a short unit let me append to it a short e-mail message about BPL, the company described in my unit #47. This message, dated November 4, 2003 and signed by Charlie Arruda, was fetched by Google.

“I have a general business comment about Blacklight Power. It's been 12+ ‘years’ now and what started as a great ‘concept’ to provide an alternate energy supply is turning into another big ‘song and dance’ story. I have stuck with the hydrino group and initially was very excited about the Blacklight Power Company and its science/business. But now I'm wondering what happened and what must being going through the minds of their poor investors.

For instance, what happened to the very promising ‘Hydrino Hydride’ battery? I remember the nice pictures of the ‘Hydrino Hydride’ compounds and thought that was very impressive to have that much pure compound so early in their initial development. It all looked like a great ‘concept’, but after several years there is neither a ‘proof of concept’ demonstration nor viable product. I'm starting to think that hydrinos, if they exist in a stable state, are possibly very hard to control/use outside the low vacuum conditions (>1torr) of the plasma ‘test’ chambers and just aren't easily commercialized in large quantities - just like the early solar cells or the current carbon nanotubes (@ $500/gram). I wonder what the energy density would be at that pressure, most likely not much to commercialize. Maybe in 20 years there will be a way to use them in marketable quantities to become viable. Maybe the new ‘Electrokinetic Microchannel Battery"’concept will prove a better alternative... or maybe not. “

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128) Screening or something else?

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Steven Jones thinks that the D+D coulomb barrier for ions embedded in metals may be lowered significantly due to the phenomenon of screening. This, he wrote to me in private, has been confirmed by a large team of German scientists [F. Raiola et al., European Physical Journal A (13:377-382, 202)]. I should read this article when I find it. It reminded me the paper of Kasagi mentioned in the unit #17. Fortunately, the www.lenr-canr.com site has a paper that Kasagi presented at the 7th International Conference on Cold Fusion. I downloaded this paper [J. Kasagi et al., "Anomalously Enhanced D(d,p)T Reaction in Pd and PdO Observed at Very-Low-Energy Bombardments."] and read it critically. Here are my comments:

1) The idea of studying the enhancement of D(d,p)T cross section at very low energies in metals is great. Unfortunately, I have no access to earlier publications of Kasagi in which procedural details are said to be described in more details. What I have is not sufficient to be convinced.

2) The experiment was performed by using a thick target (d beam is stopped). Protons were counted by using the dE-E telescope of Si detectors; the first detector was only 50 microns thin. (Detectors of such thickness were not available when I was an active researcher.) Why the observed energy spectra of protons are not shown in the paper?

3) The target was thick and a large fraction protons was certainly not detectable. That fraction was energy-dependent; the lower the beam energy the larger the fraction. Some kind of procedure must have been used to correct for this effect. But I did not see the description of their procedure. Should I rule out a possibility that the so-called "enhancement" is due to the overcorrecting?

4) As indicated by the authors, the target itself (concentration of deuterons in metal) was not stable. This was probably due to large current beams (needed to observe rare protons). Is it conceivable that the so-called "enhancement" is due to target instabilities. The authors recognize this possibility and mention another possible source of a systematic error; the assume values of dE/dx for protons and deuterons at very small energies.

5) In the resume of Kasagi's paper, presented at the 10th International Conference on Cold Fusion, I see a statement that screening alone is not sufficient to explain the enhancements. Some other phenomena, the authors say, must also be contributing.

6) I suppose that Raiola's team was familiar with papers published by Kasagi's team. It will be interesting to see what Raiola thinks about Kasagi's papers.

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What is not possible in plasma (ionized gas), some say, might be possible in condensed matter. Let me illustrate this in terms of a coulomb barrier that prevents two ions from fusing at an ordinary temperature. To produce fusion in a gas one has two well known options: to increase the temperatures to millions of degrees or to electrically accelerate one of the ions. In a solid however one might imagine some additional scenarios. One of them is screening of positive ions by negative electrons, as suggested by Jones. And another is to produce some kind of a collective effect. Can the energy

needed to overcome the coulomb barrier be supplied (to a pair of embedded ions) from atoms surrounding them? If the total number of contributing atoms is one billion, for example, then only 0.001 eV of energy per atom would provide 1 MeV needed to produce fusion. That is my naive way of understanding arguments presented in some theoretical papers. I am referring to papers presented by S, Chubb T.Chubb and P. Hegelstein at the 10th International Conference on Cold Fusion. (More recent papers of these authors are going to be presented at the March 2004 meeting of American Physical Society. The abstracts can be seen at the APS web site
http://www.eps.org/aps/meet/MAR03/baps/abs/S9530.html)

Not having an accepted theory is the major weakness of those who investigate various cold fusion (CF) fields. But arguments about models should not be confused with arguments about the validity of experimental data. It is worth recalling that no theoretical model existed when excess heat from radium was discovered by Pierre Curie. The discovery of neutrinos, on the other hand, followed the opposite path; the existence of these hard to detect particles was first predicted theoretically and then confirmed via experimental investigations. Pauli's prediction, however, was triggered by calorimetric measurements of heat released in beta decay.

The major argument of those who rejected CF claims in 1989 was the fact that these claims could not be explained in terms of the accepted model of nuclear reactions. I think that it is OK to reject a model of a physical phenomenon on the basis of a conflict with experimental data but it is not OK to reject experimental data on the basis of a conflict with the model. Physical reality is much richer than man-made models. But that is a philosophical statement. In practice experimental findings conflicting with confirmed theories, as in the case of CF, should be scrutinized very carefully before being accepted.

In reading Kasagi's paper I noticed that A. Lipson, from Russian Academy of Sciences, was one of the coauthors. Like Jones, Lipson et al. demonstrated that 3 MeV protons are emitted from metallic foils loaded with hydrogen. In their setup protons were accompanied by alpha particles. That investigation has already been described in the unit # 28. Another investigation of Lipson et al., was presented at the 10th International Conference on Cold Fusion. The major tool in that study was the glow discharge chamber of Karabut et al. (already described in the unit #13). The main question was "how does the rate of emission of charged particles depend on the applied voltage?" These particles were detected with CR-39 detectors placed inside the chamber. The paper, entitled "Enhancement of DD-reaction Accompanied by X-ray Generation in a Pulsed Low Voltage High-Current Deuterium Glow Discharge with a Ti-Cathode," can be downloaded from the ICFF10 folder at
html://www.lenr-canr.org.

Here is the abstract of that paper: "Using noiseless solid state plastic track detectors (CR-39) and Al_2O_3:C thermo-luminescent (TLD) detectors, the yields of 3.0 MeV protons (from DD-reaction) and soft X-ray photons emitted from the cathode are studied in the pulsing-periodic deuterium glow discharge with Ti-cathode at low discharge voltages (ranging of 0.8-2.5 kV) and high current density (300 – 600 mA/cm^2). Analysis of DD-proton yield versus accelerating voltages, allowed to estimate the deuteron screening potential value Us at the deuteron energy range of $0.8 < Ed < 2.45$ keV. It was found a strong DD-reaction enhancement in glow discharge (the effective screening potential Ue = 610 ±150 eV) compared to that for accelerator experiments at higher deuteron energies (Elab >2.5 keV) and lower beam current density (50- 500 microA/cm^2). X-ray measurements showed an intensive (Ix = 10^{13}-10^{14} s^{-1}-cm^{-2}) soft X-ray emission (with a mean energy of quantum Ex = 1.2-1.5 keV) directly from the Ti cathode. The X-ray yield is strongly dependent on a deuterium diffusivity in the near -- the -- surface layer of cathode."

Note that the energy range at which the D(d,p)T reaction was studied (down to 0.8 keV) is below the range explored , with traditional accelerators. Nine references to studies conducted by using accelerators are made by Lipson et. al. The authors emphasize that studies of reaction yields at very low bombarding energies are highly significant in the context of astrophysical and thermonuclear processes. Such investigations seem to bridge a gap between cold fusion and traditional nuclear physics. The authors show that at the bombarding energy of 1 keV the number of 3 MeV protons is by nine orders of magnitude higher than theoretically expected. This refers to deuterons embedded in a Ti cathode and to a theory which agrees with experimental data at higher energies. In other words, the authors show that the accepted theory is not necessarily valid at very low energies and when targeted nuclei are not free to recoil, as they are in a hot plasma. The authors argue that the observed enhancement of the D(d,p)T reaction cross section is consistent with the

assumption that the coulomb barrier is lowered by 0.61 keV, for example, due to the screening by electrons.

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But what is screening?
How can all this be made meaningful to students in an introductory physics course? The concept of the coulomb barrier has already been described in the unit #40. To explain screening I would ask students to consider the following problem. Place two deuterons on the x axis, one at the origin and another at a distance x along the positive direction. Treat the second deuteron as a probe charge and calculate the electric potential as a function of x. This gives the familiar 1/x curve. If the attractive nuclear potential is very large at x<3 F and zero at all larger distances then the coulomb barrier is the same as the value of the electric potential at x=3. It is equal to 480 kV. Now place an electron at x=-5 F. The coulomb barrier is still electric potential at the location x=3 F. But it is now due to two point charges: the deuteron at x=0 and the electron at x=-5 F. The answer is 300 kV. Repeat the exercise when x2=-2 F. This time the coulomb barrier is 192 kV. Repeat it again to see the the coulomb barrier becomes zero when x2=0 (as one would expect without any calculations).

This simple numerical exercise is sufficient to convince students that presence of an electron,somewhere outside the two deuterons, has the effect on the coulomb barrier. Closer the electron smaller the barrier. Presence of two or more elections, as one can easily verify, would further reduce the coulomb barrier. This electric effect is can be called screening. Note that the value of x=3 was chosen because the range of nuclear forces was assumed to be 3 Fermis. The coulomb barriers would be considerably lower if the range of nuclear forces were larger. For the range of 4 fermis, for example, the coulomb barriers (with x2=-5 and -2 F) would be 200 and 120 kV, respectively. Even without screening the coulomb barrier would be 360 kV (with the range of 4 F) instead of 480 kV (with the range of 3 F).

It is clear that small distances between electrons and deuterons are extremely rare in a low pressure gas (plasma). But in condensed matter average distances between atoms are much shorter and a possibility of screening can be envisaged, especially in some metals. It is well known that in metals not all electrons orbit around atomic nuclei. It is possible to speculate that clouds of electrons are formed in some regions of metallic structures, perhaps near the boundaries of microcrystals where the D ions are trapped? Billions of Ti atoms would be involved to keep them there. Conditions for forming electron configurations which lower coulomb barriers significantly are probably rare. In some places the lowering of coulomb barriers might be very small in other it can be very large. According to Lipson et al., the average coulomb barrier lowering, in Ti, is about 0.6 kV. This is a small fraction of the two-body barrier, such as 200 kV. It is counterintuitive to think that by reducing the barrier from 200 to 199.4 kV, for example, one could increase the proton emission rate by the factor of one billion, as reported in the paper. But why should my intuition be trusted in the field about which I know so little? The purpose of this essay is nothing more than to clarify the meaning of the term “screening.”

It should be clear that one-dimensional setups of charges, and an arbitrarily chosen shape of the nuclear potential (rectangular with a given range, such as 3 F or 4 F) are not reliable for calculating coulomb barriers with or without screening. But they are sufficient to explain what screening is. To estimate the height of the coulomb barrier, without screening, one should look at the energy dependence of fusion cross sections. Most nuclear physics textbooks would show such experimental data. I am now looking at the curves for the DD reactions. They tell me that the coulomb barrier is probably omewher between 100 kV and 200 kV. The yields of neutrons and protons decrease very rapidly when kinetic energies of relative motion are less than 50 keV. (That is why thermonuclear reactions in gasses are studied at extremely high temperatures.)

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Here is a message, inspired by the above, that I just posted on the Phys-L list.

1) In electrostatic you might ask students to estimate the coulomb barrier preventing positive D ions from fusing at low temperatures. Here is my suggestion. First tell students that in addition to repulsive forces the ions attract each other by very strong nuclear forces. But these forces do not obey the 1/r^2 law. They are negligibly small when r is above a distance R (called range). For x<R the attractive nuclear forces are much larger than repulsive electric forces. That would be a sufficient justification for defining the coulomb barrier, CB, as the value of the electric potential at x=R.

The rest is trivial. Assume that R=3 F, for example, and calculate the CB. Note that F is the unit of length (femtometer or 10^-15 m). My answers were 480 kV and 360 kV, at R=3 F and 4 F, respectively. Depending on your predisposition, you may or may not like linking this problem with the cold fusion controversy.

2) If you do like this idea then consider addressing the screening effect. Some scientists say that screening is possible when D ions are embedded in metals, such as Pd or Ti. Simply stated, and without trying to argue about what causes screening (local clouds of electrons at crystal's boundaries?) one can simply declare: screening consists of lowering of the coulomb barrier by nearby electrons.

To illustrate screening do the following. Place one deuteron at x=0 at treat the other deuteron, at x>0, as a probe charge. That is what I did to calculate coulomb barriers. Then place an electron at some negative value of x, for example, -2 F. The CB is now V=V1 + V2 (where V1 is the positive part due to the deuteron and negative V2 is the negative part due the electron). You will see that CB approaches zero when electron is approaching the origin, as it should be. My answer, for R=4 F, was CB=120 kV for the electron placed at x=-2 F. And nothing prevents you from introducing more than one screening electron.

Suppose a single electron at x=10 A =100000 F is replaced by a negative particle of variable charge. Assuming R=4 F, how does the magnitude of the negative charge affect the coulomb barrier? It turns out that the charge of only 250000*e is sufficient to eliminate the coulomb barrier. Here my results;

charge in 10^5*e QB in kV

2.1 0.0015
2.0 72
1.0 216
0.5 288
0.1 346
0.01 358
0.00 360 (an ideal two-body barrier)

I find them shocking. First because the distance of 10 A is about ten times larger than the distance between atoms in most metals. And second because the number of electrons needed to eliminate the barrier (for that distance) is a negligible fraction of free electrons in each cubic micron. But not being a solid state physicist I do not know how to explain the postulated clustering of free electrons.

4) Yes, I know that three or more particles would usually not be at rest on the x axis. And I know that the nuclear potential is not a rectangular well. My goal is to estimate the orders of magnitude of CB, and to illustrate the idea of screening.

5) By the way, we usually think that the so-called "free electrons" in metals are uniformly distributed, like in ionized gases. What evidence do we have for this? Yes,
I know that the physics of surfaces is very complex, even for something familiar, such as friction.

6) No, I am not trying to poison your mind with heretical pseudo science. This piece is essentially an attempt to show how a trivial electrostatic problem can be made relevant, even in an introductory course. Those who do not want to deal with the concept of screening in condensed matter might ask students "why do fusion reactions in ionized gasses occur at stellar temperatures only?" This will naturally lead to two interesting topics outside electrostatics: the QM tunneling and the Maxwellian distribution of kinetic energies of ions at various temperatures. Students love digressions but one

must make sure that the main topic is electrostatics.

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ADDED ON JULY 23, 2011

A stranger who read the unit 128 just sent me a private message. He wrote: *"Just a couple comments on the above article..... I agree that screening would seem to play a minor role and would not be able to change the Coulomb potential much in matter in which the electrons were disposed symmetrically around the nucleus. But asymmetries do occur. For instance in highly polarized matter. I've been studying dielectrics such as barium titanate which have been produced with permittivities in excess of 30,000. In such materials at high electric fields the local field on the charges in the material can be ~10TV/m. This would certainly introduce an asymmetry in the dispositions of electrons in the nucleus. It wouldn't be too difficult to set up an experiment to test this."*

What kind of experiment can be performed with barium titanite [BaTiO3]? Suppose this dielectric is used as a target bombarded by 3 MeV protons. Suppose the cross section of a known Ti(p,n) reaction is measured. Then the same is done with a metallic Ti target. If the coulomb barrier for titanium in the dielectric is really lower than for the Ti in the metal then the cross section should be target-dependent. The cross section for the dielectric target, could be several times larger for the metallic target. Naturally, one needs a well equipped nuclear laboratory.

This was posted in the private discussion list for CMNS researcher. Then I added that such result would be the first demonstration, outside the CMNS field, that structure of condensed matter can have a dramatic effect on a nuclear process. The accepted textbook wisdom is that nuclear processes are not dependent on changes taking place at the atomic level (such as Ti metal versus Ti in a dielectric). A reproducible difference in cross sections, for example, by a factor of 2 or 10, would probably produce a dramatic change in the attitude of most scientists toward our CMNS field.

One researcher referred to Kasagi experiments. Responding to this, I wrote that Kasagi's energy region was several eV. Why did I suggest 3 MeV? Because the cross section at ~3 MeV should be many orders of magnitude larger (easier to measure) than at ~10 eV. On the other hand the 3 MeV is still below the region where the dependence of the cross section on energy becomes less steep. All this is based on what I remember, not on literature. Below are my additional reflection, also posted on the CMNS list.

1) For the most common (74%) isotope, ^{48}Ti, the (p,n) reaction will not take place when the energy of protons is 3 MeV, as I suggested. Why not? Because the Q value is 4.7 MeV The needed energy should be at least 5 MeV. This still is less than the coulomb barrier (~ 7 MeV). I would not hesitate using 5.5 MeV protons.

2) The coulomb barriers are practically identical (about 7MeV) for all stable isotopes of titanium [46 (8%), 47 (6%), 48 (74%) 49 (6%) and 50 (5%)]. But the Q values are likely to be different for other isotopes.

3) Let me go back to the ^{48}Ti target. The probability of Ti+p fusion (formation of an excited ^{49}V compound nucleus) depends only on the height of the coulomb barrier. But the cross section for a particular reaction, such as the Ti(p,n), depends on how many other "output channels" are also open (energetically possible), at a given excitation energy of the compound nucleus. I am thinking about the (p,d), (p,t), (p,al), etc. The Q values of competing reactions must be known to theoretically estimate a particular cross section.

4) What is the Q value of the (p,al) reaction: 48Ti + p --> 45Sc + alpha ? It happens to be +2.6 MeV. In other words, the (p,al) reaction is exothermic; it would take place, even for 3 MeV protons, and below. Considering this fact I would measure the (p,al) cross sections, not the (p,n) cross sections, as initially suggested. And why not the (p,d) or (p,t) reaction? These reactions can also be used to show that the expected effect is real. To answer this question I would have to calculate the Q values of these reactions. Detecting and identifying energetic charge particles is likely to be easier (less costly) than detecting and identifying energetic neutrons.

5) The less demanding (lest costly) method of validation of the expected effect would be to measure the elastic

scattering cross section, ES for alpha particles, for example, at 30 or 45 degrees. The formula for the ES is well known (and experimentally verified for thin metallic targets). The ES is inversely proportional to the square of the coulomb barrier. Once the ES is measured the barrier can be calculated. Why is this method less desirable than measuring a cross section of a nuclear reaction induced by protons? Because a sufficiently strong alpha source is much less expensive than a proton accelerator. The source could be placed into a small vacuum chamber and CR-39 chips could be used to detect scattered alpha particles.

But, as the saying goes, the devil is in the details. The most difficult part, for me, would be preparation of two thin targets, metallic and dielectric. The word "thin" stands for the "thin in comparison with the range of projectiles in it." Why is this important? Because the kinetic energy of projectiles must be well defined; it appears in the ES formula. Suppose that thicknesses of two targets are known to be the same. Suppose that each target is bombarded with a collimated beam of alpha particles for one hour. Suppose the number of particles detected at 30 degrees, for a metallic target is 1000. Suppose the same is done for the dielectric target and the number of particles detected at 30 degrees is 2000, under otherwise identical conditions. That would mean the cross section for the dielectric target is twice as large as for the metallic target. It would also mean that the coulomb barrier in the dielectric environment is four times lower than in the metallic environment.

6) Yes I know that gedanken experiments are much easier than real experiments. In reality the two targets would probably have slightly different thicknesses. A correction would have to be made, to account for this. Another correction should be made to account for the presence of barium atoms and oxygens atoms in the dielectric target. These atoms would also be scattering alpha particles. To avoid a serious error (an illusion of the coulomb barrier lowering) , I would place thin layers of Ba and O atoms (in one form or another) on the metallic target, matching the mg/cm^2 of Ba and O in the dielectric target. Similar considerations would be necessary for experiments based on nuclear reactions, rather than on scattering. Yes, the devil is in the details.

ADDED ON JULY25, 2011

Relevant references can be foud in this apparently unpublished (2006?) manuscript:

http://www2.ph.ed.ac.uk/~maliotta/mypapers/kos.pdf

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129) Consumed by a Quixotic fiasco?

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In a message (2/25/04) to a friend, a retired physics teacher, I wrote: " I tried to summarized what I have learned about cold fusion in a short article that has already been submitted to a journal. They will probably reject it and I might try another journal, probably outside the US. The article addresses readers like you and I would very much appreciate your reaction to it (mostly in terms of readability). Can you spare about 30 minutes to read my article? I would be happy to send it to you as an attached file. On the other hand, I will understand your preference for not being involved. It is a highly unusual "forbidden" topic. But please do not share the article with others at this time. Your comments will remain confidential (if you wish) or I might decide to thank you for them at the end of the article (with your permission). My goal is not to argue for or against cold fusion; it is to convince leaders of our scientific establishment that another formal evaluation of the CF field is desirable. (P.S. I will retire at the end of this semester and becoming a full time CF researcher is in my mind. I am already preparing an experiment to verify the 'emission of protons' claim.")

And here is the reply; it is typical of those who have made their mind long ago and do not want to be involved. "Since you said it would take thirty minutes, I'm afraid I can't afford to read it. I am not familiar with the field, so it would take me much, much longer to read it critically. It seems to me that one can only learn more about this field by doing more and better experiments. I am familiar with the experiments done by Mike Hayden, Walter Hardy et al. [1990 reference is given] and unless a better experiment is done to invalidate that result I am satisfied that there is nothing to the original claims. Jones' muon catalyzed fusion . . . is, of course, physically sound, if not economically so. The Pons and Fleischmann stuff, however, is incompetent. I hope that you can find a better, more rewarding pursuit for your retirement. You and I have many years to go, but not as many as we've already had. I would not like to see your "declining years" consumed by a Quixotic fiasco. .....I wish you well in whatever you do, but I respectfully suggest that you pick a topic with better a priori prospects than 'cold fusion' for your retirement efforts."

Replying to the above I wrote: "I will not bother you with my paper. Cold fusion refers to several claims which may and may not be connected. Some are definitely wrong, as I convinced myself last year by repeating an experiment (together with H.F. who made the original claim several years ago and who invited me to his private lab).

I want to focus on a recent claim made by a highly respected scientist. A good experiment should always produce an answer, either YES or NO, for a well formulated question. Being able to produce a clear answer about that particular claim will make me happy, no matter what the answer will be. That claim, by the way, has nothing to do with excess heat or with alchemy.

Most people are not familiar with what was going on in the CF field in the last ten years; their opinion was formed on the basis of the soap opera of 1989-1999. My article is a brief summary of what was going on in the CF field recently; it was written for scientifically literate readers. It would make me very happy to see it in print. Yes, we do not have too many years left and we should be selective. I will keep this in mind in deciding how long to explore the 'unexpected protons' claim; it is one aspect of CF that I feel qualify to address. ....."

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130) Sonofusion

Ludwik Kowalski (3/3/04)
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March 3, 2004 New York Times (from the on-line editin; sent by a friend)

Experts Say New Desktop Fusion Claims Seem More Credible

By KENNETH CHANG

Scientists are again claiming they have made a Sun in a jar, offering perhaps a revolutionary energy source, and this time even some skeptics find the evidence intriguing enough to call for a closer look. Using ultrasonic vibrations to shake a jar of liquid solvent the size of a large drink cup, the scientists say, they squeezed tiny gas bubbles in the liquid so quickly and violently that temperatures reached millions of degrees and some of the hydrogen atoms in the solvent molecules fused, producing a flash of light and energy. "It can do some interesting science stuff as is," said Dr. Richard T. Lahey, a professor of engineering at Rensselaer Polytechnic Institute and an author of a paper describing the findings that will appear in the journal Physical Review E.

"Our interests are to see if we can't scale it up to something more exciting," he said. The experiment could conceivably shrink the science of fusion ? slamming hydrogen atoms together, producing heat and light ? from giant, expensive reactors to the tabletop. When this team of researchers made the same claim in an article in the journal Science two years ago, many scientists reacted with skepticism, even ridicule. But new experiments, using better detectors, offer more convincing data that the phenomenon is real. "We've addressed all the issues and now they all speak for themselves with far greater intensity than they did before," said Dr. Rusi P. Taleyarkhan, the scientist who conducted the experiments at Oak Ridge National Laboratory in Tennessee and is a professor of nuclear engineering at Purdue University.

Skepticism remains, but Dr. Lawrence A. Crum, a professor of electrical engineering at the University of Washington who was highly critical of the Science paper, said the new work was "much better" and deserved attention to determine whether the effect could be reproduced. "It's getting to the point where you can't ignore it," Dr. Crum said. For decades, physicists have dreamed of harnessing the ferocious alchemy of the Sun as a clean, limitless energy source. Most experiments have been conducted in giant, expensive reactors using magnetic fields to confine the ultrahot gases. In contrast, the new experiment, which cost less than $1 million, uses the power of sound to create energy comparable to the inside of stars. To many scientists, however, the phenomenon, nicknamed sonofusion, bears uncomfortable similarities to "cold fusion," which has now been discredited.

Sonofusion has already achieved more scientific respectability than cold fusion ever did, with two articles published in major journals. And unlike cold fusion, sonofusion is based on known science. Scientists have long observed a

phenomenon known as sonoluminescence, in which a burst of ultrasound causes a bubble in a liquid to collapse and emit a flash of light; some have speculated that the gases trapped in the collapsing bubbles could be heated to temperatures hot enough for fusion to occur.

Still, controversy enveloped the Science paper two years ago. The new research by Dr. Taleyarkhan and Dr. Lahey provides a much clearer picture of neutrons that are ejected when fusion occurs. Many scientists like Dr. Glenn Young, head of the physics division at Oak Ridge, said the experiment was solid, but still incomplete. "Neutrons are slippery little rascals," he said. "They can fool you. They can bounce and show up around corners you don't expect."

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A speculation:
In my mind sonofusion belongs to the area of hot fusion. The temperature of a rapidly compressed droplet of gas or vapor increases rapidly to a level at which some ions have enough kinetic energy (Maxwell tail) to fuse. The same is true for nuclear fusion induced by highly focused energetic laser beams of short duration. But what if further investigations will show that the dominant product of the D+D sonofusion is ^{4}He rather than ^{3}He and ^{3}H? In that case one would have to say that sonofusion is totally different from the thermonuclear fusion in hot plasma. I do not think that this will happen; a collapsing droplet is not surrounded by a lattice of positive ions. That metallic latice, according to some theoretical cold fusion people (Hegelstein, Chub and Chub), is essential to produce ^{4}He instead of lighter products. But this should not prevent one from trying to measure concentration of ^{4}He in heavy water after generating a lot of sonofusion events. Perhaps that concentration does increase significantely. This would indicate that the number of fusion events is higher than what one might infer from the number of neutrons. Another thing worth measuring is the excess heat; is it about 23 MeV per event (as some cold fusion people claim) or is it about 3 MeV (as in hot fusion)? Somebody will probably increase the rate of producing collapsing bubbles to make such difficult measurements possible.

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P.S.
In browsing the Internet I discovered the abstract of a paper of Roger Stringham, reporting on new sonofusion results. The paper will be presented at the March 2004 meeting of APS. I was not aware of this paper when the above speculation was composed. It is interesting that the paper has been placed into a session devoted to cold fusion. What a coincidence; the author is talking about measurable amounts of ^{4}He (up to concentration 100 times higher than in air) and excess heat (released without radiation). I was also not aware that the DD fusion takes place on the surface of a "foil lattice." The author states that compressed bubbles of D_2O (density of deutrons 10^{25} pec cc) are implanted into palladium before fissioning. The term “cavitationally induced fusion” is used in the abstract.

What kind of fusion is it, hot or cold? Producing excess heat without producing radiation seems to indicatative of cold fusion. The New York Times article, on the other hand, refers to emission of neutrons, as in hot fusion. Do Stringham and Lahey describe very different experiments? Stringham’s findings, if confirmed, would open a new field in the area of cold fusion. I should learn more about this and summarize the result in another unit. A popularizing article summarizint the situation in thermo-nuclear field (March 18, 2002, Business Week, in Science and Technology section) lists Stringham and Lahey as pioneers of sonofusion. Lahey was the coauthor of the Oak Ridge paper that started the new excitment about thermonuclear energy from another tabletop setup. Stringham is a cold fusion researcher; look for a description of his sonofusion work (by Eugene Mallove) at:

http://www.infinite-energy.com/iemagazine/issue42/breakingnews.html

On March 2, 2004 work of Lahey, a retired scientist, was described by Rusi in Pardue News. The article is available over the Internet at:

http://news.uns.purdue.edu/html4ever/2004/0400302.Taleyarkhan.fusion.html

APPENDIX:

Here is this article. Will Dr. Park also call it voodoo science? Sonofusion was not confirmed by two physicists from Oak Ridge one year ago.

Evidence bubbles over to support tabletop nuclear fusion device

WEST LAFAYETTE, Ind. – Researchers are reporting new evidence supporting their earlier discovery of an inexpensive "tabletop" device that uses sound waves to produce nuclear fusion reactions.
Rusi Taleyarkhan

The researchers believe the new evidence shows that "sonofusion" generates nuclear reactions by creating tiny bubbles that implode with tremendous force. Nuclear fusion reactors have historically required large, multibillion-dollar machines, but sonofusion devices might be built for a fraction of that cost.

"What we are doing, in effect, is producing nuclear emissions in a simple desktop apparatus," said Rusi Taleyarkhan, the principal investigator and a professor of nuclear engineering at Purdue University. "That really is the magnitude of the discovery – the ability to use simple mechanical force for the first time in history to initiate conditions comparable to the interior of stars."

The technology might one day, in theory, lead to a new source of clean energy. It may result in a new class of low-cost, compact detectors for security applications that use neutrons to probe the contents of suitcases; devices for research that use neutrons to analyze the molecular structures of materials; machines that cheaply manufacture new synthetic materials and efficiently produce tritium, which is used for numerous applications ranging from medical imaging to watch dials; and a new technique to study various phenomena in cosmology, including the workings of neutron stars and black holes.

Taleyarkhan led the research team while he was a full-time scientist at the Oak Ridge National Laboratory, and he is now the Arden L. Bement Jr. Professor of Nuclear Engineering at Purdue.

The new findings are being reported in a paper that will appear this month in the journal Physical Review E, published by the American Physical Society. The paper was written by Taleyarkhan; postdoctoral fellow J.S Cho at Oak Ridge Associated Universities; Colin West, a retired scientist from Oak Ridge; Richard T. Lahey Jr., the Edward E. Hood Professor of Engineering at Rensselaer Polytechnic Institute (RPI); R.C. Nigmatulin, a visiting scholar at RPI and president of the Russian Academy of Sciences' Bashkortonstan branch; and Robert C. Block, active professor emeritus in the School of Engineering at RPI and director of RPI's Gaerttner Linear Accelerator Laboratory.

The discovery was first reported in March 2002 in the journal Science.

Since then the researchers have acquired additional funding from the U.S. Defense Advanced Research Projects Agency, purchased more precise instruments and equipment to collect more accurate data, and successfully reproduced and improved upon the original experiment, Taleyarkhan said.

"A fair amount of very substantial new work was conducted, " Taleyarkhan said. "And also, this time around I made a conscious decision to involve as many individuals as possible – top scientists and physicists from around the world and experts in neutron science – to come to the lab and review our procedures and findings before we even submitted the manuscript to a journal for its own independent peer review."

The new findings were scrutinized by experts at Oak Ridge.

"There was a great deal of internal review at Oak Ridge National Laboratory before the paper was submitted to the journal for external review," said Lee L. Riedinger, deputy for science and technology at Oak Ridge. "It is clear that Rusi's new data are more significant statistically than his earlier data. This is an exciting result, even if I do not understand the origin of the effect."

Riedinger said new experiments should be conducted to check the findings and to spur further research.

The device is a clear glass canister about the height of two coffee mugs stacked on top of one another. Inside the canister is a liquid called deuterated acetone. The acetone contains a form of hydrogen called deuterium, or heavy hydrogen, which contains one proton and one neutron in its nucleus. Normal hydrogen contains only one proton in its nucleus.

The researchers expose the clear canister of liquid to pulses of neutrons every five milliseconds, or thousandths of a second, causing tiny cavities to form. At the same time, the liquid is bombarded with a specific frequency of ultrasound, which causes the cavities to form into bubbles that are about 60 nanometers – or billionths of a meter – in diameter. The bubbles then expand to a much larger size, about 6,000 microns, or millionths of a meter – large enough to be seen with the unaided eye.

"The process is analogous to stretching a slingshot from Earth to the nearest star, our sun, thereby building up a huge amount of energy when released," Taleyarkhan said.

Within nanoseconds these large bubbles contract with tremendous force, returning to roughly their original size, and release flashes of light in a well-known phenomenon known as sonoluminescence. Because the bubbles grow to such a relatively large size before they implode, their contraction causes extreme temperatures and pressures comparable to those found in the interiors of stars. Researchers estimate that temperatures inside the imploding bubbles reach 10 million degrees Celsius and pressures comparable to 1,000 million earth atmospheres at sea level.

At that point, deuterium atoms fuse together, the same way hydrogen atoms fuse in stars, releasing neutrons and energy in the process. The process also releases a type of radiation called gamma rays and a radioactive material called tritium, all of which have been recorded and measured by the team. In future versions of the experiment, the tritium produced might then be used as a fuel to drive energy-producing reactions in which it fuses with deuterium.

Whereas conventional nuclear fission reactors produce waste products that take thousands of years to decay, the waste products from fusion plants are short-lived, decaying to non-dangerous levels in a decade or two. The desktop experiment is safe because, although the reactions generate extremely high pressures and temperatures, those extreme conditions exist only in small regions of the liquid in the container – within the collapsing bubbles.

One key to the process is the large difference between the original size of the bubbles and their expanded size. Going from 60 nanometers to 6,000 microns is about 100,000 times larger, compared to the bubbles usually formed in sonoluminescence, which grow only about 10 times larger before they implode.

"This means you've got about a trillion times more energy potentially available for compression of the bubbles than you do with conventional sonoluminescence," Taleyarkhan said. "When the light flashes are emitted, it's getting extremely hot, and if your liquid has deuterium atoms compared to ordinary hydrogen atoms, the conditions are hot enough to produce nuclear fusion."

The ultrasound switches on and off about 20,000 times a second as the liquid is being bombarded by neutrons.

The researchers compared their results using normal acetone and deuterated acetone, showing no evidence of fusion in the former.

Each five-millisecond pulse of neutrons is followed by a five-millisecond gap, during which time the bubbles implode, release light and emit a surge of about 1 million neutrons per second.

In the first experiments, with the less sophisticated equipment, the team was only able to collect data during a small portion of the five-millisecond intervals between neutron pulses. The new equipment enabled the researchers to see what was happening over the entire course of the experiment.

The data clearly show surges in neutrons emitted in precise timing with the light flashes, meaning the neutron emissions are produced by the collapsing bubbles responsible for the flashes of light, Taleyarkhan said.

"We see neutrons being emitted each time the bubble is imploding with sufficient violence," Taleyarkhan said.

Fusion of deuterium atoms emits neutrons that fall within a specific energy range of 2.5 mega-electron volts or below, which was the level of energy seen in neutrons produced in the experiment. The production of tritium also can only be attributed to fusion, and it was never observed in any of the control experiments in which normal acetone was used, he said.

Whereas data from the previous experiment had roughly a one in 100 chance of being attributed to some phenomena other than nuclear fusion, the new, more precise results represent more like a one in a trillion chance of being wrong, Taleyarkhan said.

"There is only one way to produce tritium – through nuclear processes," he said.

The results also agree with mathematical theory and modeling.

Future work will focus on studying ways to scale up the device, which is needed before it could be used in practical applications, and creating portable devices that operate without the need for the expensive equipment now used to bombard the canister with pulses of neutrons.

"That takes it to the next level because then it's a standalone generator," Taleyarkhan said. "These will be little nuclear reactors by themselves that are producing neutrons and energy."

Such an advance could lead to the development of extremely accurate portable detectors that use neutrons for a wide variety of applications.

"If you have a neutron source you can detect virtually anything because neutrons interact with atomic nuclei in such a way that each material shows a clear-cut signature," Taleyarkhan said.

The technique also might be used to synthesize materials inexpensively.

"For example, carbon is turned into diamond using extreme heat and temperature over many years," Taleyarkhan said. "You wouldn't have to wait years to convert carbon to diamond. In chemistry, most reactions grow exponentially with temperature. Now we might have a way to synthesize certain chemicals that were otherwise difficult to do economically.

"Several applications in the field of medicine also appear feasible, such as tumor treatment."

Before such a system could be used as a new energy source, however, researchers must reach beyond the "break-even" point, in which more energy is released from the reaction than the amount of energy it takes to drive the reaction.

"We are not yet at break-even," Taleyarkhan said. "That would be the ultimate. I don't know if it will ever happen, but we are hopeful that it will and don't see any clear reason why not. In the future we will attempt to scale up this system and see how far we can go."

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ABSTRACT

Additional Evidence of nuclear emissions during acoustic cavitation

R.P. Taleyarkhan 1, J.S. Cho 2, C.D. West 3, R. T. Lahey 3, Jr., R.I. Nigmatulin 4, and R.C. Block 3

1Purdue University, West Lafayette, Indiana 47907, 2Oak Ridge Associated Universities, Oak Ridge, Tennessee 37830, 3Rensselaer Polytechnic Institute, Troy, New York 12180,
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Time spectra of neutron and sonoluminescence emissions were measured in cavitation experiments with chilled deuterated acetone. Statistically significant neutron and gamma ray emissions were measured with a calibrated liquid-scintillation detector, and sonoluminescence emissions were measured with a photomultiplier tube. The neutron emission energy corresponded to <2.5 MeV and had an emission rate of up to ~$4*10^5$n/s. Measurements of tritium production were also performed and these data implied a neutron emission rate due to D-D fusion which agreed with what was measured. In contrast, control experiments using normal acetone did not result in statistically significant tritium activity, or neutron or gamma ray emissions.

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131) Jones's manuscript on history of cold fusion at BYU

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Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

An explanation

On January 6, 2004 I visited Steven Jones at BYU (Brigham Young University) in Utah. Recent investigations of Jones et al. are described in units #113 and #124. The purpose of my visit was to see the instruments described in papers he presented at the 10th International Cold Fusion Conference (August 2004) and to talk about our possible cooperation. During that visit Steven mentioned his unpublished paper devoted to history of cold fusion. I said that I would be very interested in reading it. Then I added that readers of this web site, many of whom are physics teachers, might also be interested in that paper. Such a manuscript, written by a scientist whose work triggered the famous cold fusion controversy, is a historic document that should be available to the scientific community. Steven agreed and today I received that paper. It is a priviledge to post it here.

From a photo of Steven Jones taken during the 10th International Conference on Cold Fusion (October, 2003).

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BRIEF HISTORY OF COLD FUSION AT BRIGHAM YOUNG UNIVERSITY

By BYU Professors Jae Ballif, William Evenson, and Steven Jones

An outline:

I. Scientific Team

II. History

A. Scientific paper on cold fusion published March 1986 (submitted June 1985) by Jones and Van Siclen
   1. Theory proposed which presaged the Brigham Young University experiments
   2. "Piezonuclear fusion" used by Steven Jones of Brigham Young University to describe cold fusion

B. Brigham Young University Physics Colloquium 12 March 1986
   1. E. Paul Palmer suggested geophysical applications

C. Report to DOE (Department of Energy) 13 May 1986

D. Brigham Young University's experimental program
   1. Planning began in March 1986
   2. Use of Pd, Li, Al, Cu, Ni, Pt, etc. under non-equilibrium conditions, was outlined 7 April 1986. Jones' lab notebook page was notarized that day.
   3. Geophysical evidence for cold/piezonuclear fusion was sought in the scientific literature as early as April 1986
   4. Electrochemical cell built and measurements taken beginning May 1986
   5. Work on a highly sensitive, energy resolving neutron detector was begun in 1986, since neutrons of the correct energy are a signature for nuclear fusion
   6. Research papers presented on cold/piezonuclear fusion experiments in March and April 1988
   7. Experiments planned, research program set out and pursued vigorously
   8. Publishable results

E. Discussions with scientists at other institutions (1986-1988)

III. Scientific Contacts with University of Utah Researchers

A. Steven Jones reviews proposal by Pons and Fleishmann at Request of DOE 20 September 1988

B. Jones offered to cooperate with Pons and Fleishmann
   1. Pons responded with telephone call December 1988
   2. Jones offered use of neutron detector
   3. Pons and Fleishmann visited Brigham Young University laboratory, 23 February 1989
   4. Researchers agreed to work toward simultaneous publication

IV. Contacts Between University Administrators

A. Telephone discussion between President of University of Utah and Provost of Brigham Young University, 3 March 1989

B. Meeting between Brigham Young University and University of Utah administrators and principle scientists at Brigham Young University, 6 March 1989

C. Agreements from 6 March 1989 meeting
   1. Simultaneous publications
   2. Publication prior to APS meeting (May 1989) where Jones was scheduled to speak
   3. Exchange of preprints
   4. No further public comments on results of research until papers submitted

V. University of Utah Press Conference and Subsequent Events

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PIEZONUCLEAR FUSION AT BRIGHAM YOUNG UNIVERSITY

I. Scientific Team

A. Brigham Young University Faculty Members - Department of Physics and Astronomy

Steven E. Jones (PI)
E. Paul Palmer
J. Bart Czirr
Daniel L. Decker
Gary L. Jensen

B. Brigham Young University Faculty Members - Department of Chemistry

James M. Thorne

C. Brigham Young University students

Stuart F. Taylor
Rod Price
J. W. Wang
David Mince
Eugene Sheeley
Paul Dahl
Paul Banks
S. Seth Jones
David E. Jones

D. University of Arizona Faculty Members - Department of Physics

Johann Rafelski

II. History

A. Scientific paper published March 1986 (submitted June 1985)

The roots of our work regarding piezonuclear fusion are described in a scientific paper published in the Journal of Physics G: Nuclear physics, 12: 213-221. This paper was received by the journal on 12 June 1985 (over three years before we heard of Pons and Fleischmann, or of Andrei Lipson in Russia, or of their related work) and published in March 1986.

A1. Theory proposed which presages Brigham Young University experiments

The detailed mathematical framework given in the paper was worked out primarily by Dr. Clinton Van Siclen, author on the paper with Dr. Steven E. Jones. The paper discusses fusion at room temperature and how this might be enhanced by increasing the density of hydrogen isotopes. The paper discusses the transition of hydrogen to the metallic state under high pressures and other technical points. One significant concept explored in this paper is that a hypothetical particle "with mass twice that of the electron" could lead to room temperature fusion at a rate of approximately one fusion per minute per kilogram of deuterium. This is close to the actual rates observed in later experiments at Brigham Young University by Jones and colleagues, and the theoretical framework given in this early publication continues to be a possible explanation for the cold/piezonuclear fusion effect. Indeed, this paper is referred to in our later papers on the Brigham Young University experiments since it provides a theoretical, mathematical foundation for cold fusion.

A2."Piezonuclear fusion" used by Steven Jones of Brigham Young University to describe cold fusion

In addition to initiating the 1985 study, Steven Jones (one of the authors) coined the term "piezonuclear fusion" in analogy to the term "thermonuclear fusion," to indicate that the proposed approach is to induce fusion by "squeezing" the hydrogen nuclei together at near room temperatures rather than by heating them to very high temperatures. (The prefix "piezo-" comes from a Greek work meaning to squeeze or compress.) Dr. Paul Palmer used the term "cold fusion" beginning in early 1986.

B. Brigham Young University Physics Colloquium 12 March 1986

E. Paul Palmer suggested geophysical applications

The paper was published in March 1986, and on March 12, 1986 many of the concepts in the paper were described by Dr. Jones at a Colloquium of the BYU Physics Department. BYU Physics Professor Paul Palmer was present and associated these ideas with geological data on heat and helium-3 which are correlated in volcanoes and other thermal regions of the earth. Both heat and helium-3 are released in fusion reactions (proton-deuteron and deuteron-deuteron reactions). Dr. Palmer suggested that rock, lava, or crystals in the earth might help to catalyze the fusion reaction. This creative leap is recorded in Dr. Palmer's logbook, dated March 13, 1986 in some detail (copies available on request to BYU Physics Department).

C. Report to DOE 13 May 1986

Our work on cold piezonuclear fusion was reported to the DOE in the 1985-86 Annual Performance Report, dated 13 May 1986, along with three related documents: the Van Siclen/Jones paper on piezouclear fusion, a note entitled "Experiments in Cold Fusion" dated 28 March 1986 by Paul Palmer; and "Comments on Catalyzed Fusion," a note by Steven Jones dated 1 April 1986. It was at this time that Prof. Jones received permission from the DOE funding agent R Gajewiski to pursue research on this aspect of cold nuclear fusion under an already existing DOE grant to Brigham Young University for muon-catalyzed fusion research.

D. Brigham Young University's experimental program

D1. Planning began in March 1986

As a result of discussions generated by the Physics Department colloquium by Dr. Jones on March 12, 1986, an experimental program was worked out to test these new ideas. An important discussion meeting was held at BYU on April 7, 1986, involving Profs. Czirr, Jones, and Palmer of BYU, and Johann Rafelski of the University of Arizona, along with student researchers. Plans for the research were extensively developed at the meeting. Prof. Rafelski had been very active in theoretical work on piezonuclear fusion since late 1985 and strongly urged the active pursuit of this experimental effort at BYU.

D2. Use of Pd, Li, Al, Cu, Ni, Pt under non-equilibrium conditions, was outlined 7 April 1986 - notarized lab notebook page

Prof. Jones's brief notes from the April 7, 1986, meeting (available on request) record that the metals aluminum, copper, nickel, platinum, palladium (because it "absorbs hydrogen readily"), and lithium were discussed as prime candidates for the process. The importance of non-equilibrium conditions was discussed; in particular, "shocked hydrides" and "electric discharge" were considered. These notes were notarized that day by Lee R. Phillips, a notary and BYU attorney, showing the importance attached to these ideas by the physicists present.

D3. Geophysical evidence for cold fusion was sought in the scientific literature as early as April 1986

On April 13, 1986, Prof. Palmer noted in his logbook a number of fusion reactions to be studied, including the deuteron + lithium reaction. (On March 18, he had noted the high amounts of sodium and lithium in magmas; these later became ingredients in our electrolyte solution, commonly known as "Mother Earth Soup".) On April 16, he records the findings of a paper by the Russian physicists B.A. Mamyrin, L. V. Khabarin, an V. S. Yudenich [Dokl. Adad. Nauk. SSSR, 237: 1054 (1987)] in which they report excess helium-3 found in various metals. This paper was encouraging to us, but we were surprised that no follow-up work was recorded in the literature.

D4. Electrochemical cell built and measurements taken beginning May 1986

On May 22, 1986, our first electrochemical cell for "electrolytic infusion of hydrogen into metals" was built (see Prof. Palmer's logbook) and on May 23, D_2O (heavy water) was added. Using a sodium-iodide detector, we looked first for gamma rays from proton-deuteron fusion, and found on May 27 that the foreground rate when the cell operating was slightly higher than the background rate when the cell was not operating, but the result was not statistically significant.

In June, we developed another means of loading hydrogen isotopes into metals, using pressurized gases, and added a neutron detector.

D5.. Work on a highly sensitive, energy resolving neutron detector was begun in 1986, since neutrons of the correct energy are a sure indicator of nuclear fusion

Throughout the summer of 1986, work was done on the neutron detector, while different electrolytes were tried, including the addition of NaOH or H_2SO_4 to D_2O and the addition of "impurity salts" of various metals. We also tried loading the cathode with deuterium gas before beginning the electrolysis (see, e.g., 10 September 1986 entry). By September 3, 1986, we saw a foreground minus background rate of about $5x10^{-3}$ in the neuron counter, but the result was neither not consistently repeatable. However, this rate proved to be consistent with the rate obtained in later work when the neutron counter system had been dramatically improved. As the Fall 1986 school term began anew, we concluded that in order to make progress in our work we had to first improve the neutron detector. Bart Czirr and Gary Jensen continued this work. Some of the effort went into trying to find suitable hydrogen-rich, inorganic scintillator. While this work continued, Dr. Jones pursued muon-catalyzed fusion research.

D6. Student papers presented on piezonuclear fusion experiments in March and April 1988

In January, 1988, Prof. Jones organized a student research class along with Prof. Palmer and Prof. Larry Rees. Cold or piezonuclear fusion was one of the principal research topics, pursued by students Paul Dahl and Paul Banks. Both wrote term papers on the topic. On 12 March 1988, Paul Dahl presented an oral paper at the Spring Research Conference of the BYU College of Physical Sciences and Mathematics; his paper was entitled "An Experimental Investigation of Piezo-nuclear Fusion." On 25 March 1988, we prepared some deuterided metal samples, which we sent to Harmon Craig of the University of California at San Diego, for helium and tritium analysis. These examples were later sent to Al Nier of the University of Minnesota, but analysis had not been completed as of 30 March 1989 as our paper for Nature neared completion.

D7. Further experiments planned, research program set out and pursued vigorously from August 1988

In August 1988, the decision was taken by the scientific team to vigorously pursue experimental cold fusion research. In particular, Prof. Jones' logbook records that a fusion group meeting took place on 24 August 1988, and that this matter was aired. We decided that both gamma and neutron detectors would be used. Since energy applications for muon-catalyzed fusion appeared remote, we decided to place particular emphasis on our cold/piezonuclear fusion research program. This decision followed work in early August by Prof. Jones in which he outlined a paper on the subject. His intent was to include a discussion of piezonuclear fusion research in his paper, for publication in the proceedings of a muon-catalyzed fusion workshop previously held in Florida in May 1988, as recorded in his logbook entry dated 9 August 1988. (Prof. Jones is an editor of this Proceedings, which is publication no. 181 of the American Institute of Physics.) However, during a visit to Provo on 15-16 August 1988, Prof. Rafelski dissuaded Jones from including this work in his paper but rather encouraged more experimental studies first. Dr. Jones also discussed the BYU work on this fusion process and his intentions to emphasize cold/piezonuclear fusion studies with Dr. Alan Anderson and Dane Chapman in August and early September 1988. Our group has vigorously pursued its experimental cold fusion research at BYU since May 1986.

D8. Publishable results obtained in 1988-1989

In August 1988, we did gamma-ray studies, using the sodium-iodine detector easiest set up. As before, we saw only non-significant hints of gamma production in our 3 inch sodium iodide counter, so we decided to concentrate on using the neutron spectrometer, which was fully conditioned for use in late 1988. Our first studies with this spectrometer were done using titanium, palladium, tantalum, nickel, aluminum, iron, and lanthanum. We also used several methods of loading deuterium into metals, including the original electrochemical method. Thus, we performed anew the experiment which we had started in May 1986, namely electrolytic infusion of deuterium into metals, but with a much-improved neutron detector. Of these experiments, Paul Palmer records: "Steve [Jones] and Bart [Czirr] have set up experiments exactly as we did a year or so ago and looked for fusion-generated neutrons in Bart's liquid-scintillator, low-resolution spectrometer.... As in the previous work, the results were tantalizingly positive." Within a few weeks, the results had

reached a statistical significance of over five standard deviations. We also found correlations between tritium detected in Hawaii and volcanic eruptions there, in agreement with expectations that piezonuclear fusion occurs in the earth. We decided in early February to publish our results.

E. Discussions with scientists at other institutions (1986-1988)

Our work in this field has been communicated to a number of scientists outside of BYU in the 1986-1988 period, to name a few : Harmon Craig (University of California, San Diego geophysicist), Al Nier (University of Minnesota), Alan Anderson (Idaho Research Software), Gus Caffrey (Idaho National Engineering Laboratory), James Cohen, Mel Leon, Jim Bradbury, Richard Maltrud, Mike Paciotti (all of Los Alamos National Laboratory), Russell Kulsrud (Princeton Plasma Physics Laboratory), Archie Harms (McMaster University), and Mike Danos (National Bureau of Standards).

III. Scientific Contacts with University of Utah Researchers

A. Steven Jones reviews proposal by Pons and Fleishmann at request of DOE, 20 September 1988

According to Prof. Jones' logbook, he reviewed a proposal by Profs. Stanley Pons and Martin Fleischmann on 20 September 1988, entitled "The Behavior of Electrochemically Compressed Hydrogen and Deuterium." The proposal was sent to Prof. Jones by Ryszard Gajewski, director of the Division of Advanced Energy Projects of the Department of Energy. Dr. Gajewski had funded Jones' work on cold nuclear fusion (including muon-catalyzed fusion) since 1982, and his specific work on electro-fusion since May 1986. For his part, Dr. Jones has reviewed about eight to ten proposals relating to cold nuclear fusion, his primary field of research. The cover letter with the proposal said nothing about declining to review the proposal if the reviewer was doing related work. Indeed, most of the proposals which Dr. Jones is asked by the DOE to review related closely to his active research on cold nuclear fusion, including muon-catalyzed fusion. The cover letter did specify that the reviewer agrees to "use the information contained in the proposal for evaluation purposes only." This Jones accepted and acknowledges that he has abided by this agreement. The development of the project at BYU outlined above, including the use of electrochemical cells since May 1986, shows that Brigham Young University was conducting research in cold fusion, including the use of electrolytic cells and deuterium-gas loading, long before the review of the University of Utah's proposal at the request of the Department of Energy. We adhered to our on-going program in a straightforward way, despite the unfounded accusations which circulated in the media in 1989 following the press conference of Drs. Pons and Fleischmann.

B. Jones offered to cooperate with Pons and Fleischmann

Prof. Jones recommended that the University of Utah's proposal be approved, despite his unresolved reservations about the theoretical underpinnings. He also suggested to R. Gajewski that he inform Pons and Fleischmann that Jones has been doing related work on cold fusion since 1986 and that perhaps a cooperative effort between the nearby universities (BYU and University of Utah) would be desirable. Jones pointed out that the techniques of the two efforts (e.g. neutron detection at BYU and calorimetric measurements at the University of Utah) were complementary and that the research effort could be benefitted by cooperation.

B1. Pons responded with telephone call December 1988

Dr. Gajewski did inform Pons of the proposed cooperation, who in turn called Jones in (about) December 1988 to discuss the matter.

B2. Jones offered use of neutron detector

In ensuing contacts, Pons requested written information regarding the neutron spectrometer which had been developed at BYU. Jones mailed him this information and offered to allow Pons to use the operating neutron spectrometer at BYU. Pons seemed pleased with the offer.

B3. Pons and Fleishmann visited Brigham Young University laboratory, 23 February 1989

Finally, on 23 February, 1989, Pons and Fleishmann came to BYU to visit Jones and his colleagues in the BYU

Underground Laboratory. Pons and Fleishmann were shown the neutron spectrometer and the neutron-energy spectra which it produced, including calibration and actual data distributions. In particular, we openly pointed out the significant fusion neutron signal observed in our data. We also discussed some of our geological evidence for cold fusion (tritium in volcanic gases). In the exchange of information, Fleishmann showed us one of their electrochemical cells, although he indicated that this particular one was one that did not work. We invited them to bring their (working) apparatus to BYU to verify its operation with our neutron spectrometer. They agreed, and the date of 26 February was set for the test.

B4. Researchers agreed to work toward simultaneous publication

Over lunch at BYU that day (23 February 1989), Jones told Pons and Fleishmann that the BYU group was preparing to publish their data and offered to let them publish simultaneously. Dr. Jones reports that when he made the offer to allow the University of Utah researcher to publish simultaneously with the BYU report, he was attempting to establish an open and cooperative relationship. The University of Utah researchers did not come back to the BYU laboratory to test their equipment on 26 February as agreed. Rather, they explained that morning (via telephone) that a graduate student had had to travel to a funeral, and said that they would plan to come at the end of the week. But they did not come then, either. Subsequently, a meeting was proposed by University of Utah President Chase Peterson for 6 March 1989, to be held at BYU with the chief scientists and Presidents of the two universities present.

IV. Contacts Between University Administrators

A. Telephone discussion between President of University of Utah and Provost of Brigham Young University, 3 March 1989

On Friday, March 3rd, the President of the University of Utah called the Provost of Brigham Young University. He made some observations about the significance of cold fusion research going on at both universities, and some of the complexities surrounding the project. He then asked for a meeting with top university administrators and chief scientists involved in the projects as soon as possible. A meeting was scheduled for the following Monday.

B. Meeting between Brigham Young University and University of Utah administrators and principal scientists at Brigham Young University, 6 March 1989

On Monday, 6 March, University of Utah president (Chase Peterson), his vice-president for academic affairs (Joseph Taylor), and the two principle scientists (Drs. Pons and Fleischmann) involved in the cold nuclear fusion experiment at the University of Utah, arrived at 9:00 A.M. to begin the scheduled meeting. Brigham Young University participants in the meeting were the president of the university (Jeffrey R. Holland), the Provost and academic vice-president (Jae R. Ballif), the associate academic vice-president responsible for research (LaMond Tullis), and the principal scientist who directs Brigham Young University's cold fusion experiments (Dr. Jones).

Before the meeting, the president of the University of Utah met separately with the president of BYU and his provost to discuss the agenda. It was agreed that the University of Utah's president could pursue his agenda so long as it included a brief historical summary of the research done at BYU.

President Chase Peterson of the University of Utah first explained how wonderful an invention practical cold nuclear fusion would be. He also said that the large monetary proceeds from said invention could be extremely valuable to the University of Utah. Dr. Jones then held up a small flashlight and stated a strong cautionary note that he would be extremely surprised if enough power could be generated by the process to power even a flashlight in the foreseeable future, and that he could not see in any case how the proceeds from the invention could be vouchsafed for the University of Utah. Jones then reviewed the history of the BYU research on cold fusion (at the request of Academic Vice President Ballif). He described much of the history given above. In particular, Jones showed a notarized page from his own logbook dated (and notarized) 7 April 1986, demonstrating that the metals palladium, platinum, nickel, lithium, copper, and aluminum were particularly enumerated for the BYU research on cold/piezonuclear fusion on that date. He also showed copies of pages from Paul Palmer's notebook that demonstrate unequivocally that experimental research using electrolytic infusion of hydrogen into various metals began at BYU on 26 May 1986, 2 ? years before we learned of the University of Utah work in this area, with the first positive hints of cold nuclear fusion by this process presenting themselves on 27 May 1986. None of these dates were questioned, nor were there any questions about the proposal-

review process involving Dr. Jones. They did not allege at that time that the BYU group had pirated any ideas from their research. After Dr. Jones' review of detailed documents showing the BYU research over the years, Utah President Peterson turned to BYU president Holland and commented on the remarkable coincidence that such similar research had sprung up independently at the two universities. This met with general agreement.

Then the meeting shifted to a discussion of releasing the information to the public. The University of Utah researchers stated that they would prefer to have up to eighteen months to quietly pursue their research before announcing it. Dr Jones stated that he had been funded on the research in question since May 1986, that he had positive results, that he felt obliged under DOE grant to publish his results. Furthermore, the DOE funding agent (Dr. Gajewski) had encouraged him to go ahead with a publication on the experimental work to complement his earlier theoretical paper. In particular, Jones displayed his abstract for an Invited Paper to the Spring Meeting of the American Physical Society of 2 February 1989, which states in Part: "We have shown that nuclear fusion between hydrogen isotopes can be induced by binding the nuclei closely together for a sufficiently long time, without the need for high-temperature plasmas.....We have also accumulated considerable evidence for a new form of cold nuclear fusion which occurs when hydrogen isotopes are loaded into various materials, notably crystalline solids (without muons). Implications of these findings on geophysics and fusion research will be considered."

The University of Utah contingent expressed great concern about Jones' speaking at the May meeting in Baltimore. In particular, University of Utah President Peterson suggested strongly that it would be desirable for Jones not to give the talk. Dr. Jones replied that he was shocked that Pres. Peterson would suggest that he give up an invited APS-meeting talk on the BYU work, and Pres. Peterson finally agreed that he would not ask Jones to cancel his talk. Instead, it was agreed that the two groups would submit papers SIMULTANEOUSLY and quickly (in about three or four weeks time) in order to have the papers accepted and hopefully published before Jones' scheduled talk on May 4, 1989. It was also agreed by all that no public disclosure of the research would be made by either group prior to the simultaneous submission of the papers. In keeping with this understanding, Jones said that he would cancel a previously scheduled physics department colloquium at BYU, set for 8 May (two days later), and he did so. Jones also cancelled a talk by a graduate student (Stuart Taylor) on the BYU cold fusion research scheduled at the BYU Spring Research Conference on 11 March 1989, in order to strictly adhere to this agreement.

In subsequent discussions between Jones, Pons, and Fleischmann, it was agreed that the precise day for the joint submission would be on 24 March 1989. On 21 March Dr. Pons called Dr. Jones and the joint submission date of 24 March was re-confirmed. Dr. Pons indicated that the University of Utah paper was ready but assured Jones that it would not be submitted earlier than 24 March. No mention whatsoever was made of the University of Utah press conference held on 23 March 1989, one day prior to the agreed date for releasing the information jointly, or of the University of Utah paper on cold fusion already submitted on 11 March 1989, to the Journal of Electroanalytical Chemistry by Pons and Fleischmann.

C. Agreements from 6 March 1989 meeting

After an extended discussion of what might be done to accommodate the interests of the University of Utah delegation, it was agreed by all present that:

C1. Simultaneous publication

Scientists at Brigham Young University and the University of Utah would prepare and submit simultaneous publications to the same journal.

C2. Publication prior to APS meeting (May 1989) where Jones was scheduled to speak

Every effort would be made to submit simultaneous publications prior to the American Physical Society Meeting, even though this would be difficult in the short time available. It was agreed that publication in the most prestigious physics journals would be pursued first, but if that could not be accomplished in time, the papers would be submitted to another journal. It was agreed that if necessary journals outside the field of physics would be considered including simultaneous publication in a chemistry journal. Dr. Jones contacted George Miley regarding possible publication of these two paper in Fusion Technology as well as an editor of Nature, for he had previously published papers on muon-catalyzed fusion

in these major journals.

C3. Exchange of preprints

The scientists would exchange papers after they were completed.

C4. No further public comments on results of research until papers submitted

No further public announcements of the results of either teams' research would be made until after the papers were submitted for publication. Brigham Young University scientists had a department colloquium scheduled later in the week to discuss their research. Jones volunteered to cancel the presentation and did so following the meeting at BYU on March 6, 1989.

V. University of Utah Press Conference and Subsequent Events

On 22 March 1989, the BYU group had calls from people at the Department of Energy about a press release announcing a 23 March University of Utah press conference. It stated that net energy-producing cold nuclear fusion had been achieved at the University of Utah, and that a reviewer of the proposal had confirmed the result! We were shocked and disappointed by the announcement and communicated these feelings to Chase Peterson and James Brophy at the University of Utah. For example, on 22 March BYU Professor Grant Mason, Dean of the College of Physical and Mathematical Sciences, spoke to Dr. James Brophy, University of Utah Vice President for Research, and expressed to him that if the press conference were held, we at BYU would interpret this as a violation of the agreements between the two universities. His words were that we would interpret such a press conference "as a stab in the back."

The University of Utah press conference was held (despite our protestations) on 23 March 1989. No mention was made of the cold fusion research at BYU. In fact, a question was asked at the University of Utah press conference: "Is this going on any place else, or is this the kind of process that is currently being developed by anyone else?" to which University of Utah Vice President of Research James Brophy replied: "Let's see, I'll answer it and then perhaps you can . We're not aware of any such experiments going on." It was also stated that the University of Utah paper on their work had already been submitted, although they did not say to which journal. This was also a great shock to us at BYU. After learning of this, we could not see why we should wait until the next day to send our paper to Nature, so, after the press conference at the University of Utah, we sent our paper to Nature. We have received considerable criticism from University of Utah persons for not going to the airport to meet them on 24 March, to send our papers off together. However, it was verified to us on 24 March that Pons and Fleischmann had indeed submitted their paper prior to 23 March; their preprint entitled "Electrochemically Induced Nuclear Fusion of Deuterium" contained this statement on the title page: "Submitted to Journal of Electroananalytical Chemistry March 11, 1989." Thus, the University of Utah paper was submitted prior to 24 March, although we are given to understand that a paper on the subject was also submitted on 24 March to Nature. It is noteworthy that our paper to Nature was published (April 1989, 338:737),, while the P/F paper was not published in Nature after all.

There remains one final area to be recorded in this effort to lay out the facts. Numerous allegations and insinuations have been made to the effect that Dr. Jones pirated the idea of cold fusion in an electrochemical cell or some unspecified ideas from the University of Utah work based on his review of their proposal. For example, such insinuations appeared in a front page article of the Deseret News on 2 April 1989, quoting persons from the University of Utah. Similar accusations were made to officials at the Department of Energy and to scientists at Los Alamos National Laboratory in New Mexico.

On or about 14 February, University of Utah attorney Norm Brown spoke to the BYU attorney Lee Phillips and suggested that Jones had pirated ideas from his review of the University of Utah proposal. This was reported to Jones who reported the allegation to the DOE funding agent Dr. Ryszard Gajwski. Dr. Gajewski then questioned Dr. Pons about this. Dr. Pons apologized for the insinuations of the lawyer both to Dr. Gajewski and directly to Dr. Jones on or about 21 February 1989. We know of no further accusations or insinuations of wrongdoing against Dr. Jones until 10 March 1989, when Dr. James Brophy made allegations of this kind to Prof. John Lamb of BYU. During the week of 14 March 1989, Dr. Pons made general accusations against Jones to Dr. Gajewski, as reported to Jones by Dr. Gajewski.

We have provided evidence from logbooks and other sources that demonstrates that these unsubstantiated allegations are false. When the University of Utah proposal becomes available through the Freedom of Information Act, it will be completely clear, if it is not already, that our work was conducted independently of theirs. (According to agreement with the Department of Energy, BYU retained no copy of the proposal following the evaluation.) Meanwhile, we have opened our logbooks and other documents to public view; copies are available upon request from the BYU Department of Physics and Astronomy.

Jones reported: "A few weeks after the press conference at the University of Utah, I saw Martin Fleischmann in Erice, Italy for the first time since the March 6 meeting. He took me outside the breakfast room and acknowledging that his March 23rd press conference had been "unfair" to us at BYU. He did not suggest that we had driven him to this action. Rather, he told me that administrators at the University of Utah had pressured him to have the press conference. Note that no press conference on "cold fusion" was ever held at BYU. "

We had not agreed to join in a media circus with the University of Utah, which is one of the main reasons we declined to go on March 24 to the SLC airport to send off our paper with the P/F paper (we were told that the press would be there). In addition, at the March 23rd press conference at the University of Utah, it was announced that P/F had recently submitted a paper, before their press conference. So we simply faxed our paper to Nature without fanfare, in such a way that it would have a March 24 submission date. That someone from the U. Utah did go to the airport and indeed waited for BYU to show up - with the press there - shows clearly that they had understood that there was an agreement for parallel submission of papers and that they knew very well of the BYU work. However, they had already submitted their paper on March 11 without informing us. And at the press conference, Dr. Brophy of the University of Utah answered a question about whether any such research was going on elsewhere thus: "let's see, I'll answer it and then perhaps you can. We're not aware of any such experiments going on."

It would be grossly unfair to cast blame on the BYU group these actions by persons from the U. Utah or for the press conference held there, or for the public statement made by Brophy a few days later that the BYU research "confirmed" the P/F work, which statement was terribly misleading.

Jones reported: "During the May 1989 meeting at Sante Fe on Cold Fusion, I approached Dr. Brophy of the University of Utah and expressed my disgust at his allegation (made to BYU Prof. John Lamb, BYU research director, in early 1989) that I had stolen the P/F idea based on my review of their proposal. He told me he no longer believed that."

Addenda - Extracts from early BYU publications on cold fusion.

The following is extracted from a paper submitted in 1985 and published in 1986, on cold/piezonuclear fusion, by Clint Van Siclen and Steven E. Jones:

"It is interesting to consider whether piezonuclear fusion within the liquid metallic hydrogen core of Jupiter can account for the excess heat radiated from the planet. This excess is equal to about one and a half times the energy received from the sun. . . Assuming a metallic hydrogen core of radius 4.6 X 10^9 cm, 189 fusions per cm^3 The following is extracted from a proposal submitted by J. Bart Czirr of Brigham Young University, who performed research on cold fusion with BYU Professors Paul Palmer and Steven E. Jones:

"The present [neutron] detector with improved resolution will find application in several diverse fields, including:

Magnetically confined fusion plasma temperature measurements for d-d and d-t reactions, muon-catalyzed fusion experiments, other cold-fusion experiments, low intensity source measurements requiring good discriminating against background radiation, radioactive source spectrum measurements utilizing (alpha, n) reactions or spontaneous fission, and measurements of the neutron-neutron cross section through the negative-muon/deuteron reaction.

Experiments are currently in progress at Brigham Young University to search for evidence of electron-catalyzed fusion in metals containing deuterium atoms. A neutron spectrometer would provide an important diagnostic tool for the detection of 2.5 MeV neutrons from d-d reactions taking place in these experiments. [emphasis added]

This detector is designed to fill a gap in existing technology for neutron spectrum measurements and permit a broad

range of future experiments. The list above only represents a portion of the potential applications. (J.B. Czirr, SBIR Phase II Proposal, May 14, 1986. See also, DOE SBIR grant, number DE-AC02-85-ER80289.)

The following is a published abstract for a talk presented publicly by BYU student Paul Dahl at a conference held at BYU in March 1988. It demonstrates our active research in this research area a year before the Pons/Fleischman press conference, and long before we heard of them or their research:

"AN EXPERIMENTAL INVESTIGATION OF PIEZO-NUCLEAR FUSION.
P.A. Dahl, P.S. Banks, E.V.Sheeley, E.P. Palmer, S.E. Jones,
Department of Physics and Astronomy, Brigham Young University,
Provo, Utah 84602.

Excess helium-3 and tritium gases have been observed in volcanic emissions as well as in some metals. Independently of these results, theorists Rafelski and Muller suggested to us that excited HD (and by inference D_2) molecules might undergo fusion without the need for stellar temperatures to form ^3He (and tritium), in analogy to the muon-catalyzed fusion process. We are therefore motivated to study the possibility that fusion may occur at a measurable rate in the earth's crust and in metals wherein the fusion of hydrogen isotopes is facilitated by their dense packing amongst surrounding ions. To test this piezonuclear fusion hypothesis, we will prepare deuterated metal foils, subjecting some to high pressures as found deep in the earth. Then the growth of helium-3 and tritium in the foils will be evaluated."

Finally, from a copy of the original log book kept by BYU Prof. Paul Palmer to demonstrate early work on cold fusion at BYU. In particular, these log book entires show that the first cold fusion experiments at BYU were performed in May 1986.

Mar 13 1886 Source of volcanic heat.

Colloquium yesterday by Steve Jones of BYU physics set me thinking. He talked of muon catalyzed cold fusion — among other things such as quark search and electron-catalyzed fusion of HD molecules. He talked of spontaneous fusion under pressure (low) and catalyzed fusion (high).

$$H + D \rightarrow {}^3He + \sim 6\ MeV \quad (\gamma\ \text{below.})$$

Well, when earths sedimentary material at a continental margin gets pulled down in a subduction zone at a plate boundary, fusion could take place as the pressure increased.

<SNIP>

To measure all this, measure the ^3He that outgasses from the lava! Simple. These data must be available. The ratio of H to ^3He that outgasses could allow computation of the fraction of spontaneous fusions per average water molecule.

Perhaps the rock catalyzes the reaction! Temperature separates H and D from water and even oxygen might catalyze.

FIGURE 1

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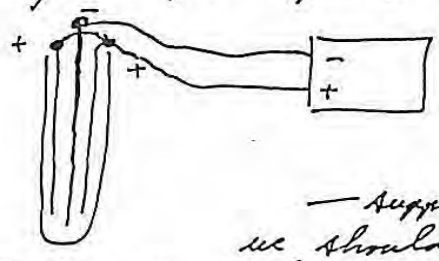
May 22 1986 Electrolytic infusion of hydrogen into metals

We constructed an electrolytic cell in a test tube to try to get hydrogen into metals.



Used a solution of HCl in water. We used copper cathode (−) and nickel anode (+) and ran enough current through to get bubbling at the electrodes. — supposedly, if Cu behaves like Fe, we should get H in the metal.

After 8-10 hrs, a heavy green coating built up on cathode(−) that flaked off when dry. The cathode was nickel plated underneath in a spotty grey-black silver coating. There was green gelatinous stuff in liquid.

We put this strip which supposedly contained hydrogen and deuterium (in the normal concentration for D_2 of .016 %) around the crystal of a gamma spectrometer. We counted background for 1200 sec and copper strip for about — ~ 2400 sec I believe.

May 22 1986 (cont.) Electrolytic infusion of H.

The results were completely inconclusive. The rate with the hydrated sample was 10% ± 7% greater than background. The count on the background was too short but the equipment was needed for other purposes. As Bart said — you wouldn't bet on cold fusion on the basis of these results — but neither would you bet against it.

We don't know if we had any H in the copper. We are going to weigh it and bake it, but that is of doubtful validity because of oxide formation and surface contamination effects.

May 23, 1986 Electrolytic cell using D_2O.

I rigged up the same cell as before but used 10% D_2O, sulfuric acid, and distilled water with copper anode and cathode. I ran it with 200 mA current and approx 1.5 V across cell. Hydrogen came off the cathode in small bubbles. Ran from ~ 10:00 AM

Bart suggests we use palladium as the metal because we know it has the ability to let H diffuse through it readily. It should work fine.

Ran cell 4½ hrs at 200 mA, then cut to 100 mA at 3:00 PM

FIGURE 2

****

(This history was originally written April-May 1989 by Professors Jae Ballif, William Evenson, Steven Jones, with revisions in March 2004.)

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132) Another testimony

Ludwik Kowalski (3/9/04)
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After posting the historical testimony of Steven Jones (item #131) I realized that the recent article of Martin Fleischmann is also worth posting here. As many might remember, Fleischmann and Pons were the first to announce the unexplained excess heat, and to claim that it was generated through a nuclear process. Unfortunately, this was done via a press release (see details in item #131) rather than through a publication in a pear review paper. The photo of Fleischmann shown below was taken during a break between two sessions of the 10th International Conference on Cold Fusion. The text that follows was extracted from an article that Fleischmann published in a journal called "Accountability in Research" *(*2000. 8: p. 19). The entire article, entitled "Reflections on the Sociology of Science and Social Responsibility in Science, in Relationship to Cold Fusion" can be downloaded from the library at www.lenr-canr.org. Let me add that Stanley Pons, who worked with Fleischmann, was not at the conference. People said he left research many years ago.

Extracts from Fleischmann's paper

"Reflections on the Sociology of Science and Social
Responsibility in Science, in Relationship to Cold Fusion;"
Accountability in Research*,* 2000. 8: p. 19.

. . . Realization that models of electrolyte solutions had to be based on the Q.E.D. paradigm inevitably focused my attention again on the Pd/H and Pd/D systems. I had realized since the end of 1947 that these were the most extraordinary examples of electrolytes....... The question of whether one could induce nuclear reactions became more clearly-defined at the end of that decade [1960s]. Work on the isotopic separation of H and D showed that it was necessary to assume that the H and D present had to be modeled as many-body systems in order to explain the macroscopic behavior . . . In the early 1980's Stan Pons and I started a number of collaborative projects. ...... We decided that the project not only had to have a 'hidden agenda', it had to be totally hidden. This was all the more necessary because the military applications of any positive outcome of the research were not at all clear. ...... The overall structure of the problem had become reasonably clear by the summer of 1988. We were observing the generation of heat in excess of the enthalpy input to the cells, and far above that commensurate with the generation of tritium and neutrons predicted by measurements on 'hot fusion.' Moreover, the excess enthalpy was far beyond that which could be attributed to any parasitic chemical reactions.

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I would be very happy to append M. Fleischmann's comments on what S. Jones wrote in the unit #131. On the other hand, I can understand his reasons for not commenting on political events of 1989-1990 at this time. So let me show how Peter Hagelstein, from MIT, summarized these events in "A Student Guide to Cold Fusion." This recently published document (authored by Edmund Storms) is avaialble over the Internet at http://www.lenr-canr.org. Less than

a year ago, Hagelstein, who has also been an important player in the field if cold fusion, wrote:

“It Started in 1989 . . .

Many of us recall the controversy surrounding the announcement of claims of observations of fusion reactions in a test tube that were made in 1989. At the time, these claims were greeted with considerable skepticism on the part of the physics community and the scientific community in general.

The principal claim of Pons and Fleischmann

The principal claim of Pons and Fleischmann in 1989 was that power was produced in palladium cathodes that were loaded electrochemically in a heavy water electrolyte. The evidence in support of this was a measured increase in the temperature in the electrochemical cell. There was no obvious evidence for nuclear reaction products commensurate with the claimed heat production. Fleischmann speculated that perhaps two deuterons were somehow fusing to ^{4}He through some kind of new mechanism.

Rejection by the physics community

This claim was not accepted by the physics community on theoretical grounds for several reasons:

First, there was no mechanism known by which two deuterons might approach one another close enough to fuse, since the Coulomb barrier prevents them from approaching at room temperature.

Second, if they did approach close enough to fuse, one would expect the conventional dd-fusion reaction products to be observed, since these happen very fast. Essentially, once two deuterons get close enough to touch, reactions occur with near unity probability, and the reaction products (p+t and n+^{3}He) leave immediately at high relative velocity consistent with the reaction energy released. To account for Fleischmann's claim, the proposed new reaction would seemingly somehow have to make ^{4}He quietly and cleanly, without any of the conventional reaction products showing up, and would somehow have to arrange for this to happen a billion times faster than the conventional reaction pathway. Most physicists bet against the existence of such a magical new effect.

Third, the normal pathway by which two deuterons fuse to make ^{4}He normally occurs with the emission of a gamma ray near 24 MeV. There was no evidence for the presence of any such high energy gamma emission from the sample, hence no reason to believe that any helium had been made.

Finally, if one rejects the possibility that any new mechanisms might be operative, then the claim that power was being produced by fusion must be supported by the detection of a commensurate amount of fusion reaction products. Pons and Fleischmann found no significant reaction products, which, given the rejection of new mechanisms, implied an absence of fusion reactions.

An alternate explanation is proposed

The physicists decided in 1989 that the most likely reason that Pons and Fleischmann observed a temperature increase was that they had made an error of some sort in their measurements. When many groups tried to observe the effect and failed, this led most of the physics community to conclude that there was nothing to it whatsoever other than some bad experiments.

The claim of Jones

A second very different claim was made at the same time in 1989 by Steve Jones. This work also involved electrochemistry in heavy water and the observation of reaction products corresponding to the conventional dd-fusion reactions. The initial publication showed a spectrum of neutron emission that Jones had detected from a titanium

deuteride cathode loaded electrochemically. The response of the physics community was skeptical, as the signal to noise ratio was not particularly impressive. Given the polarization of the physics community in opposition to the claims of Pons and Fleischmann (which were announced essentially simultaneously), the physicists were not of a mood to accept much of any claims that fusion could happen in an electrochemical experiment at all. Jones went to great lengths to assure fellow scientists that his effect was completely unrelated to the claims of Pons and Fleischmann, and was much more reasonable.

Also rejected

Physicists had reason to be skeptical. Theoretical considerations indicated that the screening effects that Jones was relying on were not expected to be as strong as needed to account for the fusion rates claimed. As this experiment could not seem to be replicated by others at the time, it was easy for the physics community to reject this claim as well.

Cold fusion, weighed and rejected with prejudice

Cold fusion, as the two different claims were termed, was dismissed with prejudice in 1989. The initial claims were made near the end of March in Utah, and the public refutation of the claims was made at the beginning of May. It only took about 40 days for the physics community to consider the new claims, test them experimentally, and then announce loudly to the world that they had been carefully weighed and rejected.

Following this rejection, physicists have treated cold fusion rather badly. For example, Professor John Huizenga of Rochester University was selected to be co-chair of the DOE ERAB committee that met to review cold fusion and issue a report. Shortly afterward, he wrote a book entitled Cold Fusion, The Scientific Fiasco of the Century, in which he discusses the claims, the experiments, and the extreme skepticism with which the new claims were greeted. Robert Park discusses the subject in his book entitled Voodoo Science. You can find many places where physicists and other scientists happily place the cold fusion claims together with claims of UFOs and psychic phenomena."

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133) Another new name for “cold fusion?”

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This morning I received a message announcing the next conference (ICCF11). And I see that the term “cold fusion” has been dropped. The new name is CMNS (Condensed Matter Nuclear Science)

> Dear colleague,
>
> It is my pleasure to announce you that ICCF11, the 11th International
> Conference on Consensed Matter Nuclear Science will he held in
> Marseilles, France from October 31 till November 5, 2004.
>
> Registrations are open at : www.iccf11.org Use the some site to the
> highlights of the conference: In addition to the usual format of this type
> of conferences we will have:
>
> 1- An open session at the University of Marseilles, with demonstrations
> 2- A special evening session where we will discuss with economists,
> philosophers, journalists and scientists, the consequences of the
> application of "Cold Fusion" in our day to day life, but also the geopolitical
> and economical changes that might occur.
> 3- The week long meeting will end with a press conference.
>
> I hope to see you in Marseilles
>
> Jean Paul Biberian, Chairman biberian@crmcn.univ-mrs.fr

Going to the conference web page I see an invitation to become a member of the Internationa Society for Condensed Matter Nuclear Reactions. The page has many tabs. One of them leads to FAQs (frequently asked questions). Here are two of them:

Why have you avoided "Cold Fusion" in the name of the Society?

Critics back in 1989 dubbed the science "Cold Fusion" because it was a convenient straw-man to demolish theoretically. The name stuck, but it is misleading. Condensed Matter Nuclear Science is about multiple anomalies and it is unlikely that these can be explained by just one class of nuclear reactions be they fusion or fission. "Cold Fusion" is a widely used name of software and could be a source of confusion.

What is the origin of the name of the Society?

It is difficult to trace the origin but Jones' 1989 paper, Nature 338 p 737 "Observation of cold nuclear fusion in condensed matter" certainly stimulated many similar titles. According to Xing Zhong Li, the

phrase "Condensed Matter Nuclear Science" was suggested at a meeting of the ICCF-9 Advisory Committee and he adopted it as the title for the conference proceedings. Subsequently the title was adopted for the electronic journal as announced at ICCF-10

http://www.rle.mit.edu/phagelstein/.

The Society's name may be abbreviated to ISCMNS. Condensed matter nuclear physics is not a new science. It includes neutron and muon physics, fields which are of interest to CF (but often overlooked).

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134 Second Evaluation by DOE?

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This Unit #134 will be used to record what I learn and think about the pending evaluation of cold fusion by the US Department of Energy (DOE). I started composing it on 3/21/04.

Below are extracts from an e-mail message received today (3/21/04). The author is Eugene Mallowe; the message was probably sent to many people interested in cold fusion. Somehow I suspect that what is written below is not true. On the other hand, why would Eugene put his reputation at stake without being certain? The title of the full text of his message is "**U.S. Department of Energy Will Review 15-Years of Cold Fusion Excess Heat and Nuclear Evidence**." It will, most likely, be available at:
www.infinite-energy.com

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1) The U.S. Department of Energy has made a startling reversal of its past refusal to evaluate with a fresh look the large body of experimental evidence that now supports highly anomalous non-chemical magnitude excess heat phenomena in some hydrogen systems, plus associated nuclear anomalies.

2) The confirmation of the DoE review came first in a draft article by Physics Today science journalist Toni Feder. This draft was circulated to several LENR scientists, critics, and others who gave input to Ms. Feder. New Energy Foundation provided input to Ms. Feder and welcomed receipt of the draft article from her. The article is to appear in Physics Today's April 2004 issue, which should be out by the first week of April.

3) The first popular journal to publish the news of the impending DoE review is, however, the UK-based New Scientist. In its March 20, 2004 issue, which was received in the mail today (3/20) at New Energy Foundation here in Concord, New Hampshire, freelance journalist Ben Daviss reports in a short article in the 'Upfront: News in perspective' section (p.6), that James Decker, deputy director of the DoE's Office of Science, 'has pledged to review evidence from the past 15 years of research in the controversial field.' Daviss also writes, 'The study could be completed by January 2005 and might open up the possibility of funding for cold fusion research projects.'

4)The initiative that helped launch the impending review was a letter to U.S. Energy Secretary Spencer Abraham from MIT Professor Peter Hagelstein, a cold fusion theorist since 1989. ..... It was shortly after ICCF10 that Prof. Hagelstein wrote to Spencer Abraham. .....The door to DoE was evidently further opened by Randall Hekman, who is an MIT graduate (1969), a former judge, and an energy entrepreneur (Hekman Industries). Hekman knows Spencer Abraham and Republican Congressman Vern Ehlers from Michigan, who is a physicist. Ehlers is quoted in the Physics Today article that it is time for a new review 'because there is enough work going on and some of the scientists in the area are from respected institutions.' "

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After reading the note of Ben Daviss (see below) I posted the following message on the e-mail list CSAM, at Montclair State University.

“Those interested in social aspects of cold fusion might like to read a just published note shown below. It appeared in the March 20, 2004 issue (page 6) of the British journal New Scientist. The title is "No cold shoulder" and the author is Ben Daviss. At first I thought that the e-mail massage about the forthcoming review of the CF field was a joke but now I believe it is true. It is not yet April 1 to print a joke like this. In my opinion there should be two independent panels, one dealing with fundamental science and another with possible practical applications. I would prefer the US Academy of Science to be in charge with the first issue. Practical applications, if any, are decades away. Making promises which can not be delivere is dangerous. That was one of the mistakes made 15 years ago (on March 23, 1989).

And I think that several months is not enough to conduct a serious study of many claims which may or may not be related. I would give scientists two years and enough money to travel to numerous distant laboratories willing to perform experiments with them. The two major claims are: occurrence of unexplained nuclear reactions and generation of unexplained excess heat. The first should be verified by physicists, the second should be verified by chemists (to show that measured heat can not be explained by chemical reactions or another energy storage mechanism).

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NO COLD SHOULDER (by Ben Daviss, 3/20/04)
After being banished by many to the realms of pseudoscience, a review by the US Department of Energy is promising to bring cold fusion back in from the...well, cold. James Decker, deputy director of the department's office of science, has pledged to review evidence from the past 15 years of research in the controversial field. The study could be completed by January 2005 and might open up the possibility of funding for cold fusion research projects.

The pledge was sparked by a letter from Peter Hagelstein of the Massachusetts Institute of Technology, a leading cold fusion theorist, to energy secretary Spencer Abraham an international cold fusion conference in Boston in August 2003. The letter summarized the field's recent progress, which includes a new theoretical framework rooted in the equations that define hot fusion. The theory makes quantifiable predictions that proponents of cold fusion say have been verified experimentally. Insiders say that energy department scientists have also been following results in cold fusion, and laying the groundwork for a review for more than a year "There's so much new, valid scientific evidence that it's high time to take another look," says George Miley, professor of nuclear engineering at the University of Illinois. "I'm hopeful that a fair review will remove any remaining prejudice so research proposals in cold fusion will finally be able to compete on their merits."

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6) I also received the draft of the article of Tony Feder. But it would not be appropriate for me to say anything about it at this time. She is one of the editors of Physics Today and her article about the new situation is going to be published. I will comment on that important article after it is printed, presumably in three weeks. In fact, I will try to turn my comments into a letter to the editor of Physics Today. Will they reject my contribution for the third time in a row? It will depend on who what motivates their deire to address the issue.

7) One might find it useful to read recommendations made by the panel of scientists that conducted the first evaluation (see unit #26). Let me quote from it again:

RECOMMENDATIONS (from the ERAB report, November 1989)

A. The Panel recommends against any special funding for the investigation of phenomena attributed to cold fusion.

Hence, we recommend against the establishment of special programs or research centers to develop cold fusion. **[They did not recommended blacklisting, they did not recommended stopping ordinary support, etc.]**

B. The Panel is sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system. **[In other words, they recognized that their negative evaluation was not the final word.]**

C. The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. .....Cooperative experiments are encouraged to resolve some of the claims and counterclaims in calorimetry.

D. A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for the production of fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels. **[And a lot of new data is now avaialable on accumulation of ^{4}He at the rate of about one atom per 24 MeV of excess heat. In other words, the commensurate products were found but they turned to be different from what was expected. It is nor longer a situation in which there were a "fire" without "ashes."]**

E. Investigations designed to check the reported observations of excess tritium in electrolytic cells are desirable. **[Another recognition of something interesting and important.]**

F. Experiments reporting fusion products (e.g., neutrons) at a very low level, if confirmed, are of scientific interest but have no apparent current application to the production of useful energy. In view of the difficulty of these experiments, collaborative efforts are encouraged to maximize the detection efficiencies an to minimize the background. **[That what Steven Jones was doing in the last 15 years. It is about time that other nuclear scientists evaluate his recent data.]**

THAT WAS WRITTEN IN 1989. I INTERPRET IT AS A CALL FOR A FUTURE INVESTIGATION OF ANTICIPATED NEW FINDINGS.

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1) Today's article (3/25/04) in The New York Times (by Kenneth Chang) gives additional details on the pending process. Should I be disappointed? The name of the article, by the way, is "**U.S. Will Give Cold Fusion Second Look, After 15 Years.**" Here are some extracts:

"Cold fusion, briefly hailed as the silver-bullet solution to the world's energy problems and since discarded to the same bin of quackery as paranormal phenomena and perpetual motion machines, will soon get a new hearing from Washington..... Last fall, cold fusion scientists asked the Energy Department to take a second look at the process, and last week, the department agreed..... 'It was my personal judgment that their request for a review was reasonable,' Dr. Decker said. For advocates of cold fusion, the new review brings them to the cusp of vindication after years of dismissive ridicule.

'I am absolutely delighted that the D.O.E. is finally going to do the right thing,' Dr. Eugene F. Mallove, editor of Infinite Energy magazine, said. 'There can be no other conclusion than a major new window has opened on physics.' ..... The research is too preliminary to determine whether cold fusion, even if real, will live up to its initial billing as a cheap, bountiful source of energy, said Dr. Peter Hagelstein, a professor of electrical engineering and computer science at the Massachusetts Institute of Technology who has been working on a theory to explain how the process works. Experiments have generated small amounts of energy, from a fraction of a watt to a few watts. Still, Dr. Hagelstein added, 'I definitely think it has potential for commercial energy production.'

Dr. Decker said the scientists, not yet chosen, would probably spend a few days listening to presentations and then offer their thoughts individually. The review panel will not conduct experiments, he said. 'What's on the table is a fairly

straightforward question, is there science here or not?' Dr. Hagelstein said. 'Most fundamental to this is to get the taint associated with the field hopefully removed.' . . .

Because cold fusion, if real, cannot be explained by current theories, the inconsistent results convinced most scientists that it had not occurred. The signs of extra heat, critics said, were experimental mistakes or generated by the current or, perhaps, chemical reactions in the water, but not fusion. Critics also pointed out that to produce the amount of heat reported, conventional fusion reactions would throw out lethal amounts of radiation, and they argued that the continued health of Drs. Pons and Fleischmann, as well as other experimenters, was proof that no fusion occurred. Some cold fusion scientists now say they can produce as much as two to three times more energy than in the electric current. The results are also more reproducible, they say. They add that they have definitely seen fusion byproducts, particularly helium in quantities proportional to the heat generated.

After a conference in August, Dr. Hagelstein wrote to Energy Secretary Spencer Abraham, asking for a meeting. Dr. Hagelstein; Dr. Michael McKubre of SRI International in Menlo Park, Calif.; and Dr. David J. Nagel of George Washington University met Dr. Decker on Nov. 6. 'They presented some data and asked for a review of the scientific research that has been conducted,' Dr. Decker said. 'The scientists who came to see me are from excellent scientific institutions and have excellent credentials.' Scientists working on conventional fusion said cold fusion research had fallen off their radar screens. 'I am surprised,' Dr. Stewart C. Prager, a professor of physics at the University of Wisconsin, said. 'I thought most of the cold fusion effort had phased out. I'm just not aware of any physics results that motivated this.' “

2) What would I suggest to scientists appointed to investigate cold fusion? **SEEK NOT THE GOLDEN EGG, SEEK THE GOOSE**. In other words, focus on essential scientific questions and not on practical applications which are far away, at best. Promising too much, and too early, was one of the mistakes made fifteen years ago. In my opinion the six most important scientific questions are:

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a) Is it true that unexpected neutrons, protons, tritons and alpha
     particles are emitted (at low rates) in some LENR experiments?
b) Is it true that generation of heat, in some LENR experiments, is
     linearly correlated with the accumulation of He-4 and that the rate
     of generation of excess heat is close to 24 MeV per atom of He-4?
c) Is it true that highly unusual isotopic ratios have been observed
     among the elements found in some LENR systems?
d) Is it true that radioactive isotopes have been produced in some
     LENR systems?
e) Is it true that transmutation of elements has occurred in some
     LENR setups?
f) Are the ways of validating scientific findings in the areas of
    LENR research consistent with accepted methodologies in
    other areas of science?
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Several other questions about cold fusion research can be asked but the above six are probably the most important. I think that **a positive answer to even one of these questions** should be sufficient to justify an official declaration that “cold fusion, in light of recent data, should be treated as a legitimate area of research.” The normal peer review mechanisms will then be used to separate valid claims from wishful thinking.

3/27/04

In a message received this morning a friend wrote “I see that Park did not mention the DoE review in his current ‘What's New’." To verify this I went to the APS web site <http://www.aps.org/WN/index.cfm> and I saw that the friend is correct. The piece dated 3/26/04 has nothing about the pending DOE review of cold fusion. Is it possible that Dr. Park knows nothing about it?

But I see something else which is perhaps significant. At the bottom of the first item Park wrote: “Are we about to be exposed? It’s time to come clean: WN has fabricated interviews for years. It gives us full control of a story, and it’s highly addictive. Having no experience at confession, WN turned to a professional, Mia Culpa, for help. ‘It’s best to be indirect,’ she mused, "perhaps you could reveal the truth in a whimsical interview with a fictitious expert. Thanks Mia."

Is this an unrelated joke? Most likely. But it was a big surprize to see the Mia Culpa on the web page consulted to see what the spokesman for APS, and the author of the book “Voodoo Science,” has to say about the pending DOE

evaluation of cold fusion. Why was this evaluation not metioned?

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135) From the Salt Lake Tribune

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Yesterday was the 15th anniversary of the infamous press conference at the University of Utah. Not surprisingly, two articles about cold fusion were printed in the Salt Lake Tribune to remember the historical event. These articles were probably written at least a week earlier; the pending DOE initiative would otherwise be mentioned. Here are some extracts. Quoting Steven Jones the author writes: " 'There's really a lot going on that looks very favorable,' Jones said of the field [of cold fusion] today. 'But it's not given a fair chance in the journals.' . . . Jones plans to publish a pair of research papers, as part of the conference record, that involve new thoughts on reactions and evidence of tritium, which he sees as a telltale sign of fusion, in two volcanoes. "

I suppose these are the papers summarized in my unit #113. "In the late 1980s and early 1990s, Jones was able to reproduce what he believes are cold fusion reactions about 15 percent of the time. Today, Jones and company claim to see evidence of charged particles as a product of nuclear fusion up to 80 percent of the time. They also indicate the presence of neutrons about 40 percent of the time. Even the 80 percent figure was still not enough for the top science journals. .....The journal Physical Review C turned down one of Jones' recent efforts because the experiment did not work 100 percent of the time. Another problem: Jones could not offer an explanation for what exactly was happening."

Hmm, interesting. Do the editors of this journal ask authors of all experimental papers about how many unsuccessfull attempts were made before a success? In the past I coauthored many published papers but I do not remember being asked about the number of unsuccessful runs or about the number of repetitions. Why is cold fusion treated differently? Because extraordinary claims call for extraordinary scrutiny. But a success rate of 80%, in my opinion, is already very impressive. Knowing the credentials of the author I would publish the article, if i were the editor.

"These sorts of woes [irreproducibility] have haunted the cold fusion community since the early 1990s. The events of 1989 have essentially stigmatized the field as a whole, said Bart Simon, a sociologist at Canada's Concordia University. 'You can't deny it would have been a different story if it had not been publicized when it was publicized,' said Simon, who wrote the 2002 book, Undead Science: Science Studies and the Afterlife of Cold Fusion. Jones continues his unfunded work with an apparatus that uses deuterium gas. Three labs are working on replicating his latest findings, he noted. Even without funding, Jones continues to set up fusion experiments."

In the second Salt Lake Tribune article the same author writes: " the [cold fusion] episode may be remembered most as a clash between science and capitalism, where science lost." I suppose that in this context the term capitalism refers to those administrators who wanted the golden egg before the goose was born. I hope the new DOE panel will focus on science and not on practical applications. Such applications seem to be much less certain than highly unusual experimental findings. Fortunately, this time, I see no call for massive support of another Manhattan-like project. The cold fusion tragedy probably would not have happened if scientific findings had been announced through peer review papers and not through a sensational press release. That is what I would emphasize, if asked to write a newspaper article about what happened fifteen years ago.

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136) What is cold fusion?

Trying to publish a review.

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What is cold fusion? This question should be answered before an attempt to validate CF claims is undertaken by the DOE. Some people think that cold fusion is generation
of excess heat in electrochemical cells. This is not a good definition. Many hidden effects, for example, parasitic chemical reactions or some kind of storage (as in an electric battery) can produce "excess heat." For several days I was trying to formulate a satisfactory definition of cold fusion; I now have one. I hope it can be useful to those who will be examining CF claims.

Cold fusion is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutron sources, stellar temperatures, cosmic rays or alpha particles.

P.S. insertion (May 5, 2004):

Referring to the above definition Steven Jones, who invented the term cold fusion, wrote to me in private: “I strongly suggest that you re-consider your definition of cold fusion. Consider:

Cold Fusion is: ‘Nuclear fusion which occurs in condensed matter.’

This brings in recent experimental observations of strongly enhanced fusion yields with low-energy deuteron beams on d and Li in metal lattices, as well as the observation of the importance of currents in deuterided metals. The "condensed matter" requirement implies "cold" temperatures without having to say "cold fusion." Note also that fusion includes p,d,t +Z fusion, where Z is equal to OR GREATER THAN ONE. (This includes hydrogen istopes, lithium, and heavier elements.) In short, your definition should include the most important new directions in the field! Note that our original Nature paper (April 1989) was entitled "Observation of cold nuclear fusion in condensed matter." "Condensed matter" is the key to the whole matter, for it enhances the p,d-Z fusion rate... We are still exploring how this trick is accomplished. But it is real!

END of the P.S. insertion

Focusing on excess heat is not the best way to investigate CF. My advice would be to focus on presence or absence of easy to demonstrate nuclear signatures, such as highly abnormal isotopic ratios or the accumulation of ^{4}He at the rate of one atom per 24 MeV of excess heat. Such findings have recently been reported by several CF researchers. They say that CF is no longer "a fire without ashes," as it was characterized in 1989. Is this true? Only highly trained chemists, working in ultra-clean labs, will be able to tell.

Will the DOE appointed panel ask cold fusion experts to perform demonstrations in their presence or will it accept testimony of several teams of scientists who observed accumulation of ^{4}He (and other presumably observed ashes)? Will they ask Steven Jones to repeat several experiments in which the probability of success (to observe neutrons and other nuclear particles) was reported to be 70%? It depends on the mandate assigned to the panel, and on financial means given to it. The idea to investigate resent CF claims is very good but the project could easily degenerate into personal clashes and inconclusive investigations due to insufficient means. Mutual personal accusations between R. Park and E. Mallove (see item 2 below) are totally inappropriate at this time. Park thinks that Mallove is a leader of CF researchers while Mallove thinks that Park is the leader of researchers who categorically reject CF.

I think that both assumptions are wrong. No one is the leader of about 200 top CF researchers, world-wide. No one is the leader of tens of thoudands of scientists, also world-wide, who are very sceptical about CF claims. The first group is loosely organized through yearly conferences, the second has a powerful bureaucracy controlling the tax money. In that sense the situation is highly assymetric. But individual scientists in each group are very similar. Most of them are not extremists and that is why, I think, an honest public dialog is possible. Will those who organize the second review of cold fusion be able to keep the debate scientific? Will they be able to correct assymetries and make sure that the entire process is not derailed by extremists? I hope so.

2) Illustrating poisonous words.

Using the American Physical Society (APS) server <http://www.aps.org/wn/> Robert Park wrote (on 4/2/04): “COLD FUSION: TRUE BELIEVERS SEE DOE REVIEW AS ‘VINDICATION.’ There hasn’t been much to celebrate in the 15 years since the University of Utah held a press conference in Salt Lake City to announce the discovery of ‘cold fusion.’ Although a brave little band of true believers continued to trumpet cold fusion, the band leader was publishing ‘Infinite Energy Magazine.’ That made it pretty hard to take this stuff seriously.” To which Mallove reacted with an e-mail message. It arrived on the same day; and was probably sent to many others. Mallove wrote:

“Dear All: One of the greatest honors that can be bestowed on a scientist is to be attacked by the mental midget, Robert Park, who represents himself as a ‘scientist’ and a spokesperson for the American Physical Society. . . I am happy for the great privilege of being repeatedly disparaged over the years (since 1991) by this buffoon. And Infinite Energy magazine gets free publicity too. As everyone knows, Park is completely out of touch with the now compendious laboratory evidence for low-energy nuclear reactions (LENR - a.k.a. ‘cold fusion’). He has not examined the papers -- peer-reviewed and otherwise, and he damned well knows that. So at some deep level, this regular purveyor of misinformation knows that he is cornered and will die the fool that he is -- for sure on the topic of low-energy nuclear reactions.

I look forward to the day when the vile corpse of Park no longer rattles and confuses the world with weekly doses of misinformation. When will the APS wake up? I hesitated to use the ‘corpse’ metaphor, but Park seems so fond of using it against cold fusion that in this case it is seems highly appropriate. And besides, there may be another oak tree waiting for him...to put him out of his misery in the face of his inevitable doom on the matter of LENR. APS members should be so lucky. But while Park still breathes and rants, they should not continue to allow him to be the anti-science black mark that he represents for the APS. Finally, I categorically deny - of course - that I am the leader of any ‘cold fusion band’! Sincerely, Dr. Eugene F. Mallove President, New Energy Foundation, Inc. (A Nonprofit, 501(c)(3) Corporation) Editor-in-Chief, Infinite Energy Magazine PO Box 2816 Concord, NH 03302-2816 www.infinite-energy.com editor@infinite-energy.com”

I hope that the pending DOE review will not trigger a war of insulting words and accusations. How can an objective evaluation of scientific facts and interpretations be conducted when poisonous bullets are flying in all directions? But who can prevent Park and Mallove from jumping at each other? Would they be able to forget what they wrote about

each other, publicly, in more than a decade? I do not think so. They probably enjoy verbal wars. That is very unfortunate.

3) On March 23, 2004, in a private e-mail message to X, I wrote:

According to today's article about cold fusion in The New York Times the second evaluation will not going to be as detailed as the first one was. Is this correct? I have a slogan to suggest for your meetings. SEEK NOT THE GOLDEN EGG, SEEK THE GOOSE. In other words, focus on essential scientific questions and not on practical applications which are far away, at best. Promising too much, and too early, was one of the mistakes made fifteen years ago. In my opinion the six most important scientific questions are:

1) Is it true that unexpected neutrons, protons and tritons are emitted (at a very low rate) in some LENR experiments?
2) Is it true that generation of heat, in some LENR experiments, is linearly correlated with the accumulation of He-4 and that the rate of generation of excess heat is close to 24 MeV per atom of He-4?
3) Is it true that highly unusual isotopic ratios have been observed among the elements found in some LENR systems?
4) Is it true that radioactive isotopes have been produced in some LENR systems?
5) Is it true that transmutation of elements has occurred in some LENR setups?
6) Are the ways of validating scientific findings in the areas of LENR research consistent with accepted methodologies in other areas of science?

I think that a positive answer to even one of these six questions should be sufficient to justify an official declaration that "cold fusion, in light of recent data, should be treated as a legitimate area of research." The normal peer review mechanisms will then be used to separate valid claims from wishful thinking. Please excuse this attempt to share with you a personal opinion on a subject with which you must now be deeply preoccupied. Feel free to share the above with anybody you wish, or to modify it in any way that can be useful.

4) My review article

In trying to publish my review article about CF I approached editors of several well known US journals. But no sucess so far. Here is what I wrote in an accompanying letter in one of the submissions.

"I am sure that you are aware of the DOE move to review the cold fusion field, as reported in The New York Times (3/25/04). Attached is a review article which, I hope, can be published in ***** . The title is 'Recent cold fusion claims: are they valid?' It is not a paper defending cold fusion claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers.

Some of these claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. The subject is interesting no matter what the final verdict of the second DOE evaluation will be.

Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. In writing this I was not aware of the pending DOE review. I am a physics teacher

at Montclair State University. Studying cold fusion was my 2003/2004 sabbatical project, which resulted in the attached manuscript." So far I was not successful; perhaps I will be more lucky with an European journal, such as Nature. That journal is also known for being negative about accepting papers devoted to cold fusion. Why should they be more receptive to my review than American journals? Instead of sending them my article I will firs ask them if they might be interested. Nature has a mechanism for this, it is called pre-submission.

5) Nature

The most impressive part of pre-submitting my CF review article to Nature was the nearly immediate negative reply. It came about ten hours after I approached them. The process of pre-submission consists of pasting information into two text boxes on their web site. The first box is for the letter about the paper; the second is for the opening paragraph of the paper, and for references used in it. In the first box I pasted what was shown in item 4 above (with some modifications). The content of the second box was:

"Cold fusion (CF), presumably discovered 15 years ago, is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutron sources, stellar temperatures, cosmic rays or alpha particles. In 1989, several months after the discovery was announced (through a press release at the University of Utah) a panel of scientists, appointed by the US Department of Energy (DOE), examined the evidence supporting the CF claims. That evidence was declared insufficient. But, as summarized in (1) 'there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system.'

CF became highly controversial and only several hundred researchers continued working on it, worldwide. Most scientists still think that cold fusion is pseudoscience. On that basis editors of many journals refuse to publish papers devoted to CF research. Only a small fraction of scientists is familiar with recent progress in that area. The purpose of this article is to objectively summarize recent findings (2) and to supply references with which I am familiar. The article was triggered by the reported initiative of DOE to review (3,4) cold fusion research. I will focus on four cold fusion claims which are, in my opinion, the most important. As a nuclear physicist, and a physics teacher, I examined some of CF publications and attended one cold fusion conference (5).

1. Huizenga, J. *Cold Fusion: the Scientific Fiasco of the Century*. Oxford University Press, 2nd edition, Oxford, 1993.
2. Cold fusion papers are usually published at specialized scientific conferences. Many of them are downloadable from the library at http://www.lenr-canr.org.
3. Daviss, B. "No Cold Shoulder." *New Scientist*, March 20, 2004, p 6.
4. Feder, T. "DOE Warms to Cold Fusion," *Physics Today*, April 2004, page 27.
5. The Tenth International Conference on Cold Fusion was held in Cambridge, Massachusetts 24 - 29 August 2003. Conference proceedings, in the form of Pdf files, can be downloaded from http://www.lenr-canr.org/iccf10/iccf10.htm "

The reply was short and clear; "Thank you for your enquiry about submitting your paper entitled 'Cold fusion 15 years later' to Nature. I regret that the paper that you describe seems unlikely to prove suitable for publication in Nature, and we accordingly suggest that you pursue publication elsewhere. I am sorry that we cannot respond more positively on this occasion. Yours sincerely Dr Karen Southwell, Senior Editor." I was aware, from browsing, that the rate of acceptance in Nature is about 1 out of 10. On that basis I should have expected a rejection.

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137) Another nonsense?

Ludwik Kowalski (4/12/04)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

In browsing the Internet I found a web site of a team promoting a cold fusion reactor. The arguments are not at all convincing. Do the authors themselves believe in what they are saying or are they fully aware that it just a commercial pitch? I suspect that a con artist directing behind this non-existing project. The URL of the web site is:

http://dspace.dial.pipex.com/brook.farm/fusion.shtml

SUMMARY REACTOR PROJECTS
A specific Reactor design has been developed by a group of eminently recognized scientists during the last fifteen years. Funding for this project has been obtained from STU, a Swedish government funding institution which invests in Hi-Tech projects for development in Sweden. Additional capital investment has also been provided by private Foundations, Institutions and the Inventors.

WHY ARE THEY SO VAGUE ABOUT EMINENTLY RECOGNIZED SCIENTISTS AND ABOUT PRIVATE FOUNDATIONS?

The ultimate aim of the Reactor application is to develop a small portable 500kW power generator based on a new nuclear "cold" fusion reaction. With the prospect of depleted natural energy resources in less than 100 years this invention could replace the totality of the worlds estimated energy demand. The unit can be made small enough to replace a standard car engine or the boiler of a family house. When selling "power-by-the-hour" each unit is expected to produce magnificently high returns. Comprehensive third party computer simulations of the process has shown its feasibility.

COMPUTER SIMULATIONS ARE OF NO HELP IN SITUATIONS IN WHICH NOTHING IS KNOWN ABOUT UNDERLYING PHENOMENA. IT IS LIKE RELYING ON A THEORY THAT DOES NOT EXIST.

There are a number of further applications for this reactor described below.

COLD FUSION REACTOR INTRODUCTION

The Inventors has taken a very close and active look at the spectacular proposals to gain nuclear fusion energy from a body of the element Palladium. However, for scientists it has been known for a long time that this element has unusual properties. By direct information from advanced scientists in USA and Japan we now know that proofs exist that some unexplained reactions occur. For this reason a materials expert has already been engaged in our group who is working with the bulk treatment of the materials in question.

METHOD
Of particular interest is that of the basic concept seen to be extremely useful in treatment of materials, implanting

deuterium into the matrix, and triggering energy releasing events.

NO EVIDENCE IS GIVEN THAT THEY KNOW HOW TO DO THIS.

We keep close collaboration with Japanese Scientists as we hold a patented construction which may be essential in the future development of a reactor type which is known to be of interest to the Japanese. We believe that our patents will be essential for the final development of a working unit to their stated requirements.

CLAIM
It has been claimed lately especially from Japan that it is possible to make energy producing units based on the original idea. In combination with our patents the possibility of a working prototype may be very feasible within a 5-7 years.

AHA, NO WORKING PROTOTYPE, ONLY SIMULATIONS. THE STYLE IN WHICH THIS WEB PAGE IS WRITTEN IS NOT VERY CONVINCING. I WOULD NOT INVEST A PENNY IN THAT REACTOR.

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138) Other summaries of events

Ludwik Kowalski (4/15/04)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Let me start with an article fetched by google (at www.pureenergysystems.com). It ends with links worth having.

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by Marc J. Plotkin
Pure Energy Systems News Service
March 27, 2004

FAIRFAX, VA USA

After fifteen years of wandering in the wilderness, the “cold fusioneers” may finally see their field get the recognition they believe it deserves.

Since 1989, that small but growing band of scientists has persisted in trying to verify the existence of low-energy nuclear reactions, at great personal costs and in the face of overwhelming opposition and ridicule from the mainstream physics community. But now, their persistence may finally be bearing fruit. The New York Times reported on March 25, 2004, that the U.S. Department of Energy has decided to give cold fusion a second look. At a meeting with several top cold-fusion researchers, officials from the Department indicated that given the Matterhorn of experimental evidence that has accumulated over the past fifteen years, a second review was reasonable. The Department’s findings will be presented in December 2004 or January 2005.

Three days earlier, New Energy Times science journalists Steven Krivit and Nadine Winocur have released a 50-page report on the current state of cold fusion. According to this report, almost 15,000 cold fusion experiments have been performed around the world since the field was declared anathema in 1989. In the first years after the initial announcement, experimental results were erratic and inconsistent, often with positive results occurring in only about 10 percent of the experiments. Within the last five years, however, successful replications have been occurring much more frequently. Five years ago, the Fleichmann-Pons effect had been observed in only about 45 percent of the experiments performed. Now, according to Krivit and Winocur, the effect has been reproduced at a rate of 83%. Experimenters in Japan, Romania, the United States, and Russia have reported a reproducibility rate of 100 percent.

This experimental success is due in large measure to more refined methods of measuring excess heat and detecting the signatures of nuclear reactions. Over the years, experimenters have discovered that in order to obtain more robust results, the ratio of deuterium atoms in the electrolyte solution to palladium atoms in the cathode must be above a certain minimum threshold. This is referred to as “loading.” The density of

the electric current passing through the system must likewise reach a certain threshold. More recently, it was discovered that excess heat could be generated faster if the reaction could be triggered in some fashion. In a paper presented at the 10th International Conference of Cold Fusion, held at MIT in August 2003, researchers Dennis Cravens and Dennis Letts presented a variety of methods that could be used to "shock the system," including current-pulsing, radio frequency excitations, and laser stimulation. Actual experiments were carried out at the conference, and the results were manifest for all to see.

According to Dr. Eugene Mallove, editor of Infinite Energy Magazine and a passionate advocate of cold fusion development, the evidence of excess heat and products from nuclear reactions is so extensive as to compel a finding that the cold fusion phenomenon is real. Were it not for Dr. Mallove and others who kept the faith, cold fusion might well have faded from the public consciousness.

When the Department of Energy decided to give cold fusion another hearing, it made no public announcement and did not post any information about its decision on its website. Nevertheless, Dr. Mallove remains confident that once the Department evaluates the evidence in an open-minded and unbiased fashion, it will reconsider its earlier rejection of cold fusion and pave the way for funding of next-generation cold fusion research.

Whether or not cold fusion can be turned into a useful source of energy remains uncertain. But the first step of that 1000-mile journey has been taken. The existence of the phenomenon discovered by Fleischmann and Pons in 1989, then disavowed by the scientific establishment, but subsequently confirmed worldwide in thousands of experiments, may finally be recognized as a revolutionary discovery of science. Cold Fusion may become hot news again.

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For Immediate Release:
http://www.prweb.com/releases/2004/3/prweb114697.htm

References

The Cold Fusion Report - based on personal communication with more than 50 scientists from around the world. Prominent U.S. scientists verify the efficacy of this controversial discovery.
PDF email attachment - order from *New Energy Times* ($15.00) http://tinyurl.com/2fvqk
PDF as CD-ROM shipment - from *New Energy Times* ($20.00) http://tinyurl.com/yuox7
http://www.coldfusioninfo.com -*New Energy Times* (Steven Krivit and Nadine Winocur)
http://www.infinite-energy.com/resources/pressreleasedoe.html -U.S. Department of Energy Will Review 15 Years of "Cold Fusion" Excess Heat and Nuclear Evidence
New Report Establishes Case for Cold Fusion - press release by Steven Krivit (*ZPEnergy* ; Mar. 22, 2004)
US Will Give Cold Fusion Second Look, After 15 Years (*New York Times* ; March 24, 2004)
Energy Department accepts scientists' request to revisit cold fusion (*Salt Lake Tribune* publication of the *New York Times* ' story [no login required]; March 27, 2004)
New studies of cold fusion prompt an official review (*International Herald Tribune* , France; March 25, 2004)
Cold fusion gets second look (*India Times* ; March 25, 2004)
http://www.infinite-energy.com -*Infinite Energy Magazine* (Eugene Mallove)

http://news.google.com/news?hl=en&q=cold+fusion&btnG=Search+News - Latest international news reports on "cold fusion" according to Google.

Links

http://www.lenr-canr.org - premier Cold Fusion site
http://FreeEnergy.GreaterThings.com/Directory/ColdFusion/ Cold Fusion -- 15 Years and Heating Up (anniversary March 23)

See also

Contact

PESN - Pure Energy Systems News

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Last updated March 27, 2004

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http://www.washtimes.com/upi-breaking/20040331-050432-3573r.htm

Here is the ending of the article posted by Charles Choi at the above URL

"I was working at Los Alamos National Laboratory when Pons and Fleischmann made their announcement," Storms recalled. "The laboratory took an enthusiastic interest in the claims and many efforts were undertaken to replicate. Only three were successful, one of these being my effort. Actually seeing the effect is a powerful reason to believe it is real and caused me to continue my research after I retired."

Within a year after the initial announcement, a Department of Energy review decided cold fusion did not bear special federal funding. "The Department of Energy has been the single most important impediment in the development of the cold fusion phenomenon," Storms said.

McKubre, director of SRI's Energy Research Center, told UPI he felt the original Department of Energy review was "premature and hasty, but it couldn't have been avoided. And it really was not that damning if interpreted rationally. The original panelists said they didn't see any evidence to merit special treatment. That was interpreted as a condemnation, which meant no money was made available." In the past 15 years, researchers in universities, government, military and private labs in at least 13 countries have pursued cold fusion, according to New Energy Times. McKubre noted cold fusion results are now more reproducible.

Now, a number of prominent international researchers treat cold fusion seriously, including physics Nobel laureate Carlo Rubbia, McKubre said, adding the U.S. government has provided funding for cold fusion research, albeit through military agencies, such as the Defense Advance Research Projects Agency, and the Naval Research Laboratory -- not DOE.

McKubre said the new review has a target date of January 2005 for reporting its findings, although he said "it seems to

be acted on in the Department of Energy at lightning speed. My guess is it could be done by the end of the academic summer."

Park had no objections to the review.

"The way the system is supposed to work is that everybody is supposed to make their point, that science is not closed," he said.

Charles Choi covers research for UPI Science News. E-mail sciencemail@upi.com

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And here is one which is short but very significant. I can say this because I met some of those who are motivated by "results once seen." On 3/29/04 Richard Hull posted this message"

". . . As much as physicists would like to see it all just go away like N rays and polywater, there are just too many folks impressed by the process and befuddled by its capriciousness to just drop it. Results, once seen, even if not replicable on demand, keep a faithful cadre of workers beguiled. (as it should any true scientist.). "

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John Harris, like myself, went to the 10th cold fusion conference to learn what about the field. On 3/27/04 he posted a message, under the subject "Cold fusion - Another official look." Here is his messages:
"The tenth conference on cold fusion was held last August in Cambridge. I went up to see what was going on. Here is a quick summary of what I gathered in. Various researchers have by this time accumulated evidence for heat, helium-4, helium-3, and tritium. New to me were reports of quite energetic alphas loose in the system (8 MeV and faster) and energetic protons as well. It was not clear to me whether or not the protons were knocked out of deuterium by the alphas' passage. Fast neutrons are still missing. I don't think anyone has ever assessed the slow neutron population in a PF (Pons Fleischmann) cell.

There was a report of a change in lithium isotope ratios in the cathode, but it was not clear (to me, anyway) if this change could be attributed to reactions or to electromigration. One energetic trend in experimental systems is to thin films, and I had the impression some investigators would like to get away from the electrolysis cell altogether, that is, load palladium (or other metals) with deuterium under pressure rather than electrolytically. It has been found that it helps to stimulate the surface of a deuterium loaded cathode with a low power (semiconductor) laser. There were plots of laser stimulation vs xs heat out.

On the theoretical side, several researchers are still emphasizing d+d, and especially d+d => He-4. Scott Chubb has a helpful paper on the net explaining his and various other theoretical views of this concept. Peter Hagelstein has also created a theoretical basis for this pathway. The problem is that no gamma comes out. The theoretical efforts are aimed at somehow visualizing a way to turn the energy that one would expect to emerge as a gamma into heat instead. Another popular (maybe I should say modish) idea is a sort of triple deuterium reaction system. It actually seems like a pretty clean idea, and there were some experimentalists at the conference who are finding results that would seem to support it. One thing has not changed over the years. Although the umbrella name for the whole enterprise has been changed to "low energy nuclear reactions," or LENR, and there is a nodding recognition that other reactions or processes could be invoked to account for the results, the discussion is still pretty much all about the fusion of deuterium. Many of the conference papers can be downloaded, now, from the LENR site."

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Here are two messages I posted on www.MarsNews.com list:

My definition: "Cold fusion is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutrons, cosmic rays, alpha particles or stellar temperatures." Unfortunately the term

"cold fusion" means different things to different people. It is certainly premature to define cold fusion as a practically unlimited energy resource. But that is what most people say when asked to describe CF. I suspect that many disagreements about CF would disappear if its definition were accepted by all antagonists. The main issue today is validity of scientific claims, such as accumulation of He-4 at the rate of about one atom per 23 MeV of excess heat (reported by many teams), and not practical applications. Promissing too much too early was one of the mistakes made in 1989. I hope that the DOE will focus on science and not on applications. Applications would follow naturally after basic scientific claims are recognized as valid.
Posted by: Ludwik Kowalski at April 13, 2004 04:14 PM

* * * *
I am responding to my own message above. Excess heat (at the level of 1 W and less) can be due to non-nuclear causes. Most of us are not qualified to rule out possibilities of some parasitic chemical reactions (already mentioned in 1989 reports) or some electric battery effects (also mentioned by those who criticized cold fusion). That is why direct demonstrations of "nuclear signatures," such as neutrons, protons, tritons and alpha particles is much more convincing. See recent reports of Steven Jones (about neutrons and protons) or the report of Lipson (about alpha particles). These reports can be downloaded from the library at <http://www.lenr-canr.org> Excess heat, if it were reproducible at the level 50 W or more, would be not only very convincing but also very promissing. I have a web site devoted to cold fusion: <http://blake.montclair.edu/~kowalskil/cf/ >
Posted by: Ludwik Kowalski at April 14, 2004 05:57 AM
* * * *

At <http://www.prweb.com/releases/2004/3/prweb114697.htm> is an interesting letter **from Tom Bearden to Eugene Mallove**, the editor of Infinite Energy. On 3/21/04, in the second half of his letter ,Tom wrote:

. . . The only conventional way to overcome this ordinary "Coulomb barrier" blockage of fusion at low temperature is to go ahead and use high temperature and the resulting very high ion momentum necessary for some ions to penetrate and overcome the normal Coulomb barrier between themselves and their approaching ions, headed at each other and thus colliding. In short, some collision and formation of the necessary "quasi-nuclei" is achieved by brute force temperature and momenta, for some of the ions on mutual collision courses.

Now consider a "reaction reversal zone" up to a cubic micron in size, where indeed the reactions do run backwards negentropically (due to the production of negative work) for up to two seconds. When reactions are reversed, then the law of attraction of charges can also be reversed. In this special zone, momentarily, now the "reversed Coulomb law" is that like charges attract and unlike charges repel -- for up to two seconds and in zones up to a cubic micron in volume. So up to a few dozen billion ions and molecules can be involved in reversals of the coulomb barrier into a coulomb attractor.

Interestingly, the difference between a proton and a neutron is merely the orientation of a single quark. Consequently, theorists need to look into the implications at the quark level when two protons are in such a "negative entropy region" with reactions reversed. In that case, the Coulomb barrier is now reversed between the two protons! It is now the "Coulomb attractor" rather than the "Coulomb barrier". It seems the two protons could now certainly attract each other so closely that each does indeed penetrate to the strong force "deep" region of the other (if things were normal). Further, instead of the "deviation aside" of nominal close misses, the reversed Coulomb barrier can convert a near miss into a collision "hit".

It may also be that the strong force of each particle is also momentarily reduced, depending on the extent of reversal action on the gluon forces and on the orientation of the quarks. At any rate, it appears that a "quasi-nucleus" of two H+ ions can form, with the probably "flipping" of one quark in one proton to turn that proton into a neutron, lowering the excitation. That would be the formation of a quasi-nucleus of deuterium. Then as the transient thermodynamic fluctuation reverses in sign and things move back toward equilibrium, the strong force would again resume its strength (much stronger than the now emerging Coulomb repulsion between the two protons). The notion is that the quasi-nucleus of deuterium would just "tighten" into a normal deuterium nucleus, or just a D+ ion.

At least this notion of a reversal of the Coulomb barrier and a reversal of the law of attraction and repulsion of charges,

precisely fits theknown fact that negative entropy, reversed reaction zones do occur and have been experimentally demonstrated by thermodynamicists completely independently of cold fusion experiments. This then lends yet one more powerful argument that cold fusion can and does occur under the proper circumstances, and those circumstances may necessarily include the proven "reversal of reactions" that occur in such thermodynamic reversal zones that experimentally violate the second law of thermodynamics by producing negative work, negentropy, and reversal of the Coulomb barrier into a Coulomb attractor.

In our book, Energy from the Vacuum, Cheniere Press, 2002 we also listed candidate "reversed reactions" that would well occur in such fluctuation zones, and that would yield the experimentally observed alpha particles, tritium, etc. in the experiments. These suggested "reversed reactions" are based on the temporary "reversal" of the law of attraction and repulsion of charges, occurring in one of the thermodynamic reversal zones that have been experimentally demonstrated by thermodynamicists. As is well known, the occurrence of such excess deuterium, tritium, and alpha particles is icommon to a great many of the successful cold fusion experiments conducted in multiple laboratories by many researchers, in multiple nations of the world.

Anyway, let us fervently hope that the DoE gives a very rigorous and very fair review and appraisal of the cold fusion situation. And let us hope they also take into account the very important and pertinent transient fluctuation thermodynamics work and its production of significant "reversal zones", as shown by researchers such as Evans, Searles, Rondoni, Wang, et al.

My comment:
Another possibility is that potential energy curves (plotted against distances) might be more complex than a textbook curve with a single maximum. A V(r) curve, especially in a solid, may have two, or more, peaks separated by local minima. It is conceivable that a local minimum, (or a sequence of minima on the outer side of the coulomb barrier hill) play an important role in cold fusion. Fission isomers, discovered in 1960s, (by Polykhanov?) were explaied in terms of such potential energy minima.

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139) Suggestions for the DOE investigators

Ludwik Kowalski (4/25/04)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

While waiting in suspense for the pending DOE investigation of cold fusion, I would like to speculate about what I might do if I were in charge of the project.

1) By my definition: "Cold fusion (CF) is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutrons, cosmic rays, alpha particles or stellar temperatures." Unfortunately the term "cold fusion" means different things to different people. It is certainly premature to define cold fusion as a practically unlimited energy resource. But that is what most people say when asked to describe CF. I suspect that some disagreements about CF would disappear if the above definition were accepted by all antagonists.

2) Is the so-called “excess heat,” reported by many investigators, real or not? I would not focus on this question, unless the heat were generated reproducibly at the rate of 10 W or more. Excess heat per se is not a signature of a nuclear process; heat generated at the rate of one watt or so can be due to parasitic chemical reactions or to other hard-to-identify effects. The most convincing argument against CF, in the 1989 ERAB report (1), was the lack of evidence for nuclear reaction products accumulating at rates commensurate with excess heat. This argument, according to many cold fusion researchers, is no longer valid. They claim that in some experiments ^{4}He has been observed to accumulate at the rate of about one atom per 23 MeV of excess heat. This claim would be the first to investigate on my agenda.

3) In practical terms I would proceed as follows. First I would select a research team claiming to be able to conduct the experiment. The Japanese team headed by Arata, for example, would be a good candidate; they did build the apparatus and described highly reproducible experiments on accumulation of ^{4}He (2). I would consult many cold fusion researchers before selecting a team of proponents. Then I would select a highly qualified team of scientist who are highly critical of the reports published by proponents. I would call them opponents. Again, I would consult many cold fusion researchers before selecting a team of opponents. Proponents would be asked to perform experiments in presence of opponents. The claim would be validated if opponents were satisfied with conclusions of proponents. In case of a disagreement each team would be asked to write a report. Another group of experts would read the reports and decide the outcome.

4) What else is worth investigating? The number of claims is large and one must be selective. The most convincing nuclear signature in a CF setup, as far as I am concerned, were dramatic changes in isotopic ratios of trace elements accumulating in CF setups. It is conceivable that some of these elements result from redistribution of contamination but dramatic changes in isotopic ratios can not be explained in that way. To investigate the claim I would proceed, more or less, as described in 3 above.

5) For practical reasons I would limit the number of claims to investigate to three. The third claim would be the unexpected emission of nuclear particles, at very low rates, as reported by Jones (3). The rates of emission are too low to produce measurable heat. This, however, does not mean that the confirmation of the claim is not important. It might indicate that a nuclear process similar to hot fusion (emission of neutrons, protons, tritons and ^{3}He) can take place at ordinary temperatures. The name “cold fusion” would be appropriate for this kind of reaction, but not for other cold fusion processes.

6) What makes cold fusion possible at room temperatures? Is it screening by electrons or minima near the top of the

potential-energy-versus-distance curve? Such questions would be left for later studies. Another question to be left for further study has to do with our model of stellar energy generation. Is it conceivable that cold fusion also contributes to generation of heat inside stars. Is it conceivable that the sun emits fewer neutrinos than expected because not all of its heat is generated through fusion reactions with which we are familiar? Yes, I know that this is only a speculation. But who said that cold fusion is not possible at stellar temperatures?

7) I would totally ignore proposals dealing with practical applications at this time. Such proposals would evolve naturally if cold fusion were no longer blacklisted. The main overall task would be to decide about validity of scientific claims in the area of cold fusion. And about quality of research. Were methods of validation, in the area of CF, different from those used in other areas of science or not? Were major CF researchers qualified in their sub-disciplines? Was there any evidence of fraud or mutual deception?

8) Only highly respected scientists, preferably Nobel prize winners, would be selected to evaluate reports, and to announce decisions, which either confirm or refute claims made by cold fusion researchers. Time limit to reach decisions, and material support to conduct experiments, would be generous. Recall that the 1989 ERAB report was published only nine months after the discovery of cold fusion was announced. In my opinion this was not appropriate.

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140) Seven papers from Kasagi

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This morning I received an e-mail message from Kasagi in Japan. He wrote: "Recently, I have found your www site on cold fusion by chance. And I knew, in the essay, that our works on low energy nuclear reaction have been cited several times. I appreciate it very much. I noticed that some of our papers are not in your hands; for example, that gives a detailed description of the experiment, spectra of observed particles, and so on. Thus, I decided to send them. Please find attached files (pdf files), which are our published papers on LENR in refereed journals." I am pleased that a scientist like Kasagi read my entries on this site. What follows are my short summaries based on fast reading of these seven paper. Dr. Kasagi saw the draft of this unit and made several small corrections.

At the same time (5/4/04) he wrote: "I started this kind of research in 1989, of course, after the big announcement [of the discovery of cold fusion]. Since I thought that low-energy nuclear reactions in materials were not investigated, I decided to measure DD reaction rates in metals by lowering incident energies as low as possible. The paper #2 is based on the work in 1989-1990 by using an ion source at Tokyo Institute of Technology where I was working at that time. I also started to observe reaction products during and after deuteron bombarding on metals. The paper #1 is based on the work in 1990-1994 by using a small Cockcroft-Walton machine in Tohoku University, where my friend was working for the Cyclotron laboratory, and, later on, I moved to the Laboratory of Nuclear Science (we have electron machines here). Thus, what led me to the cold fusion field was the original works of Fleishmann-Pons and Jones, although the first publicatio on this subject in the refereed journal was delayed too much."

Paper #1 (1995)
(1) J. Kasagi, T. Ohtsuki, K. Ishii and M. Hiraga, Energetic Protons and alpha-particles Emitted in 150-keV Deuteron Bombardment on Deuterated Ti, J. Phys. Soc. Jpn, 64, 777-783 (1995)

Deutrons of 150 keV were used to bombard a thick target of Ti loaded with D. In addition to expected 2.45 protons from the D(d,p)T reaction they discovered unexpected protons of 17 MeV and alpha particles of 6.5 MeV. The Ti, before loading it with D, was 99.5% pure and chemical composition of the remaining 0.5% (mostly O and Fe) was shown in the table. These people know that extraordinary claims call for extraordinary evidence and they investigated many kinds of scenarios, such as reactions in impurities, reactions due to neutrons, protons and ^{3}He. Here is one example. The dominant D(d,^{3}He)n reaction produces neutrons and ^{3}He. Thus targets were exposed to deliberately stronger doses of ^{3}He and n in order to show that not enough alpha particles were produced. Some protons could be explained as due to the D(^{3}He,p)^{4}He reactions (whose Q value is 18.35 MeV) but most high energy protons must be due to something else, claim the authors. Similar discussion was presented to rule out a possibility that most of the 6.5 MeV alpha particles are due to known reactions.

I was pleased to read this paper because I am familiar with instruments described by authors. They are similar to instruments I used in postdoctoral work at Columbia University about 35 years ago. It is interesting that the word "cold fusion" is not mentioned. Was it because the topic was considered pseudoscientific in Japan (as described in Mizuno's book) or because this work led the authors to cold fusion? In order to interpret the results they speculate that (for unknown reason) deuterons in Ti are sometimes much closer to each other than distances between Ti atoms. That would allow the D+D+D --> p+n+ a reaction in which the available energy of 21.62 MeV is shared among three products. The authors conclude: "However, this possibility requires an anomalously large enhancement factor which is not understood at present."
P.S.

The term LENR (low energy nuclear reactions) certainly appropriate for this study. But the term CF (cold fusion) is not appropriate because, by my definition in unit # 136, "Cold fusion is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutron sources, stellar temperature temperatures, cosmic rays or alpha particles." Yes, formation of compound nuclei can be called fusion (of two nuclear drops) but even 150 keV is not "cold."

Paper #2 (1995)
J. Kasagi, T. Murakami, T. Yajima, S. Kobayashi and M. Ogawa, Measurements of the D+D Reaction in Ti Metal with Incident Energies between 4.7 and 18 keV, J. Phys. Soc. Jpn. 64, 3716-3722 (1995)

This paper is also devoted to D+D reactions in Ti but the energies of projectiles are lowered to 18 and 4.7 keV . Reaction cross sections, in that energy region, are very small, due to the Coulomb barrier. But they are known to be larger than expected. This effect has been attributed to screening (lowering of Coulomb barriers caused by orbital electrons. That is why the distance hydrogen nuclei in Ti and Pd are much shorter than H_2 molecules. Studying of screening effects was the motivation behind this investigations. Is this a camouflage for the "forbidden word" - cold fusion? The lowering of the Coulomb barrier is expressed in terms of so-called astronomical factor, S, (obtained by dividing a measured reaction cross section by the Gamow factor and by the incident energy). The main conclusion of this work is that screening in Ti is not significantly different from screening in a gas target (reported by other researchers).

Paper #3 (1997)
H. Yuki, T. Sato, T. Ohtsuki, T. Yorita, Y. Aoki, H. Yamazaki, J. Kasagi and K. Ishii, Measurement of the D(d,p)T reaction in Ti for 2.5 < Ed < 6.5 keV and electron screening in metal, J. Phys. Soc. Jpn. 66, 73-78 (1997)

This paper is also devoted to the study of D(d,p)T reaction in Ti but beam energies were between 2.5 and 6.5 keV. This was possible because the new accelerator was able to produce beams up to several hundreds microapmers (instead of only 2 in the previously used machine). Slight enhancement of cross sections (over those in bare D+D reaction) was noted. It was interpreted as caused by the electron screening. The first reference is for the "effect of the electronic environment on nuclear phenomena." It is G.T. Emery in Annu.Rev. Nucl. Sci. 22 (1972) 165.

The second reference is the 1989 CF paper of Fleischmann and Pons. The authors gave a very good description of controversies in that field; they wrote: "The so-called cold fusion has roused attention more generally on the influence of the environment where nuclear processes take place. Although most of the experiments reported at that time were known to have a difficulty in the reproducibility and, hence, to be under suspicion, the influence of the environment in various nuclear processes is one of the interesting subjects which need more study, because of its interdisciplinary nature involving nuclear physics, condensed matter physics, material science, and so on. In addition, one can develop its applications in various fields if the electron environment really affects the nuclear processes very strongly."

The paper focusses on difficulties of obtaining reliable data on cross sections in the region of very low energies. Overcoming some of these difficulties the authors demonstrate the enhancements of cross sections below 4 keV. The value of the screening potential for the D+D in Ti is reported as 19 eV, plus or minus 12 eV.

Paper #4 (1997)
H. Yuki, T. Sato, T. Ohtsuki, T. Yorita, Y. Aoki, H. Yamazaki and J. Kasagi, The D+D reactions in metal at bombarding energies below 5 keV, J. Phys. G: Nucl. Part. Phys. 23, 1459-1464 (1997).
This paper, published in an journal is devoted to the same topic as the previous paper. It shows that enhancements of D+D reactions in Yb are stronger that in Ti. The screening potential in palladium is reported as 81 eV, plus or minus 10 eV.

Paper #5 (1998)
H. Yuki, J. Kasagi, A.G. Lipson, T. Ohtsuki, T. Baba, T. Noda, B.F. Lyakhov and N. Asami, Anomalous enhancement of DD reactions in Pd and Au/Pd/PdO heterostructure targets under a low energy deuteron bombardment, JETP Letters 68 (1998) 823-829.

This paper is devoted to the same topic as the previous two papers but the metal is Pd (palladium). The enhancements in Pd (250 eV, plus or minus 15 eV.) was found to be much stronger than in Ti and Yb. Even more pronounced enhancements (601 eV) is reported for Au/Pd/PdO heterostructure. The authors believe that conductive electrons (due to mobility of D ions in Pd) might contribute to very large enhancements in Pd and in Au/Pd/PdO sandwich.

Paper #6 (2002)
J. Kasagi, H. Yuki, T. Baba, T. Noda, T. Ohtsuki and A.G. Lipson, Strongly enhanced DD fusion reaction in metals observed for keV D+ bombardment, J. Phys. Soc. Jpn, 71 (2002) 2881-2885.
This paper elaborates on results obtained in the previous one and provides additional data on enhancements in PdO. The authors begin by showing how the maximum possible deuterium density depends on temperature in several metals. In Ti, for example, the density is about $4*10^{22}$ atoms per cubic centimeter while in Au it is about two times less. In Pd, on the other hand, the density is about ten times smaller that in gold. In all cases the saturation density decreases with the temperature, as illustrated in Figure 1. Great effort was made to keep the deuteron densities stable during bombardments with highly intensive deuterium beams. The discussion focusses on the observed correlation between the deuterium densities (smaller in Pd) and screening potentials (largeer in Pd). The authors speculate that low density may indicate increased “fluidity” which in turn leads to a screening caused “not only by electrons but also by positive ions.”

Paper #7(2004)
J. Kasagi, Y. Yuki, T. Baba, A. Taguchi, M. Shimokawa and W. Galster, Strongly enhanced Li + D reaction in Pd observed for deuteron bombardment on PdLix with energies between 30 and 75 keV, J. Phys. Soc. Jpn. 73 (2004) 608-612.
This very recent paper paper also indictaes that “the enhancement in the Pd case cannot be explained by electron screening alone.” The conclusion is reached by analyzing the energy dependence of cross sections for Li(d, a) reactions occurring in host metals (Pd and Au). Beam energies were between 30 and 75 keV. Screening in PdLi (~1500 eV) was found to be much stronger than in AuLi (~60 eV). As in the previous paper, the authors speculate that high fluidity of Li ions in the Pd host might be responsible for the strong enhancement.

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141) A paper from Dubna in Russia

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About a month ago I sent an e-mail message to a large number of cold fusion researchers in Europe. It was about the pending reevaluation of cold fusion by the DEO. F.A. Gareev read my message. On April 28, 2004 he wrote to me: "Dear Professor Ludwik Kowalski, Thank you very much for extremely important information about cold fusion. I was involved in this field starting from 1989 when information we get in NBI in Copenhagen where I spend some few years. I now nearly finished **review about cold transmutation in Russia** in russian and some my mechanisms cold transmutation. If you are able to read in russian I will be happy send for you my review paper and 2 original one. So give me your usual mail address. Professor of Physics, Gareev Fangil A."

The copy of the paper, published in 2003 (in "Geoinformatica, #1, p 51), arrived this morning. What follows is my translation of the first two and of the last sections. I do not want to translate the main section of the article because I am not familiar with that field of theoretical physics. The e-mail of the first author is gareev@thsun1.jinr.ru

P.S.
On May 11, 2004, he sent me a much longer paper (also in Russian) entitled "**Influence of excitation and ionization of the atoms on the velocity of nuclear processes at low energies.**" This 47-pages-long paper contains 246 references. The authors are: F.A. Gareev, I.E. Zhidkova and Yu.L. Ratis. In the summary they write: " We have concluded that cold transmutation of nuclei is possible in the framework of the modern physical theory excitation and ionization of atoms and universal resonance synchro-nization principle are responsible for it. Investigation of this phenomenon requires knowledge of different branches of science: nuclear and atomic physics, chemistry and electrochemistry, condensed matter and solid state physics,... The results of this research field can provide a new source of energy, substances and technologies."

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Mechanism of cold transmutation of chemical elements

F.A. Gareev, G.F. Gareev, I E. Zhidkova
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1. Introduction

Several publications describing low energy transmutations of atomic nuclei appeared recently. Such transmutations have been observed in electric explosions of thin metallic foils in liquids (1), in water solutions of salts exposed to ultrasound (2), in melting of zirconium (in a vacuum) by bombarding it with electrons (3), and in lead traversed by high electric current in a strong magnetic field (4). Neutrons were observed during electroacustical cavitation (5). It is significant to emphasize that cold transmutations of atomic nuclei consist an exothermic reactions in which the released energies

(MeVs) exceed the interaction energies (eVs and keVs) by 4 to 6 orders of magnitude. No gamma rays or alpha particles are emitted in these reactions. Theoretical objection against cold transmutations of atomic nuclei are based on “missing protons,” on low penetrability of Coulomb barriers, and on the absence of nuclear particles. The purpose of this work is to show how the unexpected nuclear processes can be explained in terms of resonance synchronization and resonant tunneling.

2. Mechanism of cold transmutations

Transmutation reactions must be isothermic because external influences causing them are weak. The energy must be conserved; the released energies are due to mass differences between the input and output channels. The “missing protons” objection can be removed by assuming that changes in Z are due to weak interactions:

$$A(Z,N) + n*e^- \rightarrow Z(Z-n,N) + nn_e,$$

and

$$A(Z,N) \rightarrow Z(Z+n,N) + ne- + nn_e,$$

where n is the number of leptons. Note that ‘ and “ refer neutrinos and antineutrinos, respectively. Probabilities of such interactions are extremely low and an amplifying mechanism must exist to make them observable. Such mechanism exists: it is the universal principle of resonant synchronization (7,8,9,10) and of resonant tunneling (11).

3. Principle of resonant synchronization

4. Conclusions

To summarize let us say that resonance synchronization and resonance tunneling amplify weak, acoustical and electromagnetic processes to such extent that cold transmutations of atomic nuclei become possible, at least in principle. Resonant amplification of the rate of transmutation depends only on the frequency of external interactions, it does not depend on the nature of these interactions. In recent experiments

In recent experiments large clusters of 100 to 1,000,000 atoms were observed to interact with laser beam pulses of very short duration. Energies of ions emitted from such clusters were shown to increase with sizes of clusters. In some cases atomic nuclei emitted ions whose energies approaching 1 MeV; such energies are sufficiently large to produce fusion. According to (18), for example, the $D(D,n)^3He$ reaction does take place when deuterium clusters interact with ultraist laser pulses. The authors concluded that the cross section of that fusion reaction increases rapidly with the size of the cluster.

The mechanism of cold transmutations consists of resonant amplification of reaction rates resulting from synchronized external interactions and from formation of large nuclear clusters. This can easily be verified by performing relatively simple and inexpensive experiments. The way is open for production of new chemical elements, and for generation of energy from radioactive waste. It turns out that different kinds of external interactions lead to similar results, as far as cold transmutations are concerned.

5. References

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142) In memory of Eugene Mallove

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Eugene Mallove, the author of “Fire from Ice,” is dead. He was killed yesterday. It was “a homicide and an investigation is underway," according to Steven B. Krivit. Steven is another young journalist trying to promote the reevaluation of cold fusion; his e-mail message made me aware of Eugene’s death this morning. It was a real shock. I met Eugene at the 10th International Conference on Cold Fusion. We had several conversations. We disagreed on some topics but I was very impressed by his personality. We lost an admirable fighter and a master of carefully chosen words. He died without knowing the outcome of the pending DOE reevaluation of cold fusion. It was Eugene who made me aware, two months ago, that the reevaluation project was informally announced.

Here are nice words that Steven Krivit wrote about Eugene. **”Gene is survived by his wife Joanne, his son, Ethan, and his daughter, Kim..... We believe that Gene Mallove will one day be recognized as one of the greatest science journalists of his time. He was an authoritative historian and innovative investigator within the new energy field. Through his 1991 book, "Fire from Ice: Searching for the Truth behind the Cold Fusion Furor," Gene was the first to courageously and boldly express the reality of cold fusion, long before any other science journalist dared to ask questions that challenged the prevailing view. In 1996, he reached an even broader audience with his highly educational video, ‘Cold Fusion: Fire From Water.’**

For 15 years, he worked tirelessly and relentlessly, at great personal sacrifice, to foster and oversee the development of cold fusion and other new energy experimentation at his New Energy Research Laboratory. He published Infinite Energy: The Magazine of New Energy Technology, which provided a "torch" of information that drew and enlightened many to the existence and legitimacy of cold fusion and other emerging new energy technologies. Most recently, he established the New Energy Foundation, a non-profit corporation, which provides grants to researchers and developers in the field of new energy.

Gene will be remembered for the immense support and encouragement he provided to cold fusion researchers around the world, whose progress and achievements he recognized, chronicled and publicized through a wide variety of media. His tremendous efforts have shown increasing success over the past few years and continue to bear fruit, as is evidenced by the growing media attention in support of cold fusion. Gene's generosity toward the scientific community, his commitment to the integrity of Science, and his dedication to creating a better world, will be forever appreciated and celebrated.”

Let me add that Mallove’s book is availble at <http://www.amazon.com>;I strongly recommend it for school libraries. Here is fragment from one of the reviewers of this fascinating book. “**In Fire from Ice,**

astronautical engineer and well-known author, Eugene Mallove, sheds a new and very different light on the cold fusion confusion. Based on personal interviews with many of the people involved, as well as his firsthand experiences in laboratories and scientific conferences, he offers a unique insiders view of that divisive controversy, while at the same time clearly explaining the relevant science and technology. And Dr. Mallove convincingly argues that cold fusion may yet prove to be real. A story of scientific ambition and professional rivalry, political intrigue and hard science, Fire from Ice is the fascinating account of one of the most intense and momentous scientific controversies of all time.

And another book reviewer wrote: "**"Mallove brings dramatically to life the human side of this important scientific controversy, which has tapped the emotions of its scientific participants in a way usually typical of major scientific revolutions. Fire from Ice is highly recommended reading for anyone who is interested in the nature of scientific controversy and scientific change. I frankly could not put the book down once I started it."**

In an Internet testimony his friend, Mark Plotkin writes: "**His greatest accomplishment and legacy will be the 52 bi-monthly magazine issues of Infinite Energy that he founded and edited. Infinite Energy magazine is a compendium of scientific research into all branches of unconventional energy research from contributors around the world. Many of the authors simply couldn't get published elsewhere, but had the courage and foresight to get most papers peer-reviewed before they were published. His magazine has thousands of loyal subscribers from over 40 countries including Russia and China. See www.infinite-energy.com.**

Gene traveled to dozens of international conferences, most of the time at great personal sacrifice simply to network with energy researchers and benefactors from around the world. He knew nearly everyone in the unconventional energy community worldwide. Gene has been a champion of cold fusion for many years highlighted by his organizing last summer's successful (International Conference on Cold Fusion) ICCF-10 held in Cambridge, Massachusetts from 24- 29 August 2003. Gene's presence has been the rare voice of scientific reason in a field filled with many phony claims and charlatans. See the web page with the experiments at http://www.lenr-canr.org/Experiments.htm.

Equally significant but perhaps not as well known is the *New Energy Foundation*, which he founded with the help of a generous wealthy anonymous benefactor. The New Energy Foundation has become a science-based clearinghouse for generating much needed funds for promising energy research leading to commercialization.

His latest triumph was to reverse over a decade of ignorance at Department of Energy by presenting compelling evidence of anomalous reactions of Low Energy Nuclear Reactions. DOE's decision to review the files on LENR aka Cold Fusion could possibly open a new area of scientific inquiry that has been closed since 1989. This was a triumph not just for Gene personally, but for every scientist who spoke as lone voices in large auditoriums. Gene's voice gave courage to those brave individuals who toiled in their laboratories, struggling to survive with virtually no funding, yet many developed innovative ways produce low energy nuclear reactions (LENR). Researchers often put their careers in jeopardy and only approached this field when retirement was assured.

Gene tirelessly climbed every mountain with courage and grace articulating truth in a field meriting

serious scientific investigation of anomalous energy phenomena despite constant criticism from ignorant skeptics who refused to examine even the best peer-reviewed data. One of the best examples of his fearless battles was when he reviewed Professor Bob Park's book “Voodoo Science.“ This bloody battle was over paradigm shifts in science revealing the complete ignorance of Bob Park by his refusal to consider any peer-reviewed data.

Gene's vision was of a world with abundant energy produced without fossil fuel or nuclear waste. It is now up to us to fearlessly make that vision a reality.

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143) Social Aspects: Two Items

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Item 1.
In browsing the Internet I found this page:

http://www.wpi.edu/Academics/Depts/Chemistry/Courses/CH215X/coldfusion.html

It is an old document, entitled "The Cornell Cold Fusion Archive" written by Bruce V. Lewenstein. The date is not specified; I think it was posted in 1994. The author asks questions belonging to the realm of "science and society". I think that these questions are worth thinking about. What follows are very extracts from his paper.

Introduction
". .....Many analysts have attempted to draw morals from cold fusion, labeling it "pathological" or worse, while others have pointed to the ongoing investigations and challenged critics to approach cold fusion with the openness that is claimed to be the hallmark of scientific inquiry. But we should look at the cold fusion saga neither as an episode of error nor as an exemplar in science. Instead we learn much more by considering what it tells us about science at the end of the twentieth century. Among the key issues:

a) What role does the press play in modern science?
b) Does the press merely disseminate the results of experimental investigations, or does it become an active player in the scientific process?
c) Was its behavior in the cold fusion saga somehow different than in other scientific episodes?
d) What changes in science communication does cold fusion foretell?
e) What is the place of politics in science? Are politicians just players on the outskirts of scientific investigation--or are they integral to modern scientific work?
f) What does politics in science accomplish?
g) How important is competition between research teams?
h) Is science fundamentally a search after nature's truths, or is it a game with winners and losers?
i) How do patents affect research?
j) How can a field that puts so much emphasis on priority of discovery deal with instances of double discoveries?

k) How can scientific commitments to open communication be reconciled with intellectual property rights?
l) And, perhaps most fundamental, what does it mean for science when experiments are unclear? If modern science depends on the belief that an experiment can yield an answer, how can it deal with ambiguity and incoherence in the research process?

Some scientists hope that cold fusion is unique. But I believe that the opposite is true. Virtually nothing about cold

fusion is new or different. Instead, the cold fusion saga simply illustrates much of what we already know about science--about how researchers interact with the media, with politicians, with the patent system, with each other, and with nature. We can use the history of cold fusion as a window through which to view modern science.

The History of Cold Fusion

. . . . Recollections differ on the outcome of the March 6 meeting. Jones believes the two groups agreed to simultaneously submit manuscripts to *Nature* , with no public statements before the submission on March 24. Pons and Fleischmann have said that they planned on the simultaneous submission, but made no commitment to staying quiet.

In the event, they didn't. A week after the meeting, they submitted a "preliminary note" on their work to a specialty journal that promised them quick publication. And the day before they were to meet Jones at the airport Federal Express office for the simultaneous submission, they announced their findings by press conference. Fleischmann showed up at the airport as scheduled, but Jones didn't; by then, furious at what he believed to be a broken agreement, he had faxed his manuscript to *Nature*

According to Frank Close, a British theoretical physicist whose book on cold fusion, *Too Hot to Handle* , is critical of Pons and Fleischmann, this prehistory explains much of the chaos that followed. Close argues that the competition between the groups fueled the secrecy and speed with which they worked, leading to incomplete experiments, misinterpretations that persisted because they were never disclosed and debated with other scientists, and various other mistakes. On the other hand, the succor which the groups provided to each other--after all, each of them seemed to offer independent verification of the same results--also explains the certainty with which each side proceeded to defend its findings.

. . . The findings of the two groups were not identical. Jones claimed--and in 1993 continues to believe--that his apparatus yields signs of a small amount of nuclear fusion, an amount so small that it comes nowhere close to 'break-even,' the point at which an experiment produces more energy than it consumes. Pons and Fleischmann, on the other hand, claim that their experiments produce more heat (and thus energy) than can be explained by the chemical reactions occurring in the cells. Pons and Fleischmann acknowledge that their experiments do not produce the typical signs of fusion--neutrons, gamma rays, tritium, and helium in amounts correlated with excess heat. They argue that a "hitherto unknown nuclear process"--a new form of fusion--must be causing the heat.

But whatever the findings of Pons, Fleischmann, and Jones, they were not the only actors in the cold fusion drama. Within days of the press conference, scientists around the world had taken up the challenge posed by the two teams, and attempted to replicate the experiment themselves . . . The first widely-publicized confirmation of heat production came on April 10 from Texas A&M University in College Station, Texas. On the same day, a team of researchers at the Georgia Institute of Technology in Atlanta announced that they had detected neutrons in a cold fusion cell, confirming that fusion was occurring. (The Georgia Tech team publicly withdrew its claims within a few days; with less fanfare, the Texas A&M group later did the same.) By the end of April, a dozen laboratories from around the world had publicly announced partial confirmations of some aspect of cold fusion; at least 40 articles had been submitted to refereed journals on cold fusion.

. . . The president of the University of Utah, who had already secured a $5 million commitment from his state legislature, asked for $25 million from the federal government. On May 1, at an American Physical Society meeting in Baltimore, physicists turned the tables on the chemists, producing a string of negative evaluations of cold fusion in a marathon session that began in the early evening and ran until well past midnight. A similar session took place the next evening, as well. The APS meeting marked the beginning of the end of media attention to cold fusion. But the scientific controversy rolled on. . . .

Understanding the Cold Fusion Saga

Cold fusion skeptics have called this sequence of events everything up through fraud, pointing to abundant evidence of sloppiness and error in many of the experiments. True believers prefer to highlight new experimental work and recall the admonition that all scientific knowledge is tentative and temporary. But both of these approaches are normative attempts to say what science ought to be. Instead, we should ask, What does cold fusion tell us about science as it really is ? What, after all, is unique about cold fusion?

It's not the presence of the press. The media have been covering science closely for most of this century, with the active support of the scientific community. In the United States, the American Chemical Society, the American Physical Society, and the American Association for the Advancement of Science are just three of the mainstream scientific organizations that devote significant fractions of their resources to cultivating press coverage of their fields. Press conferences have been used in the past, and will be used in the future, to present research both before and after it is published in the peer-reviewed literature.

A content analysis of the media coverage of cold fusion conducted by my students suggests it matched media coverage of other scientific topics. It met journalistic standards of "balance"-- that is, it reported both (or more) sides of the story. It rarely sensationalized. It was essentially accurate, erring primarily by the sin of omission. While scientists may not always be happy with the way science is covered by the media, many observers have come to understand that publication in the worlds of journalism and science have different meanings. Journalists will never provide the details scientists want, and scientists will rarely look for the "story" that excites journalists. Given the constraints under which science journalists operate, they did a competent job with cold fusion.

. . . One unusual aspect of cold fusion was the media's role as an active information broker. In several cases, reporters served as go-betweens for scientific groups that were not in direct contact.....Science communication is a complex, multifaceted, multidirectional process that involves many actors in various reciprocal arrangements. The media comprise merely one set of actors in the process. One new aspect of science communication in the cold fusion saga was the role of electronic communication (faxes, electronic mail, and computer bulletin boards) in the ongoing distribution of information about cold fusion. When preprints of manuscripts by the BYU and University of Utah teams became available about a week after the public announcement, copies of the preprints were faxed and (after having been scanned into computer-readable format) e-mailed around the world.

. . . Another area in which cold fusion is sometimes considered unique: competition. But competition between research teams is not new. In 1991, *Science* magazine featured an article on the ways in which teams try to beat each other to press, to shave just a few days off the publication process, in order to win to themselves the kudos of their peers. The physicist Philip Morrison has recently suggested that science is like sports. One implication is that while some parents may try to shield their children by saying that "doing your best" is the goal, we all know that for many people in sports, winning isn't everything, it's the only thing. The same holds true for many scientists. They are in the game because they want to win.

. . . The implication of the experimenter's regress for modern science is multifaceted. If science is not the objective investigator of nature that its proponents often assert, then what is the basis of its claim to a privileged role in our daily affairs? If, on the other hand, we say that nature greatly constrains the possible judgments that a researcher (or a citizen) can make, even in the face of experimental uncertainties, how should we deal with arguments that push the limits of the uncertainties? Cold fusion is not unique in raising these questions; it simply puts them in bold relief.

Conclusion

. . . Cold fusion doesn't teach us anything that we don't already know about science. But perhaps the saga of cold fusion itself may be explained by the confluence in a single case of all the things we already know. And since the same confluence of media, patents, controversy, and politics shows up in other recent examples of high-profile science (high temperature superconductors are probably the best example), it seems likely cold fusion is merely a harbinger of confluences to come. In other words, cold fusion is a prototypical example of the contemporary social context of science.

Item 2.

In browsing the Internet I also found this page:

http://www.newenergytimes.com/Audio/2003NagelAug25-2003PPP.mp3

It is a sound file, a public lecture of David Nagel at the 10th International Conference on Cold Fusion. Some social issues are touched upon. I was in the audience and my voice can be heard. I tried to compare cold fusion with

Manhattan project. The number of scientists working on cold fusion, worldwide, is about the same as the number of those who worked on Manhattan project. And caliber of scientists were more or less the same. How come, I asked, that in fifteen years cold fusion researchers could not produce a single item to convince other scientists that a new phenomenon was discovered? David said that those who developed atomic weapons had unlimited governmental support while those who discovered cold fusion were declared to be pseudo scientists. One can note that the efforts of weapon scientists were highly organized while the efforts of cold fusion researchers were not coordinated by an army general. Each of these two factors (support and coordination) is important.

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144) Catalytic Mechanisms

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Fusion of two deuterons into one helium atom, in hot plasma, is a rare process (one out of million fusion events). It is an exothermic process; the released energy of 23.7 MeV appears in the form of gamma rays. In condensed matter, on the other hand, production of helium has often been reported as a dominant fusion event in which the released energy appears in the form of heat and not in the form of gamma rays. What is the mechanism of nuclear fusion of deuterons in condensed matter? According to R.H. Parmenter (1) cold fusion in metallic lattices can be a two-step process whose steps are:

$^{1}H + {}^{2}H \rightarrow {}^{3}He + 5.4$ MeV

and

$^{3}He + {}^{2}H \rightarrow {}^{4}He + {}^{1}H + 18.26$ MeV

Note that common protons, presumably dissolved in the metallic lattice, play the catalytic role; they are used in the first step and released in the second step. The author claims that that the probability of the Coulomb barrier penetration, for the first step of the above sequence, is six orders of magnitude higher (but still very small), than for the direct fusion mechanism ($^{3}H + {}^{2}H \rightarrow {}^{4}He$). The ^{3}He produced in the first step (of the above sequence) has sufficient energy to overcome the Coulomb repulsion hindering the second step.

A sequence of two nuclear reactions, producing two ^{4}He atoms (and 22.3 MeV of energy), is also discussed. In that reaction neutrons play the catalytic role; ^{6}Li is supposed to be introduced into the lattice from the LiOD or LiOH salt dissolved in the electrolyte. The envisioned two-step process is:

$^{6}Li + n \rightarrow {}^{3}H + {}^{4}He + 4.8$ MeV

and

$^{3}H + {}^{2}H \rightarrow {}^{4}He + n + 17.5$ MeV

The issue of the critical mass (in a real cell not all neutrons produced in step 2 would react with ^{6}Li), and the issue of the absence of gamma rays, are not addressed by the author.

References

1. Parmenter, R.H., *Enhancement of Cold Fusion Processes in Palladium by Catalytic Agents.* Infinite Energy, 2002. 8(43): p. 66.

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145) Non-equilibrated solids

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A typical distance between the atoms of a metallic crystal is 0.1 nm (one angsrom). The same applies to ions of hydrogen dissolved in metals when saturation densities (about one ion of hydrogen per atom of a metal) are reached. At such distances the attractive nuclear forces are too large to overcome the repulsive electric forces. That has been the main theoretical argument against cold fusion since its experimental discovery has been announced 15 years ago. But cold fusion has been observed by many investigators. The only way to consolidate theoretical arguments with massive experimental data, according to D. Cravens and D. Letts, is to postulate that cold fusion takes place at locations in which local conditions are far from equilibrium.

What follows is the first section of their practically-oriented paper. The authors refer to excess heat but their observations are probably applicable to all LENR-CANR processes {"Low Energy Nuclear Reactions and Chemically Assisted Nuclear Reactions" is a new (very auckward) name for what is commonly known as "cold fusion."}

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Cravens, D. and D. Letts.

Practical Techniques In CF Research - Triggering Methods. in *Tenth International Conference on Cold Fusion*. 2003. Cambridge, MA: [The entire paper can be downloaded from the library at http://www.LENR-CANR.org.]

1 Background

1.1 Static Equilibrium Often Produces Sporadic Results

The study of nuclear events at low temperatures often has resulted in frustrating investigations. The field of cold fusion has often been marked with sporadic and non-reproducible work. Critics have often pointed to the sporadic nature of the heat generation in electrolytic systems as indication of poor experimental procedure. However, it now seems that the sporadic nature of the results is a characteristic of an electrolytic system, which is initially near equilibrium, and slowly loaded to a transition point which is best described by the mathematical term as a chaotic transition. For example: slowly loading palladium can be driven between beta and gamma states and cause internal fluxes of deuterium.

Electrolytic cells using bulk palladium often require loading times of 10 to 20 times longer than would be expected by diffusion times of deuterium within the metal before they be expected to produce excess heat.[1] This was likely the cause of failure of early researchers who rushed to replicate Fleischmann's and Pons' early work.[2] In the first few years after the announcement, it was easier for a researcher to rush to print and claim negative results than to patiently wait until the system was fully loaded and driven into internal transitions that drive the reactions. As a result, early work more often than not failed to see excess heat.

This work will illustrate methods that will help drive CF systems off equilibrium and trigger internal events that lead to production of excess heat. The viewpoint taken here is that a system must be allowed to depart from static equilibrium before the required reactions can take place.

1.2 Theoretical Limitations

Most simple theoretical models fail to predict that nuclear reactions within a deuterated metal lattice can take place at significant rates. Such models rely on reaction rates that are based on equilibrium placement of deuterium within a metal lattice or on wave functions based on such placements. In particle models, the global average of the deuterium density within the metal is on the order of an Angstrom or more even for extreme loading ratios of D/Pd. It is clear that deuterium at such remote nuclear separations would not be expected to lead to nuclear events.

The imposition of dynamic conditions can cause the local separations of deuterium to be significantly different from the value predicted by the global density alone. It also seems that dynamic conditions provide ways for coupling of energy to drive the reactions and impurities within the lattice can allow for spin exchanges required for spin selection rules. It is a surety that the energy required to drive any nuclear events and energy released from such events are much larger than any external energy available to the deuterium based on a per atom division of energy.[3] This means that any external energy driving the possible nuclear events must act in a coherent way to channel energy from a large region of many atoms to the active sites.[4,5,6] This coherent channeling must involve over 10^8 atoms and likely many more. The experimental conditions then must make use of non-equilibrium events acting on a system that has some group coherent nature. The methods described here are simple and practical methods that can be used to produce such dynamic conditions, which may lead to the desired nuclear events. The assumption here is that the reactive nuclear species must be driven to a dynamic active state before the desire events can produce excess energy within the system. . . .

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146) Scientific or not scientific?

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The following extracts (about scientific methods and pseudosciences) were found in browsing the Internet. I think that they are worth sharing. Keep in mind, however, that any wikipedia article can be changed by any reader. Thus what I extracted today may no longer be there tomorrow.

FROM ***http://en.wikipedia.org/wiki/Scientific_method***

1) The ***scientific method*** is a sequence or collection of procedures that are considered characteristic of scientific investigation and the acquisition of new scientific knowledge based upon physical evidence. This method is believed to distinguish science from other intellectual traditions, such as painting, philosophy or theology.

2) In the twentieth century Karl Popper introduced the idea that a hypothesis must be falsifiable ; that is, it must be capable of being demonstrated wrong. ..... Today falsifiability is often cited as a main distinction between science and pseudoscience.

3) The essential elements of the scientific method are traditionally described as follows: (a) Observation, (b) Hypothesis/Prediction, (c) Experiment (d) Collect data, (e) Conclusion and (f) Repeat. This can be called the hypothetico-deductive method. These activities do not describe all that scientists do. [It is an idealized description, not all discoveries are made in this way.] Science is a social activity, and one scientist's theory or proposal cannot become accepted unless it has become known to others (usually via publication, ideally peer reviewed publication), criticised, and finally accepted by the scientific community.

4) A hypothesis is a suggested explanation of the observations. It needs to be consistent with the phenomenon or set of facts observed. Sometimes this is nothing more than a "guess," especially in the case of students. Scientists use whatever they can—their own creativity (currently not well understood), ideas from other fields, induction , or even systematic guessing, or any other methods available—to come up with possible explanations for the phenomenon under study. There are no definitive guidelines for the production of new hypotheses. The history of science is filled with stories of scientists claiming a "flash of inspiration", or a hunch, which then motivated them to look for evidence to support or refute their idea.

5) A specific prediction should arise (as a logical consequence of the hypothesis), that can be put to the test of an experiment, which should allow concrete measurements. If results contradictory to the predictions are found, the hypothesis under test is incorrect or incomplete, requiring either revision or abandonment. If results consistent with the hypothesis are found, the hypothesis might be correct, but is always subject to further tests. Deductive reasoning is the way in which predictions are developed with which to test a hypothesis.

6) Experiments, whether widely accepted or not, should be performed by many different scientists so as to guard against bias, error, misunderstanding, fraud, etc. Those that seem to call into question, or even force rejection of, an existing previously satisfactory theory should be especially carefully checked. Scientific journals use a process of peer review ,

in which scientists' papers describing experimental results and their conclusions are submitted to a panel of fellow scientists for evaluation.

Scientists are rightly suspicious of results that do not go through this process. For example, the cold fusion experiments of Fleischmann and Pons were never peer reviewed—they were announced directly to the press before any other scientists were able to evaluate their efforts or reproduce their results. Their results have not been reproduced elsewhere else in the decades since [is this true?]; the press announcement was regarded at the time, by most nuclear physicists, as very likely wrong. Peer review may well have turned up problems and led to a closer examination of the experimental evidence Fleischmann, Pons, et al. believed they had found. Paul Kammerer 's experiments on acquired physical traits in amphibians (described in Arthur Koestler 's The Midwife Toad) seem to have been deliberately faked, while the confusion in the 60s and 70s about ' polywater ' seems to have been the result of micro contamination. Much embarrassment and wasted effort might have been avoided by proper peer review in many such cases.

7) The primary constraints on science are: (a) Publication, i.e. Peer review and (b) Resources (mostly, funding). It has not always been like this: in the old days of the "gentleman scientist" funding (and to a lesser extent publication) were far weaker constraints. Both of these constraints indirectly bring in the idealised method - work that too obviously violates the constraints will be difficult to publish and difficult to get funded.

8) The study of the scientific method is distinct from the practice of science and is more a part of the philosophy, history and sociology of science than of science itself. Such studies have limited direct impact on day-to-day scientific practice.

9) Scientific Method is often touted as determining which disciplines are scientific and which are not. Those which follow the scientific method might be considered sciences; those that do not are not. That is, method might be used as the criterion for demarcation between science and non-science.

10) In his book The Structure of Scientific Revolutions Kuhn argues that the process of observation and evaluation take place within a paradigm. 'A paradigm is what the members of a community of scientists share, and, conversely, a scientific community consists of men who share a paradigm' (postscript, part 1). On this account, science can be done only as a part of a community, and is inherently a communal activity. For Kuhn the fundamental difference between science and other disciplines is in the way in which the communities function.

FROM **http://en.wikipedia.org/wiki/Pseudoscience**

1) A pseudoscience is any body of knowledge purporting to be either both factual and scientific, or of an even higher standard of knowledge , but which fails to comply with scientific method. Motivations for the advocacy or promotion of pseudoscience range from simple naivety about the nature of science or of the scientific method, to deliberate deception for financial or other benefit. Some people consider some or all forms of pseudoscience to be harmless entertainment. Others, . . .consider all forms of pseudoscience to be harmful, whether or not they result in immediate harm to their followers.

2) Pseudoscience is distinguishable from revelation ,theology or spirituality in that it claims to offer insight into the physical world by "scientific" means (i.e., most usually in accordance with the scientific method). Systems of thought that rely upon "divine" or "inspired" knowledge are not considered pseudoscience if they do not claim to be scientific or to overturn well established science.

3) Pseudoscience also differs from protoscience . The latter may be defined as speculation or hypothesis which has not yet been tested adequately by the scientific method, but which is otherwise consisent with existing science or which, where inconsistent, offers reasonable account of the inconsistency. Pseudoscience, in contrast, is characteristically wanting adequate tests or the possibility of them, occasionally untestable in principle, and its supporters are frequently strident in insisting that existing scientific results are wrong.

4) The boundaries between pseudoscience, protoscience, and "real" science are often unclear to non-specialist observers.

They can even be obscure to experts. Many people have tried to offer objective criteria for the term, with mixed success. Often the term is used simply as a pejorative to express the speaker's low opinion of a given field, regardless of any objective measures.

5) After more than a century of active dialogue, the question of what marks the boundary of science remains fundamentally unsettled. As a consequence the issue of what constitutes pseudoscience continues to be controversial. . . . Examples of fields of endeavor that many consider – to varying extents – pseudoscientific include Cold fusion ,Pseudoarchaeology ,Gene Ray 's Time Cube ,astrology and homeopathy.

6) There are also young fields of science that are sometimes frowned upon by scientists from established fields, primarily because they are speculative in nature. [For example]: (a) exobiology /astrobiology , (b) Search for Extraterrestrial Intelligence (SETI) and (c) Communication with Extraterrestrial Intelligence (CETI). These fields are not considered pseudoscientific or protoscientific by most scientists, though, and they are studied at many universities and specialized institutes.

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147) EXTRACTS FROM A GOOD OLD PAPER

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Four years ago **Edmund Storms** published a long review paper entitled "**A Critical Evaluation of the Pons-Fleischmann Effect**." It appeared in two parts (1); the paper is also available over the Internet (2). In the abstract of the paper the author wrote: "Many new studies are available to make an objective evaluation of the Pons-Fleischmann effect possible. The phenomenon is conventionally known as "cold fusion," or "chemically assisted nuclear reactions (CANR)" when the environment is emphasized, or "low-energy nuclear-reactions (LENR)" if emphasis is placed on the process. A wide range of observations involving anomalous production of energy as well as nuclear products have been published. While many of the claims are still open to interpretation, the general conclusion is that an important, novel phenomenon has been discovered which deserves renewed interest." Below are arbitrarily selected extracts from this paper. Many findings described at the 10th anniversary of the announced discovery of cold fusion have been confirmed in subsequent five years. That is what, I suppose, prompted the pending DOE evaluation of cold fusion. Storms was probably an important promoter of this initiative.

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1) Ten years of work worldwide have produced over 2500 published papers[5], many peer reviewed, which have answered most of the objections leveled by critics. It is now possible to make a more objective evaluation of the phenomenon than was previously possible. Unfortunately, during this time the claims have been the subject of considerable distortion. The reader is asked to lay aside the emotional reaction "cold fusion" can generate and read the following arguments with an open mind. Some readers will be put off by the clear conflict between claimed behavior and well accepted theory of nuclear interaction presently in vogue. The author is well aware of this problem. Nevertheless, a large collection of anomalous behavior must be explained. The fact that this behavior cannot be explained by conventional theory should only serve to challenge theoreticians rather than be used as justification for rejection. .....The field which is conventionally called "Cold Fusion" has grown and now should be called "Chemically-Assisted Nuclear Reactions" or "Low-Energy Nuclear Reactions," depending on the emphasis one wishes to apply. The claims are supported by a wide range of anomalous behaviors involving nuclear reactions and energy production.

2) Numerous critics have observed that "extraordinary claims require extraordinary proof." This is a very high standard which has prematurely doomed many new ideas to the trash bin, some deserving and some not. We need to realize that potential errors can be found by clever critics in any study, no matter how well done. Hence, a perfect proof is almost impossible to obtain until considerable information has accumulated. Such an accumulation is very slow and difficult if an idea is completely rejected, as has been done in this case. Consequently, at the very least, I would hope that the skeptical reader would entertain the possibility that some part of the claims deserve further study, even though all important questions have not, as yet, been answered.

3) If the process does not involve conventional fusion, what is producing the anomalous energy? Critics attribute the extra energy to error in measurement or normal, overlooked chemical processes. This assertion will be addressed in the following section. Four questions must be answered when evaluating this experimental work: (a) Was the calorimetric technique used by Pons and Fleischmann sufficiently stable and accurate to see the claimed extra energy? (b) Can

prosaic sources of chemical energy be ruled out? (c) Have other people replicated the claims using sufficiently stable and accurate calorimeters? (d) Have reasons for success or failure been discovered?

4) Before answering the first question, a general comment about the calorimetric method is necessary. Critics have misunderstood the difference between relative and absolute calorimetry in their search for errors.[12] Although the absolute approach was used during early measurements in order to detect an immediate production of energy, this method was later found to be unnecessary, because production of energy was found to require a long wait. This delay allows a null condition to be defined, independent of absolute knowledge of power production. Any random variation in the signal is also revealed during this interval. In addition, repeated calibration during this delay can be used to demonstrate that the calorimeter is stable. Consequently, referencing the data to an initial zero value, as is done in most publications, is convenient but not necessary.

Instead, power production is claimed when the signal rises above a previously steady but arbitrary background value. Seeing a signal rise out of the noise is easy and does not depend in any way on knowing the actual amount of heat being produced in the cell. The magnitude of the resulting energy is then calculated using the *change* in cell conditions and the calibration constant. All absolute values of error in temperature, voltage, current, and other conditions cancel, provided the values for these quantities have remained constant. As a result, the observed presence of excess energy is more certain than is its absolute value. If an error is to be found, it must explain how a sudden change can occur in previously stable conditions and how this change can be missed by repeated calibrations and studies of inert materials, so-called control experiments. Nevertheless, some overlooked errors are important and will be discussed.

5) Electrolytic action decomposes heavy water to give D_2 and O_2, which carry away chemical energy upon leaving the cell. Should some fraction of these gases unexpectedly react to reform water within the cell, the actual amount of energy being generated in the calorimeter would be uncertain. Kreysa *et al*.[13] immediately raised this issue because P-F did not address the problem in their original paper. Later P-F [14] stated that the amount of recombination was determined by measuring the amount of water which disappeared from the cell as a function of time and applied current, and that the amount was insignificant.

6) Overlooked by these studies is another possible source of error. Knowledge of the amount of energy being produced in such a calorimeter requires the thermal conductivity of the wall be known and stable. Stirring changes the effective thermal conductivity of the wall because the amount of stagnate fluid next to the wall is changed. This factor is very sensitive to the amount of fluid convection next to the wall and shows no saturation as convection is increased.[25] This effect can be seen in Figure 3 which shows the relationship between effective thermal conductivity of the cell wall as a function of stirring rate. Even mechanically stirred devices will suffer an error if the stirring rate should change. Whether this effect is important depends on the frequency of calibration, the type of calibration used, and the constancy of bubble action. Therefore, the effect of this error on the various studies using the isoperibolic method is hard to judge. To the extent that it operates, one should see an apparent positive as well as negative anomalous energy. All reported data show only positive excursions.

7) Three studies have evaluated the basic design of the P-F calorimeter. The first was commissioned by General Electric Co. and reported by Wilson *et al.* [26]. The two most serious problems they note are the change in calibration constant produced by liquid level change, and potential loss of heavy water with the evolving gases, thereby producing a loss of overlooked energy. The authors acknowledged that P-F avoided the effect of these errors, as well as several others, by frequent calibration. Thus, changes in cell conditions could be quickly eliminated as being the cause of anomalous energy. Although the authors claimed to find some minor mistakes, the final conclusion of this analysis is that only the magnitude of the anomalous energy can be questioned by their analysis, not its existence. Because their attempts to duplicate the claimed energy production using various types of palladium, cell designs, electrolytes, and anode metals were unsuccessful, they were not optimistic about the reality of the claims.

8) Before going on to examine attempts at replication, it is worth first discussing the P-F measurements in detail because they were the focus of so much criticism. Figure 4 shows a drawing of the P-F cell. In this case, electrolytic action occurs in a cell which is surrounded by a vacuum jacket that is silvered except near the top. Thus, the thermal barrier is located at the top of the cell, including the lid. This assembly is placed in a constant-temperature water bath. Contained in the cell along with the anode (Pt) and cathode (Pd) are a glass-covered heater, a single glass covered thermistor, and a

reference electrode. P-F answered some of the criticisms in several papers[30; 31; 32] from which the following observations can be summarized: Currents between 25 mA and 804 mA were used with most measurements taken above 100 mA. Thus, most measurements were outside of the critical current range for internal recombination- . Furthermore, they state that the amount of recombination was measured by monitoring the amount of D_2O used. Recombination was found to be no greater than 1% of applied power. Mixing was found to be adequate to eliminate temperature gradients based on experimental observation as noted above. with the resulting temperature rise and decay noted. This type of calibration allowed pulsed as
well as sustained heat production to be evaluated.

9) The hard-to-accept claim for a nuclear source is based, in part, on the belief that observed energy production exceeds any known chemical source. Therefore, the potential chemical sources must be examined. Before discussing this subject in detail, the reader should realize that a typical cell contains very few chemical components, all of which are stable with respect to each other. A chemical reaction can only be initiated by applying an electric current, a process which uses energy. Only after the water has been split into deuterium and oxygen can chemical reactions occur. This process causes several chemical reactions, including an uptake of deuterium by the palladium and slow deposition of lithium and platinum on the cathode surface.

Each of these reactions involve very little energy. Strain energy accumulates in the palladium and small amounts of reaction products such as D_2O_2 can accumulate in the solution under proper conditions. These processes have the potential to store energy within the cell. Only release of stored energy can be used to explain the anomalous energy, which appears after many hours of electrolysis. The magnitude of such processes was addressed in several papers. Kainthla *et al.*[37] discussed eight possible sources, including recombination, which has been already discussed above. The other sources are the energetics of PdD formation, the energetics of PdLi formation, and energy accumulation as stress. Each of these was found to be much too small to account for even the smallest reported excess energy. Handel[13].

10) These [calorimetric setups] share the following features: [44]; (a) The cells are sealed and contain a recombiner. As a result, no gas leaves the cell. Therefore, uncertainty in the amount of recombination is not an issue. Successful action by the recombiner is monitored using different methods including the change in gas pressure. (b) The cells contain a heater which maintains a constant inner temperature. Power to this heater is adjusted to compensate for any change in power production within the cell produced by electrolysis or by anomalous processes. This heater is also used to determine whether the power measurement, based on the flow rate and temperature change of the cooling fluid, is accurate. A sensitivity of better than ±0.01 W (±0.1%) is claimed. (c) The electrolytic cell, its surrounding heater, and the cooling-fluid channels are all contained within a silvered, evacuated dewar in order to isolate them from the environment. (d) The whole assembly is immersed in a fluid bath which maintains a constant environment of 30±0.003°C. This bath is also the source of cooling fluid. Consequently, most studies are done at a constant temperature of 30°C. (e) A constant flow pump is used to circulate cooling fluid. Flow rate is checked periodically by weighing the fluid. Better than 98% of power produced within the cell is captured in this fluid. (f) All aspects of the measurement are under computer control, which provides continuous monitoring, and redundant RTDs are used for temperature measurement. (g) The deuterium content of the palladium cathode is determined by measuring its change in resistance. (h) Most studies involve a similar calorimeter containing an inert cathode as a reference. Both calorimeters are run electrically in series and measurements alternate between the two systems using the same voltage and current meters.

Flow calorimetry is relatively simple and suffers from fewer errors compared to the isoperibolic method used by other people, as well as by P-F. Only four physical measurements are required. The applied power is determined by measuring applied voltage and current at the isothermal boundary, and the released power is obtained by measuring the flow rate and the temperature change of the cooling fluid. Internal temperature gradients are not important, stirring is not an issue, and uncertain recombination is not a source of potential error. Only unexpected changes in the measuring systems can introduce error. McKubre *et al*. have demonstrated their instruments to be stable and accurate through years of use. On the other hand, an evaluation of this study can assume prosaic errors which might have been overlooked in spite of this experience, as several skeptics have attempted to do. Two possibilities will be examined. . . .

11) Figure 5 compares many studies, some of which show a clear relationship between applied current density and heat

production. In addition, an example obtained by McKubre *et al.* is shown in Figure 10. Similar studies by Storms [50] and by Hasegewa *et al.*[48] are shown in figures 9 and 11, respectively. Indeed, even P-F mentioned this behavior but their advice was largely ignored by those who suffered failure.

What prosaic process could cause the apparent excess energy to rise as applied current is increased? Increased current has three major effects. It increases the AC component being measured in the DC current and voltage, increases the number of bubbles, increases the amount of energy being dissipated by the cell, and increases the chemical activity of deuterium at the cathode surface. The effect of an error in the measured current and voltage has been discussed above. Increased bubble production will cause increased mixing. For those studies using an isoperibolic calorimeter, this can only have a minor effect as explained above. Flow-type calorimeters would not be affected at all. Increased production of heat could have an effect if the calibration constant were not constant, but changed as the electrolyte temperature changed. Failure to recognize this nonlinear behavior could produce an error at temperature or current extremes. The author has seen this behavior after the current was applied to an isoperibolic calorimeter for a long time. Apparently, the path for heat loss can change, thereby changing the behavior of the calibration constant. Figure 8. Effect of average bulk composition on production of excess power using palladium wire. However, the effect is small, causing less than a 0.5 W error. Repeated calibrations, as is done in many studies, would reveal the problem. This effect would not apply to a flow-type calorimeter such as used by McKubre *et al*. because such calorimeters are largely immune to where heat is being produced within the cell.

12) The effect occurs in only a small fraction of samples but more often in certain batches than in others. Only a few organizations have the funding to allow many samples to be investigated for heat production. SRI [51] studied 176 samples with 19 giving excess energy. However, many of these studies were for the purpose of learning how high loading could be obtained rather than Figure 11. Effect of applied current on excess power shown by Hasegewa *et al.*[48] seeking excess energy. The successful samples correlated to a high degree with the average composition and many came from the same batch. Takahashi [52] reports studying twenty plates, only three of which gave excess energy in the 3-5 watt level. Both Storms[53] and Kobayashi *et al.*[54] were able to replicate excess energy production using material from the same batch of palladium which produced excess power for Takahashi. Ota *et al.* [55] studied 79 samples over a nine year period and found fourteen to give less than 0.25 W, 5 to give power between 0.25 W and 0.50 W and only three to produce greater than 0.50 W. The rest showed no sign of excess energy. Storms studied fourteen plates and found only six that produced excess power in the 1.1-4.5 watt range. The effect correlated with the amount of cracking experienced by the samples and the average composition. Miles *et al.*[56] found that 20% of their samples gave excess energy with a high fraction coming from a few batches.[57] The results are listed in Table 2. Those samples containing boron or cesium produced
a high success rate, while other samples and alloys were frequently dead. It is important to note that all samples supplied to

Miles by P-F produced excess energy. In addition, a sample which was found to produce excess energy at China Lake (U.S.) also produced excess when it was studied at the NHE Laboratory in Japan.[58] Thus, once again, the effect could be duplicated when the same material was used. This effect is attributed to the variable nature of palladium metal, especially because certain batches give a very high success rate. However, the behavior can also be explained by assuming poor stability in the calorimeters used, thus the insistence by critics that blank, control cells be studied. Unfortunately, no one working in this field has had funds to support an extensive study of blank cells, although most workers have studied a few such samples. Such blanks normally use platinum in place of palladium and H_2O in place of D_2O. When blank cells were studied, no excess energy is reported.[59] Many skeptics discount this claim by not trusting the experimenters to objectively evaluate the results or they attribute the claimed excess power to chance variations in the measuring system.

13) The above requirements provide many avenues for failure. Success, first of all, requires palladium which is able to achieve a critical composition at the surface.[60] The average bulk composition is only important because it is required to support this high surface composition. Unfortunately, most palladium forms cracks when it loads with deuterium so that the deuterium escapes faster than it can be delivered.[50] Only a small fraction of available palladium does not show this behavior. Second, the heavy water must be free of normal water to prevent deuterium in palladium from being diluted by normal hydrogen.[61; 62] Because heavy water quickly picks up normal water from the air, it can easily become diluted and made inactive. Once potentially active palladium has been acquired[63], it must be handled

correctly.

This includes making sure the surface is free of finger prints and other contaminants as well as scratches. Annealing must be done in a very good vacuum to prevent formation of even a monolayer of surface impurity. The ease with which palladium can suffer surface contamination is one of the important problems which is frequently overlooked. Crystal size is also thought to be important, a property which is strongly influenced by annealing. Treatment with Aqua Regia is sometimes needed to remove unavoidable surface films. After these pretreatments, the material must be subjected to proper loading conditions. Applying only a small current for the first several days improves the chance of reaching a high composition. Only after the composition has been achieved at stable value should the current be increased into the critical range. Too fast loading or premature application of high current can produce cracking, followed by immediate loss of deuterium.[51; 64] Palladium is much more sensitive to how it is treated than most people realize. These requirements were not known by most early workers in the field, hence success was more a matter of luck than skill. Even now, many attempts to duplicate the claims do not apply these lessons. Unless this experience is applied, a failed effort cannot be claimed as a true duplication.

14) Clearly, anomalous energy is not a product of a conventional fusion reaction, nor does it show the behavior found during "hot fusion." Nevertheless, anomalous emissions have been detected on numerous occasions, including neutrons, X-rays, g-rays, charged particles, as well as radiation from radioactive products. While such radiation along with production of radioactive and nonradioactive products suggest anomalous nuclear activity, this paper will not attempt to assess these claims. Helium is another possible nuclear product which can be produced by several nuclear reactions besides fusion. This element was looked for and found by numerous investigators in several different environments including in the gas[56; 65; 66; 67; 68; 69; 70], dissolved in the materials[71; 72; 73; 74; 75; 76; 77], and emitted as charged particles[78; 79; 80]. Naturally, not all studies are definitive and some failed to find helium when it was sought. While these observations are suggestive, only two independent measurements have provided a quantitative relationship between anomalous power production and helium production rate.

Both studies used all-metal systems and measured helium in the flowing gas generated during continuous electrolysis. Two different calorimeter types were used and the helium was measured at two different laboratories. These two studies are compared in Figure 12. Three conclusions can be drawn from the figure. First, the two studies agree very well, given the difficulty of the measurement. Second, the He/sec-watt values are largely independent of observed anomalous power, as would be expected if the two quantities are functionally related. Third, the average values are within a factor of 2 of being consistent with an energy of 24 MeV/helium atom, the value expected when ^{4}He is occasionally produced by conventional fusion. . . .

Thus, anomalous helium was found only when anomalous heat was detected. Only one cell, which used a Pd-Ce alloy, showed heat but no helium. This result is strongly against chance alone. When the helium producing branch of the fusion reaction has been previously observed using conventional fusion, a 23 MeV gamma emission has been detected. This radiation results because fusion of two deuterons produces only one product nucleus. Gamma emission is required to conserve momentum. Because this gamma energy is not detected during claimed anomalous energy production, most critics dismiss the claimed helium as being an artifact. The other two branches more frequently observed during conventional fusion are apparently not the source of significant energy in this environment. This distortion of expectations also adds to the skepticism.

15) To be successful, a theory [explanation of cold fusion] must answer at least five basic questions to explain the P-F effect and several other questions if the entire range of published observation is to be explained. (a) What mechanism allows the Coulomb barrier to be overcome? This question is basic and will have to explain how nuclei as heavy as palladium can suffer a reaction with nuclei as heavy as oxygen, in addition to the proposed fusion between deuterium nuclei. (b) What mechanism distributes the released energy throughout the lattice rather than requiring it to be focused on a few individual particles? This mechanism must also explain why some nuclear energy is retained by the nuclear products when these products are produced very near a surface. Otherwise, charged particles having significant energy to leave the material would not be detected. (c). How is the proposed mechanism related to the physical environment? Most present theories assume the nuclear reactions occur in b-PdD having a composition near PdD1.0. The model must explain why anomalous reactions occasionally involve other materials and why the required conditions are so difficult

to achieve. (d) What nuclear reaction is the source of observed helium? Fusion is not the only conceivable source of helium as a nuclear product. (e) If helium results from a fusion reaction, what mechanism allows conservation of momentum and energy, and what mechanism distorts the reaction paths to produce helium rather than neutrons and tritium?

16) Calorimetry is a well understood technique which has been applied in various forms for over a hundred years. A vast literature of chemistry and physics is based on such measurements. While measurement of power at the microwatt level is a challenge, measurement of watts, as is being done here, is not considered difficult. Prof. Hansen has suggested that a calorimeter cannot be trusted unless it has demonstrated accuracy in measuring the heat from a known reaction. This is a fair request provided absolute calorimetry is used. However, as noted at the beginning of this review, relative measurements are actually being made. Stability is the only requirement, a condition which is much easier to evaluate and much less prone to hidden error. This is not to say that all claims for anomalous energy are correct or accurate. The question which must be examined is whether some studies are sufficiently correct and accurate to demonstrate the claims to be highly probable, not necessarily absolutely certain. When such evaluations are made, the critic needs to keep in mind the potential magnitude of suggested errors. Just because an error can be imagined and justified does not mean it can explain multiwatts of apparent power production. In general, the magnitude of the effect has frequently overwhelmed any plausible errors or prosaic explanations. The statement that the claims are not convincing is often heard. While this euphemism is actually a gentle way of saying, ”I just don’t believe you,” one needs to ask just what is not believed and just what deserves additional study. Is it rational to reject everything just because some part does not make sense? Would it not be better to support some focused work on the subject to answer a few basic questions? What are we to make of the consistent patterns of behavior as well as the influence of material properties and the presence of helium, a possible nuclear product? Is it reasonable to believe that numerous independent studies show the same patterns just because of chance? While such arguments are not a proof, they are commonly used by prudent people to evaluate all aspects of life. Indeed, this is the rationale behind requiring many duplications of a claim, a condition which has been met in this case. When a new phenomena is evaluated, a belief system based on probabilities needs to be adopted. An absolute rejection or acceptance is not useful. The issue is whether the likelihood of the phenomena being real is sufficiently large so as to justify further work. In the case of the CANR claims, I suggest further study is justified.

17) CONCLUSION: The claims for anomalous energy production using electrolysis of heavy water have been evaluated and found to have a high probability of being caused by a novel phenomenon. In addition, the most likely source of the heat is a nuclear reaction which produces helium. This nuclear reaction is not normal fusion and it does not follow the rules required by conventional theory. Numerous models have been proposed to explain the observations, but none at the present time can account for all of the reported behaviors. More work is required to determine which of the behaviors are part of this novel phenomenon and which can be explained by ordinary processes. However, the claims have now reached a level of understanding which justifies a reexamination of the published work and attention by the scientific community.

References

1. “A Critical Evaluation of the Pons-Fleischmann Effect (Part 1), E. K. Storms, Infinite Energy **6**, #31, p10 and #32, page 52 (2000).

2. Available from the library of the LENR-CANR web site <http://www.lenr-canr.org>

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148) ON DIFFICULTIES COMMUNICATING

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Introduction
Chemists and material scientists are often more familiar with physics, than physicists are with chemistry or material science. That is why reports written by chemists or material scientists are likely to be frustrating reading to a physics teacher, such as myself. This is unfortunate because cold fusion is a field involving three disciplines. This essay illustrates the kind of frustration I encounter. It is not a criticism of the paper that I am using as an example; that paper was written by an expert--Edmund Storms--and it was presented at a scientific conference attended by other experts. I suppose that most of them could find the answers to my questions "between the lines" of the reports (they are interested in what is new and not in what has already been debated). Experts communicating across the borders of scientific disciplines should be aware that their messages are not always clear to others.

How It Started
In browsing the Internet I found the following e-mail message (January 28, 2000), authored by E. Storms.

"I would like to invite the resident skeptics and curious to view the results of a successful cold fusion experiment, in particular a study of the Pons-Fleischmann Effect. ...... To give you a little background, I have been studying the P-F effect for about 10 years starting at the Los Alamos National Laboratory. During that time I have seen anomalous energy production from Pd-D_2O cells having values over a range up to 7.5 watts. Recently, I have constructed several flow-type calorimeters having much better sensitivity and stability than the isoperibolic type I was using. Consequently, I can detect and believe lower excess power levels, values I would have ignored before. Recently, several samples produced anomalous power and will be described in detail at ICCF-8. I hope this rare success will stir up some interest and suggestions. Please feel free to give it your best shot. "

Fortunately, Storms' ICCF-8 paper can be downloaded from the Internet. Google pointed to that paper after the following search phrase was specified:

E. Storms, 2000. "Excess Power Production from Platinum Cathodes Using the Pons-Fleischmann Effect," in *Eighth International Conference on Cold Fusion.* 2000 Lerici (La Spezia), Italy: Italian Physical Society, Bologna,Italy.

What follows is the text of that important paper. I am showing it to identify places at which I faced conceptual difficulties. My numbered questions are inserted in red capital letters. The figures, and the list of references, are skipped to shorten the description. I notice the term "Pons-Fleischmann effect' in the title of Storms' paper. Presumably this refers to generation of unexplained exess heat. That is not the same thing as as the term "cold fusion," invented long before 1989, and defined as a nuclear process taking place in condensed matter. The two are likely to be related but this should not prevent us from saying that the P-F effects belongs to chemistry while the CF effects belong to physics. Division of labor along traditional lines is useful.

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EXCESS POWER PRODUCTION FROM PLATINUM CATHODES USING THE PONS-FLEISCHMANN EFFECT

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ABSTRACT
Excess power was produced using a platinum cathode. Efforts to produce active cathodes by plating palladium onto various metals were largely unsucessful.

INTRODUCTION
Palladium has been the cathode of choice since Pons and Fleischmann made their original claims. Occasionally, anomalous energy has been claimed to result from other elements such as Pt[1; 2], Au[3], Ti[4]. Thin layers of palladium on various inert substrates have also been claimed to produce anomalous energy [5; 6; 7; 8]. From this collection of experience, one might conclude that any layer of Pd made to stick tightly to the surface of another material would produce energy with greater ease than the bulk metal. This assumption has been found to be false even though such material can achieve a D/Pd ratio greater than 1.5[9]. Layers of electroplated Pd can be just as difficult to reproduce as bulk material. Although several successful samples were made, this paper will describe only one example of uncoated platinum which produced excess energy after being electrolyzed for an extended time in LiOD+D_2O.

EXPERIMENTAL

Calorimeter Design:

The calorimeter, shown in Fig. 1, consists of a Pyrex glass cell surrounded by a watercooled jacket. This assembly is contained in a vacuum dewar, thereby allowing most of the energy lost through the lid to be picked up by the cooling water. A magnetic stirrer is used to stir the electrolyte, thereby reducing temperature gradients.

(1*) AT WHAT RATE WAS THE LIQUID HEATED BY THE STEERER?
(2*) WHY SHOULD I ASSUME THAT IT WAS A NEGLIGIBLE RATE IN COMPARISON WITH THE EXCESS POWER (EP~ 0.1 W)?

The entire assembly along with all reference resistors is contained in a constant temperature environment. Table I lists values and uncertainties for the various quantities. The electrolytic cell contains three linear thermistors within the electrolyte, one near the top of the solution, one near the bottom, and the third just above the cathode. The anode is equidistant (0.5 cm) from the flat plate cathode (1 cm x 2 cm). Temperature of distilled water flowing through the jacket is measured just as it enters the jacket and just as it leaves. Data are recorded every 15 min. using a National Instruments data acquisition system after averaging 15000 values. The flow rate is measured by allowing the water after it leaves the calorimeter to fill a container on a balance while the weight and time are recorded every 120 sec. In addition, the cell contains a Pt-coated-carbon recombiner catalyst

(3*) TO “RECOMBINE” MEANS TO MAKE H_2O FROM H_2 AND O_2 THAT ARE PRODUCED IN THE CELL. (AN EXOTHERMIC PROCESS).

and an exposed Pt wire heater for calibration. Luggin capillaries

(4*) WHAT ARE LUGGIN CAPILARIES?

allow the voltage between a platinum reference electrode and the cathode to be measured.

(5*) WHAT IS THE THIRD (REFERENCE) ELECTRODE FOR?
(6*) WHERE IS THE ELECTRIC DIAGRAM?
(7*) WHICH VOLTAGE WAS USED TO CALCULATE THE INPUT POWER?

Because the cell is connected to an oil reservoir, any gas generated within the cell can be detected by weighing the oil displaced onto a balance.

(8*) WHICH GASSES WERE GATHERED IN THE CELL AND AT WHAT RATES?
(9*) WAS ANY CHEMICAL ENERGY GENERATED IN PRODUCING THESE GASSES?

Samples can be quickly changed or replaced by an inert cathode for calibration.

(10*) SAMPLES OF WHAT?
(11*) WHY ARE SUCH CHANGES OR REPLACEMENTS NECESSARY?
(12*) WHAT ELSE CHANGES WHEN A SAMPLE IS REPLACED?

Lengthy studies of inert platinum show a stability of ±75 mW.

(13*) WHEN IS PLATINUM NOT INERT?

Calibration and Error

A typical calibration for the flow-mode is shown in Fig. 2, using a clean piece of platinum for the cathode.

(14*) WHAT IS FLOW MODE? WHAT ARE OTHER MODES?
(15*) TO CALIBRATE USUALLY MEANS TO ESTABLISH A RELATION BETWEE TWO OR MORE VARIABLES. WHAT VALUES ARE INVOLVED IN THIS CALIBRATION?

Values are taken both going up and going down in applied power in the same manner as the sweeps described later. The standard deviation of the electrolytic values from the least-squares line is ±30 mW

(16*) WHAT IS THE “ELECTROLYTIC VALUE” (EXPRESSED IN WATTS)?
(17*) “THE LEAST SQUARES LINE” OF WHAT VERSUS WHAT?

which is the same as the standard deviation from a constant value when stable excess energy is being observed at low applied power.

(18*) HOW WAS “STABLE EXCESS HEAT” DEFINED OPERATIONALLY?

Figure 1. Drawing of the calorimeter. The electrolyte is 65 ml of 0.3 N LiOD and the anode is Pt mesh. The cell lid is Lucite and the Dewar lid is expanded foam insulation. All thermistors are glass covered. Time to reach a steady temperature is 50 min.

TABLE I
Summary of uncertainties in measured quantities
Water temperature entering the jacket = 20±0.02°
Environment temperature = 20±0.03°
Flow rate = 31.00±0.05 g/min (long term variation)
Precision of current measurement = ±<0.001 A
Precision of voltage measurement = ±<0.001 V
Precision of temperature measurement = ±<0.005°
Absolute accuracy of temperature measurement = 0.1°
Stirring rate = 300 rpm ± 1 rpm
Average heat capture efficiency = 98±0.5%

(19*) WHICH VOLTAGES WERE MEASURED AND HOW WERE THEY USED?

This scatter increases to ±0.1 W at the upper limit of applied power (27 W). A zero drift as much as -0.05 W has been observed over an extended time. Consequently, changes in excess energy production are more accurate than absolute values. Good agreement between the electrolytic- and Joule-based calibrations shows that the location of heat production does not affect the accuracy of the device. Doubling the fluid flow from 22.3 g/min to 45.3 g/min caused a change in the calibration constant from 0.0732 W/degree-g/min to 0.0738 W/degree-g/min, indicating that good thermal mixing is achieved in the exiting cooling water.

(20*) WHAT WATTAGE IS USED IN THE UNIT OF CALIBRATION CONSTANT?
(21*) HOW WAS THE CALIBRATION CONSTANT DETERMINED?

Because samples can be easily changed, the cathode is frequently replaced by clean platinum when the need arises to recalibrate. Good stability is shown by a scatter of only ±1.6% in the calibration constant when measured many times over three months.

Figure 2. Comparison between electrolytic and heater calibrations before and after the study using the flow method. The heater and electrolysis agree within 1.2%.

(22*) I SUPPOSE THAT DJ (SEE FIGURE 1) IS THE TEMPERATURE DIFFERENCE BETWEEN WATER-OUT AND WATER-IN. BUT WHY IS IT DJ AND NOT DT?
(23*) IN FIGURE 2 T FOR THE TEMPERATURE "ACROSS THE JACKET."
IS IT THE SAME AS DJ?

The cell can also be used as a rough isoperibolic calorimeter by measuring the average temperature between the electrolyte and the cooling jacket.

(24*) OK, THIS BE THE SECOND MODE OF OPERATION. (FIRST WAS THE FLOW MODE).
(25*) FORTUNATELY, I KNOW WHAT "ISOPERIBOLIC" IS. BUT MANY PHYSICISTS
ARE LIKELY TO BE CONFUSED BY THIS ADJECTIVE.

However, this method is not stable, in spite of active stirring, because of changes in the convection currents in the electrolyte and in the jacket. Figure 3 shows how the average temperature across the jacket changed between the first and final calibration using the electrolytic method and clean Pt.

Figure 3. Comparison between applied electrolytic watts and the average temperature across the jacket for two calibration runs.

Excess Energy Measurement

Figures 4, 5, and 6 show the time history for excess power (EP) production using a special platinum cathode.

(26*) WHAT MAKES THIS ELECTRODE "SPECIAL"?

Current sweeps consist of stepping the current up in value, waiting for the calorimeter to achieve steady-state (50 min), taking five values, and repeating the process.

(27*) HOW WERE THE VALUES OF EXCESS POWER (EP) CALCULATED?
(28*) THE ERROR BARS ARE GIVEN IN TABLE 1. HOW SHOULD I USE THEM TO ESTIMATE ERROR BARS FOR THE VALUES OF EP?

After reaching 3 A, the current is reduced in steps. Notice in Fig. 4 that the expected EP was achieved at 0.5 A and 1.0 A, but decayed away when 1.5 A was applied.

(29*) WHAT IS THE “EXPECTED” EXCESS POWER?
(30*) IF IT MEANS “EXPECTED ON THE BASIS OF ANOTHER REPORT,” THEN WHAT REPORT? IF IT MEANS “THEORETICALLY EXPECTED,” THEN BY WHAT THEORY?

A sweep taken after the decay showed very little EP. After the current was turned off for a brief time, the study was resumed in Fig. 5. Notice that the EP again gradually increased after 0.5A was applied. Sweep #3 again showed EP and this continued while 0.75A and 1.0A were applied. However, application of 1.5A again caused the EP to decay away.

Figure 4. Time history of excess power production from a Pt sample.

Once again the current was turned off. Application of 0.5 A, shown in Fig. 6, again showed EP. The calorimeter was calibrated at 430 h using the internal heater and later using an inert Pt cathode. This experience shows a consistent pattern of behavior which was repeated once again, but is not shown here. The first sweep is shown as applied current vs EP in Fig. 7. Notice that excess power is indicated by the isoperibolic method during this initial sweep, but later sweeps do not show an effect because of a change in calibration, as indicated in Fig. 3. Also notice that the excess power falls on a higher line upon reduction in applied current. Subsequent cycles, as shown in Fig. 8, produce excess power that fall on this higher line. The final relationship is linear and extrapolates to zero EP at zero applied current, in contrast to the behavior of palladium, which requires a critical applied current before EP is produced. Figure 9 shows the sweeps taken later using different numbers of values to produce the plotted average and these are compared to the heater calibration.

Figure 5. Time history of excess power production from a Pt sample.

DISCUSSION

A large average composition within the cathode is thought required to produce excess energy. Yet many researchers have failed to produce excess power after achieving large compositions, for example Nakata et al.[10]. Now, metals that do not even dissolve hydrogen are found to make excess energy. Clearly, additional variables are operating.

Figure 6. Time history of excess power production from a Pt sample.

Figure 7. Comparison between excess energy measured using flow method and isoperipolic method during first current sweep.

In the case of platinum, this study suggests that an energy-active layer of unknown composition can deposit on a Pt surface. This observation might also be related to the frequent detection of Pt on the Pd cathode after excess energy is observed. Such a layer is slow to form, which is consistent with the observed long delay in producing excess energy, and, for this sample, it is unstable. Is it possible that such a layer might be the active material in all studies, even when palladium is used?. McKubre et al. [11] also suggest that a critical layer is required to maintain the required high composition in Pd. Perhaps this is the actual active material. If this is the case, the bulk properties of palladium are only important in that they must support a critical concentration within this layer. Since palladium can easily permit loss into the metal, such material might be the worst substrate to use. Platinum, gold, and other inert materials would appear to be better choices. Use of such materials would only require forming the active layer without a need to use special batches of the substrate. If this suggestion is true, significant changes in various theories will be required.

Figure 8. Applied current vs excess power for the various sweeps, compared to Pt calibrations taken before and after the study.

Figure 9. Applied current vs excess power for sweeps using different number of values to average are compared to the heater calibration.

****

P.S. (6/30/04)

Let me end with a quote from a paper E. Storms wrote for those who are not familiar with excess heat calorimetry:

"Calorimetry is the measurement of heat energy. Although this is an old science that underpins much of thermodynamics and modern chemistry, when such measure-ments are applied to LENR, several unique problems become immediately apparent. For the LENR effect to be initiated, normal energy is frequently required as electric power to produce higher temperature, to generate hydrogen ions, or to stimulate the process in various ways. Consequently, the small amount of anomalous energy being sought is fre-quently superimposed on a large heat flux resulting from normal processes. As a result, the calorimeter must be able to handle large heat flux while being sufficiently sensitive and stable to see the small amounts of anomalous energy. This requirement has severely limited calorimeter design and has complicated evaluation of results. However, as better calorimetry has been applied to the problem, sufficient anomalous power has been re-ported to completely overwhelm uncertainties in heat measurement. This situation con-tinues to improve as ways are found to generate increasing amounts of anomalous power."

The entire article, entitled "Calorimetry 101 for Cold Fusion; Methods, Problems and Errors," can be downloaded from www.lenr-canr.org .

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149) A message from a young outsider

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This morning I received the following an e-mail message from David Pesta:

"I am finally starting to understand what this issue is all about after finding your site. To the lay person, all of the other technical material out there is incomprehensible. Keep up the great work! David."

My reply to this compliment was a simple "thank you." But a little later I sent another message. Here it is: "David: It occurred to me, after replying with one word, that I should ask you for a favor. Can you summarize, on one or more pages, your own experience with the issue of cold fusion? I would be glad to post your piece (anonymously, if you prefer) on my web site. Tell a little bit about yourself, at the beginning, and about your experience with the subject." Several hours later I received a short answer; it is indeed worth sharing. The piece shows how a young high school student learned about cold fusion in 1996. It also shows what he thinks about the field today, after graduating from a university. I hope that there are many open-minded young scientists with similar experience. Will the upcoming DOE investigation be positive enough to encourage some of them to become cold fusion researchers? I hope so. Most of the cold fusion researchers, as far as I know, are in their 60's or older. David wrote:

" Hello; you can post this piece. My experience with cold fusion may not be impressive, depending upon what sort of things you are looking for.

I graduated magna cum laude from Oklahoma State University in 2001 with a degree in biochemistry. I have an A.S. Degree in physics. In 1996 when I was a junior in high school, my chemistry teacher was on a mission to enlighten us on the pseudo-science that pervades society and how to steer clear of baloney. He had a lot of knowledge, but he never did have a good baloney detector kit and seemed to make appeals to emotion rather than reason. I suppose that was suspicion number one. As a part of his mini curriculum, he showed us a television program about cold fusion, I believe it was part of a series by Nova.

After I watched the presentation, I was left with a strong suspicion that there was more to this issue than the program was reporting. Two things went through my mind.

1. After all the negative media coverage, the two scientists (Fleischmann and Pons) admitted some mistakes but still sincerely believed that many of their initial results were genuine. These two scientists were distinguished and seasoned researchers who never exhibited any sort of history indicative of the kind of accusations that they were receiving. They would have known better than to make up fake evidence and hope that nobody would notice as society tried to implement the fake technology. What is the likelihood of formerly reliable scientists out of the blue trying to do something silly like that? That would be like guaranteeing destruction to their reputation and ending their long career as a scientist, and they would have done it deliberately together. No, something fishy is going on here.

2. Other scientists set out to reproduce the experiment in their own laboratories. Maybe the experiments were hard to reproduce precisely and the results were very sensitive to the way the original scientists conducted the experiments. That being a possible scenario, wouldn't it be sensible for the skeptical scientists to visit the laboratory of Fleischmann and Pons and confirm or deny their work directly? If I were a scientist in the field and I heard that someone stumbled across some positive results in their lab over an issue this momentous, I would be the first one on a flight to their location to see it for myself! The film didn't mention any visit of scientists to their laboratory. Considering how

important such a finding would be, it just didn't seem like there was much of an effort placed on reproducing their experiment exactly. To me, it seemed like people were too quick to give up. Cutting federal funds to future research in cold fusion soon after made this
point as well.

These weren't strong reasons to completely doubt the scientific community. But still, for some reason, I walked away from that video with the impression that the scientific community may be missing out on something important and that further investigation should be done before putting the matter to rest. The idea that cold fusion may still be possible remained a thought in the back of my mind ever since.

Last night, while working on a tricky html problem, I was struck with inspiration to see if the scientists of 2004 were making any progress on cold fusion, or if it was a dead field. To my surprise, your website made a lot of issues more clear. I literally read articles all night long and got no sleep, and I haven't done that since the days of college when I was forced to study all night. I am amazed that there is progress that confirms my suspicions that the two scientists Fleischmann and Pons may have actually stumbled onto something. I love rooting for the underdog, especially when there is momentum behind them. I look forward to seeing what actually ends up being discovered, if anything.

Skepticism is great, as long as it is reasonable and not based upon emotion. I do believe that conspiracies happen in American science. Most of them are accidental, but because we are human beings, they happen. And history shows it. Have a great day, David."

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After reading my item #148 Richard Eskimos sent me a set of comments. He wrote:

"I noticed questions about Storms' paper on your website. Since I've been looking at the paper in detail, I may have some of the answers for you. In any case, it will help with my explanation of the Storms/Shanahan question [see item #151]. I've attached the .html file with my answers."

WHAT FOLLOWS IT THE CONTENT OF RICHRD'S FILE. THE ONLY DIFFERENCE BETWEEN ITEM #148 AND THIS ITEM #150 ARE RICHARD'S COMMENTS AND LINKS. THEY FOLLOW MY QUESTIONS PRINTED IN RED. LET ME ADD THAT RICHARD ESKIMOS IS THE AUTHOR OF ITEM #151

150) ON DIFFICULTIES COMMUNICATING

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Introduction
Chemists and material scientists are often more familiar with physics, than physicists are with chemistry or material science. That is why reports written by chemists or material scientists are likely to be frustrating reading to a physics teacher, such as myself. This is unfortunate because cold fusion is a field involving three disciplines. This essay illustrates the kind of frustration I encounter. It is not a criticism of the paper that I am using as an example; that paper was written by an expert--Edmund Storms--and it was presented at a scientific conference attended by other experts. I suppose that most of them could find the answers to my questions “between the lines” of the reports (they are interested in what is new and not in what has already been debated). Experts communicating across the borders of scientific disciplines should be aware that their messages are not always clear to others.

How It Started
In browsing the Internet I found the following e-mail message (January 28, 2000), authored by E. Storms.

“I would like to invite the resident skeptics and curious to view the results of a successful cold fusion experiment, in particular a study of the Pons-Fleischmann Effect. ..... To give you a little background, I have been studying the P-F effect for about 10 years starting at the Los Alamos National Laboratory. During that time I have seen anomalous energy production from Pd-D_2O cells having values over a range up to 7.5 watts. Recently, I have constructed several flow-type calorimeters having much better sensitivity and stability than the isoperibolic type I was using. Consequently, I can detect and believe lower excess power levels, values I would have ignored before. Recently, several samples produced anomalous power and will be described in detail at ICCF-8. I hope this rare success will stir up some interest and suggestions. Please feel free to give it your best shot. “

Fortunately, Storms’ ICCF-8 paper can be downloaded from the Internet. Google pointed to that paper after the following search phrase was specified:

E. Storms, 2000. "Excess Power Production from Platinum Cathodes Using the Pons-Fleischmann Effect," in *Eighth International Conference on Cold Fusion.* 2000 Lerici (La Spezia), Italy: Italian Physical Society, Bologna,Italy.

What follows is the text of that important paper. I am showing it to identify places at which I faced conceptual difficulties. My numbered questions are inserted in red capital letters. The figures, and the list of references, are skipped to shorten the description. I notice the term “Pons-Fleischmann effect’ in the title of Storms’ paper. Presumably this

refers to generation of unexplained exess heat. That is not the same thing as as the term "cold fusion," invented long before 1989, and defined as a nuclear process taking place in condensed matter. The two are likely to be related but this should not prevent us from saying that the P-F effects belongs to chemistry while the CF effects belong to physics. Division of labor along traditional lines is useful.

= =

EXCESS POWER PRODUCTION FROM PLATINUM CATHODES USING THE PONS-FLEISCHMANN EFFECT

Edmund Storms Energy K. Systems,
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ABSTRACT
Excess power was produced using a platinum cathode. Efforts to produce active cathodes by plating palladium onto various metals were largely unsucessful.

INTRODUCTION
Palladium has been the cathode of choice since Pons and Fleischmann made their original claims. Occasionally, anomalous energy has been claimed to result from other elements such as Pt[1; 2], Au[3], Ti[4]. Thin layers of palladium on various inert substrates have also been claimed to produce anomalous energy [5; 6; 7; 8]. From this collection of experience, one might conclude that any layer of Pd made to stick tightly to the surface of another material would produce energy with greater ease than the bulk metal. This assumption has been found to be false even though such material can achieve a D/Pd ratio greater than 1.5[9]. Layers of electroplated Pd can be just as difficult to reproduce as bulk material. Although several successful samples were made, this paper will describe only one example of uncoated platinum which produced excess energy after being electrolyzed for an extended time in $LiOD+D_2O$.

EXPERIMENTAL

Calorimeter Design:

The calorimeter, shown in Fig. 1, consists of a Pyrex glass cell surrounded by a watercooled jacket. This assembly is contained in a vacuum dewar, thereby allowing most of the energy lost through the lid to be picked up by the cooling water. A magnetic stirrer is used to stir the electrolyte, thereby reducing temperature gradients.

(1*) AT WHAT RATE WAS THE LIQUID HEATED BY THE STEERER?
(2*) WHY SHOULD I ASSUME THAT IT WAS A NEGLIGIBLE RATE IN COMPARISON WITH THE EXCESS POWER (EP~ 0.1 W)?

Since the stirrer was used at the same rate for active and calibration runs, it shouldn't matter. I imaging the heating effect of the stirrer is difficult to measure, since most of the power generated by the driving electromagnet is wasted elsewhere.

The entire assembly along with all reference resistors is contained in a constant temperature environment. Table I lists

values and uncertainties for the various quantities. The electrolytic cell contains three linear thermistors within the electrolyte, one near the top of the solution, one near the bottom, and the third just above the cathode. The anode is equidistant (0.5 cm) from the flat plate cathode (1 cm x 2 cm). Temperature of distilled water flowing through the jacket is measured just as it enters the jacket and just as it leaves. Data are recorded every 15 min. using a National Instruments data acquisition system after averaging 15000 values. The flow rate is measured by allowing the water after it leaves the calorimeter to fill a container on a balance while the weight and time are recorded every 120 sec. In addition, the cell contains a Pt-coated-carbon recombiner catalyst

(3*) TO "RECOMBINE" MEANS TO MAKE H_2O FROM H_2 AND O_2 THAT ARE PRODUCED IN THE CELL. (AN EXOTERMIC PROCESS).

Absolutely. Since an electrolytic cell generates hydrogen and oxygen gas while operating, there can be alot of argument about how much heat was lost (the chemical energy, possibly the latent heat of vaporization, etc). That makes a Pin vs Pout comparison very suspect. By recombining any gases produced, the energy stays in the calorimeter. It makes the equipment more complex, but the understanding simpler.

and an exposed Pt wire heater for calibration. Luggin capillaries

(4*) WHAT ARE LUGGIN CAPILARIES?

allow the voltage between a platinum reference electrode and the cathode to be measured.

(5*) WHAT IS THE THIRD (REFERENCE) ELECTRODE FOR?
(6*) WHERE IS THE ELECTRIC DIAGRAM?
(7*) WHICH VOLTAGE WAS USED TO CALCULATE THE INPUT POWER?

I'm confused by the reference electrode too. I assume the input power calculation was done correctly, but an electric diagram would be useful.

Because the cell is connected to an oil reservoir, any gas generated within the cell can be detected by weighing the oil displaced onto a balance.

(8*) WHICH GASSES WERE GATHERED IN THE CELL AND AT WHAT RATES?
(9*) WAS ANY CHEMICAL ENERGY GENERATED IN PRODUCING THESE GASSES?

The purpose of the oil is to check for failure of the recombiner. My understanding is that recombiners are very sensitive to surface contamination - any oil displaced onto the balance makes that run invalid because of recombiner malfunction.

Samples can be quickly changed or replaced by an inert cathode for calibration.

(10*) SAMPLES OF WHAT?
(11*) WHY ARE SUCH CHANGES OR REPLACEMENTS NECESSARY?
(12*)WHAT ELSE CHANGES WHEN A SAMPLE IS REPLACED?

"samples" are platinum cathodes. Because this experiment is the comparison of an active run against a control run

(rather than relying on absolute accuracies), it is critical to change the equipment as little as possible between runs. Storms has specifically designed it for changing cathodes with minimum disturbance of the setup.

Lengthy studies of inert platinum show a stability of ±75 mW.

(13*) WHEN IS PLATINUM NOT INERT?

The $24000 question in cold fusion. I would have loved to see Storms use this equipment as a tool to study just that question. Probably, he has, but hasn't come up with a definitive answer. My biggest complaint with this experiment is the circular argument: "this sample is inert" because it had a low Pout reading, and "this sample is active" because it had a high Pout reading. The excess heating claim is made by selecting runs based on the results. Ideally, you should specify your run as calibration or active beforehand, then compare the results to prove excess heat.

Calibration and Error

A typical calibration for the flow-mode is shown in Fig. 2, using a clean piece of platinum for the cathode.

(14*) WHAT IS FLOW MODE? WHAT ARE OTHER MODES?
(15*) TO CALIBRATE USUALLY MEANS TO ESTABLISH A RELATION BETWEE TWO OR MORE VARIABLES. WHAT VALUES ARE INVOLVED IN THIS CALIBRATION?

There are two possible measurement modes that can be made with Storms' calorimeter: flow mode and isoperibolic. Storms compares them in this review.

Isoperibolic mode means this: the calorimeter cell is surrounded by a constant-temperature jacket. The thermal conductivity "U" from the heat-generating device to the jacket should not change. You graph the temperature of the inside of the cell vs Pin. Pout is U(Tinside - Tjacket). (See eqution 5.4 in this paper.) The benefit of isoperibolic mode is that it is simple (just a temperature probe) and the cell is almost at a constant temperature. The drawback is in electrolytic cells: as you increase the power, more hydrogen and oxygen bubbles are produced. This drastically reduces the thermal conductivity.

The flow mode is: measure the flow of water, and the temperature of in-flowing water and out-flowing water. Any dependence on constant thermal conductivity is removed. However, you are now very dependent on the heat capacity of the water not changing.

"Calibration" here really means control run. You plot Pin (electrical measurement) vs Pout (heat flow out of the water) on a graph for your inert platinum cathode. You haven't really "calibrated" anything in the normal sense of the word - as in making sure a measured value is traceable to some NIST standard. You've just characterized the behaviour of the calorimeter with one cathode ("inert") for later comparison with another cathode.

Values are taken both going up and going down in applied power in the same manner as the sweeps described later. The standard deviation of the electrolytic values from the least-squares line is ±30 mW

(16*) WHAT IS THE “ELECTROLYTIC VALUE” (EXPRESSED IN WATTS)?
(17*) “THE LEAST SQUARES LINE” OF WHAT VERSUS WHAT?

The graph of Pin (electrical) vs Pout(heat flow in water) should be a straight line. Also, for a perfect calorimeter, (100% heat collection efficiency) the slope should be 1W/W.

Storms generated heat inside the calorimeter in two ways for his "calibration" runs: with a simple resistor, and with the inert cathode. The simple resistor obviously doesn't generate hydrogen and oxygen gas (I think he calls these the joule-heating values). The graph for the inert cathode does generate gases: this data is the electrolytic values and is the data he uses for comparison.

If the graph of Pin vs Pout was not a straight line, Storms would have to explain why is calorimeter changed in behaviour from one power level to another. His statement of ±30 mW least squares fit simply says that the calorimeter did not change performance from an ideal one (except for the fact that the slope was 0.98W/W rather than 1).

Note thate the Pin vs Pout graph was done on values going up in power, and going down in power. This is because one could say that chemical energy is being stored in the electrolytic cell at some point, and what looks like excess heat is actually just the release of the stored energy. The Pin vs Pout graph lies on a nice straight line going up in power and down: energy storage would cause hysteresis (the up and down lines would not overlap).

which is the same as the standard deviation from a constant value when stable excess energy is being observed at low applied power.

(18*) HOW WAS “STABLE EXCESS HEAT” DEFINED OPERATIONALLY?

Good question. There is really nothing "stable" or "repeatable" about cold fusion cathodes in general. Specifically, here he probably means that at constant voltage and current settings, the Pout stays constant.

Figure 1. Drawing of the calorimeter. The electrolyte is 65 ml of 0.3 N LiOD and the anode is Pt mesh. The cell lid is Lucite and the Dewar lid is expanded foam insulation. All thermistors are glass covered. Time to reach a steady temperature is 50 min.

TABLE I
Summary of uncertainties in measured quantities
Water temperature entering the jacket = 20±0.02°
Environment temperature = 20±0.03°
Flow rate = 31.00±0.05 g/min (long term variation)
Precision of current measurement = ±<0.001 A
Precision of voltage measurement = ±<0.001 V
Precision of temperature measurement = ±<0.005°
Absolute accuracy of temperature measurement = 0.1°
Stirring rate = 300 rpm ± 1 rpm
Average heat capture efficiency = 98±0.5%

(19*) WHICH VOLTAGES WERE MEASURED AND HOW WERE THEY USED?

From what I can figure out in the raw data, the voltage used for Pin is the voltage from the cathode to the anode. I'm not sure yet how the "reference voltage" fits in here. The measurement of Pin should be so straightforward that I'm not inclined to think Strorms' screwed up here. It is a bit of faith on my part, because I don't want to delve deeper.

This scatter increases to ±0.1 W at the upper limit of applied power (27 W). A zero drift as much as -0.05 W has been observed over an extended time. Consequently, changes in excess energy production are more accurate than absolute values. Good agreement between the electrolytic- and Joule-based calibrations shows that the location of heat production does not affect the accuracy of the device. Doubling the fluid flow from 22.3 g/min to 45.3 g/min caused a change in the calibration constant from 0.0732 W/degree-g/min to 0.0738 W/degree-g/min, indicating that good thermal mixing is achieved in the exiting cooling water.

(20*) WHAT WATTAGE IS USED IN THE UNIT OF CALIBRATION CONSTANT?

Wattage: the Pin (electric power) and the Pout (water flow rate and temperature in and out)

(21*) HOW WAS THE CALIBRATION CONSTANT DETERMINED?

OK. Here is the crux of the whole Storms/Shanahan discussion: unfortunately, it can't be explained without going into some detail.

The water flowed at 31 grams/minute. The heat capacity of water is 4.18188 J/g/degC at 20C. 1 Watt is 1 Joule/second. So the theoretical "calibration" constant for a flow-mode calorimeter with 100% heat collection efficiency would be 4.18188 J/gC * 31g/min * 1min/60sec = 0.069698 W/degree-g/min * 31 g/min. This is the same value Shanahan calculates for m*Cp in Table 1. **It is the value for a calorimeter with 100% heat collection efficiency**.

Storms has is an abstruse~~obtuse~~ way of presenting the calibration data, but I guess he does it because he multiplies the flow rate x (Tout - Tin) later and he wants to get Watts out.

In any case, Storms never calculates a theoretical 100% efficiency value. He gets his value of m*Cp experimentally from his graph of Pout vs Pin. Storms' value of 0.0732 W/degree-g/min to 0.0738 W/degree-g/min above corresponds to 95% to 94% heat collection efficiency.

From his two "calibration" runs with the inert cathode, Storms gets values of 0.071221 and .070892 (See Table 1 of Shanahan or, worse, go to Storms' raw data [html][txt][zip] courtesy of that 8th wonder of the world, the Wayback Machine!) which correspond to 97.8 and 98.3% efficiency. Storms concludes he has excess heat because some active cathode runs produce values like .0680 (run 4), .685 (run1), etc, (Table 1) which clearly shows that Pout has increased by a few percent, as compared to the inert cathode. Shanahan argues that .0685 is not that different than his calculated value of .069698, and concludes no excess heat. Shanahan has made the comparison against the wrong "calibration".

Because samples can be easily changed, the cathode is frequently replaced by clean platinum when the need arises to recalibrate. Good stability is shown by a scatter of only ±1.6% in the calibration constant when measured many times over three months.

Figure 2. Comparison between electrolytic and heater calibrations before and after the study using the flow method. The heater and electrolysis agree within 1.2%.

(22*) I SUPPOSE THAT DJ (SEE FIGURE 1) IS THE TEMPERATURE DIFFERENCE BETWEEN WATER-OUT AND WATER-IN. BUT WHY IS IT DJ AND NOT DT?

Confused me too, for a while. I guess delta T is for the temperature difference between the 3 thermal probes inside the electrolytic cell, and that left "delta Jacket". Confusing choice!.

(23*) IN FIGURE 2 T FOR THE TEMPERATURE “ACROSS THE JACKET.”
IS IT THE SAME AS DJ?

The cell can also be used as a rough isoperibolic calorimeter by measuring the average temperature between the electrolyte and the cooling jacket.

(24*) OK, THIS BE THE SECOND MODE OF OPERATION. (FIRST WAS THE FLOW MODE).
(25*) FORTUNATELY, I KNOW WHAT “ISOPERIBOLIC” IS. BUT MANY PHYSICISTS

ARE LIKELY TO BE CONFUSED BY THIS ADJECTIVE.

However, this method is not stable, in spite of active stirring, because of changes in the convection currents in the electrolyte and in the jacket. Figure 3 shows how the average temperature across the jacket changed between the first and final calibration using the electrolytic method and clean Pt.

Figure 3. Comparison between applied electrolytic watts and the average temperature across the jacket for two calibration runs.

Excess Energy Measurement

Figures 4, 5, and 6 show the time history for excess power (EP) production using a special platinum cathode.

(26*) WHAT MAKES THIS ELECTRODE “SPECIAL”?

Current sweeps consist of stepping the current up in value, waiting for the calorimeter to achieve steady-state (50 min), taking five values, and repeating the process.

(27*) HOW WERE THE VALUES OF EXCESS POWER (EP) CALCULATED?
(28*) THE ERROR BARS ARE GIVEN IN TABLE 1. HOW SHOULD I USE THEM TO ESTIMATE ERROR BARS FOR THE VALUES OF EP?

After reaching 3 A, the current is reduced in steps. Notice in Fig. 4 that the expected EP was achieved at 0.5 A and 1.0 A, but decayed away when 1.5 A was applied.

(29*) WHAT IS THE “EXPECTED” EXCESS POWER?
(30*) IF IT MEANS “EXPECTED ON THE BASIS OF ANOTHER REPORT,” THEN WHAT REPORT? IF IT MEANS “THEORETICALLY EXPECTED,” THEN BY WHAT THEORY?

I think "expected" is a poor choice of words and the cause of some concern that Storms is not unbiased. It sounds like he "expects" to see excess power, and then he sees it. I think the point of the statement is just that higher current (1.5A) somehow killed the excess power reaction.

A sweep taken after the decay showed very little EP. After the current was turned off for a brief time, the study was resumed in Fig. 5. Notice that the EP again gradually increased after 0.5A was applied. Sweep #3 again showed EP and this continued while 0.75A and 1.0A were applied. However, application of 1.5A again caused the EP to decay away.

Figure 4. Time history of excess power production from a Pt sample.

Once again the current was turned off. Application of 0.5 A, shown in Fig. 6, again showed EP. The calorimeter was calibrated at 430 h using the internal heater and later using an inert Pt cathode. This experience shows a consistent pattern of behavior which was repeated once again, but is not shown here. The first sweep is shown as applied current vs EP in Fig. 7. Notice that excess power is indicated by the isoperibolic method during this initial sweep, but later sweeps do not show an effect because of a change in calibration, as indicated in Fig. 3. Also notice that the excess power falls on a higher line upon reduction in applied current. Subsequent cycles, as shown in Fig. 8, produce excess power that fall on this higher line. The final relationship is linear and extrapolates to zero EP at zero applied current, in contrast to the behavior of palladium, which requires a critical applied current before EP is produced. Figure 9 shows the sweeps taken later using different numbers of values to produce the plotted average and these are compared to the heater calibration.

Figure 5. Time history of excess power production from a Pt sample.

DISCUSSION

A large average composition within the cathode is thought required to produce excess energy. Yet many researchers have failed to produce excess power after achieving large compositions, for example Nakata et al.[10]. Now, metals that do not even dissolve hydrogen are found to make excess energy. Clearly, additional variables are operating.

Figure 6. Time history of excess power production from a Pt sample.

Figure 7. Comparison between excess energy measured using flow method and isoperipolic method during first current sweep.

In the case of platinum, this study suggests that an energy-active layer of unknown composition can deposit on a Pt surface. This observation might also be related to the frequent detection of Pt on the Pd cathode after excess energy is observed. Such a layer is slow to form, which is consistent with the observed long delay in producing excess energy, and, for this sample, it is unstable. Is it possible that such a layer might be the active material in all studies, even when palladium is used?. McKubre et al. [11] also suggest that a critical layer is required to maintain the required high composition in Pd. Perhaps this is the actual active material. If this is the case, the bulk properties of palladium are only important in that they must support a critical concentration within this layer. Since palladium can easily permit loss into the metal, such material might be the worst substrate to use. Platinum, gold, and other inert materials would appear to be better choices. Use of such materials would only require forming the active layer without a need to use special batches of the substrate. If this suggestion is true, significant changes in various theories will be required.

Figure 8. Applied current vs excess power for the various sweeps, compared to Pt calibrations taken before and after the study.

Figure 9. Applied current vs excess power for sweeps using different number of values to average are compared to the heater calibration.

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151) Experimental errors in excess heat

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On May 25, 2004 Richard Eskimos (who prefers to remain anonymous), referring to items #116 to #119 on this web site, asked me (via e-mail): "Has there been any further recent discussion about Shanahan and Storms' papers? I'm still skeptical about cold fusion, but I have problems with Shanahan's criticism. I'd like to know if I'm the only one to give Shanahan's paper a critical look, as opposed to all the negative attention I'm sure any pro-cold fusion paper gets." In subsequent correspondence Richard elaborated on the above. After reading Richard's messages I asked him to summarize them in the form of a new item.

What follows is the text he composed for the readers of this web site. I am happy that the form chosen for the item is the same I used in unit #117. In fact, the first half of his essay is taken from unit #117. But then Richard focuses on aspects of the Storms-Shanahan debate that escaped my attention. Richard, who is an engineer in Canada, wrote to me that he is very interested in the cold fusion debate. He emphasized that the essay "isn't intended to address the validity of Storms' finding of excess heat, only the invalidity of Shanahan's criticism of the Storms' experiment. I'm still very skeptical on platinum."

In another message he wrote: "Mr. Shanahan's general discussion of systematic and random errors on your web site is certainly factual. He proposes that Mr. Storms has not analyzed the systematic errors adequately: he is probably right, and since no budget is infinite (especially Mr. Storms'), one can always make that claim about any experiment. Also, Mr. Shanahan has the advantage in a discussion about excess energy, since the burden of proof lies with Mr. Storm. However, in his paper, 'A Possible Calorimetric Error in Heavy Water Electrolysis on Platinum', Mr. Shanahan states that Mr. Storms has made an error in the experiment. So Mr. Shanahan now has the burden of proof to show exactly what the error is - a general claim of a systematic error because of unaccounted variables is not sufficient."

As in unit #117, electrochemical nuances are skipped by discussing a setup with two cylinders (magic and not-magic) instead of discussing real electrochemical cells. The nature of what is going inside these cylinders is ignored. Is such an approach appropriate to discuss the Storms and Shanahan controversy? I think it is appropriate because the debate, as far as I understand it, does not focus on electrochemistry. I am glad that Richard found time to elaborate on comments made in e-mail messages. In one message he wrote; "If I were close to retiring, I would definitely be getting more involved."

Please note that Pin (power input) refers to the rate at which electric energy is used to run a device while Pout (power output) refers to the rate at which heat is generated in that device. Without any additional input of energy the value of Pout can not possibly exceed the Pin. In a common light bulb, for example, Pin can be 60 W while Pout is close to 57 W. The positive difference between Pout and Pin, presumably detected by many scientists, is called excess heat. Dr. Edmund Storms is one of these scientists. Dr. Kirk Shanahan, also a scientist, thinks that claims of discoveries of excess heat result from calibration errors, as described in units #116 to #119. Is he right or is he wrong? The dialog between people below focuses on that question. My comments appear at the end.

= = = = = = = = = = = = = = =

On Storms-Shanahan Controversy

Richard Eskimos

1) Student:
My friend, an electrical engineer, discovered an unusual resistor. It behaves normally when the current is low (below 100 mA); the electric power, Pin, and the heat generation rate, Pout, are practically identical, as they should be. But at a larger current the Pout exceeds the Pin. The engineer thinks that he has discovered a new phenomenon; some unknown exothermic process is triggered by the current in the material from which the resistor is made. Is this possible?

2) Teacher:
We do not know everything about nature; new discoveries are likely to be made anywhere and at any time. Your friend may indeed be making an important discovery but I am not willing to accept his claim on faith.

3) Student:
My friend has a calorimeter and other instruments. He said he is willing to make a demonstration. Would you be interested in seeing it?

4) Teacher:
Yes, I would.

5) Engineer (one week later):
Thanks for allowing me to use the laboratory. Everything is ready. Should we start collecting data?

6) Teacher:
I would prefer if you first tell us exactly what you want to do. Where is your "magic resistor"

7) Engineer (reaching in his pocket):
Actually, I have two cylindrical resistors, a magic and a non-magic. They are mechanically identical. The only difference between them is the composition of powders placed in small cavities near the centers of cylinders.

8) Teacher:
What is the nature of these powders? Is it possible that a chemical reaction producing heat takes place in one cylinder and not in another?

9) Engineer:
For the time being I can only say that powders are not consumed. In other words they are not chemical fuels. A qualified chemist verified this by examining powders before and after many experiments.

10) Teacher:
OK, we might ask other chemists to verify this. For the time being we are interested in the small amount of excess heat observed by you. Is it real or not? Small quantities of anything are difficult to measure because of unavoidable experimental errors. Tell us more about your experimental setup.

11) Engineer:
Two insulated wires are soldered to the terminals of my resistors. I will connect each resistor to a d.c. power supply and show that V/I is essentially constant (Ohm's law), at least up to I=0.3 A. The R will be close to 160 ohms. The input power, Pin, will be calculated as V*I, where V is the measured voltage across the resistor.

12) Teacher:
How accurate are your voltmeter and ammeter?

13) Engineer:
According to manufacturer's specifications the accuracy of each instrument is 0.5%. This means that the accuracy of Pin will be 1%.

14) Teacher:
And what kind of instruments will be used to measure the heating rate, Pout?

15) Engineer:
For this I have built a flow calorimeter. It is a well isolated container whose capacity is close to one liter. Water enters through the pipe at the bottom (passing through a commercial flowmeter) and exits through the pipe near the top. I will set the flow rate to about 50 cubic centimeters per minute. To measure Pout I will immerse a resistor into the calorimeter. The temperature of entering water, T_1, and the temperature of exiting water, T_2, will be measured.

16) Teacher:
How accurate are the thermometers and the flowmeter?

17) Engineer:
Temperatures can be measured with an accuracy of plus or minus 0.1^{o}C. The factory-specified accuracy of the flowmeter is 1%. The container is thermally insulated as well as possible; this means that almost all of the heat (98%) is removed from it by water.

18) Teacher:
Your goal is to determine Pout. How will this be accomplished?

19) Engineer:
I will calculate Pout from the following equation: $Pout=S*F*(T_2-T_1)$, where S is the known specific heat of water (4186 J/kg*C), F is the flow rate (in kg/s) and (T_2-T_1) is the measured difference of temperatures.

20) Teacher:
That makes sense. Will the (T_2-T_1) remain constant?

21) Engineer:
At the beginning T_2 will be the same as T_1. Then T_2 will start increasing slowly while T_1 will remain constant (room temperature). After about two hours T_2 will stop changing and (T_2-T_1) will remain constant. This will indicate that in each minute heat generated and heat removed (by circulating water) are equal.

22) Teacher:
How accurate will be your determination of Pout?

23) Engineer:
The difference of temperatures will be about 10 ^{o}C. Thus the accuracy of the (T_2-T_1) term will be slightly less than 1%. To be on the safe side I will assume it is 1%. The accuracy at which F will be measured, as I said before, will be 1%. In other words, Pout will be known at the accuracy of 2%.

24) Teacher (addressing students):
Let me make a comment. Our guest is using the word "accuracy;" rather than the word "precision." This is correct. The word "precision" should be used when we are referring to random errors while the word "accuracy" should be used when we are referring to systematic errors. Suppose Pout turns out to be 40 W. Then, knowing that the accuracy is 2%, we would be able to say that the "true value" of Pout can be anywhere between 39.2 and 40.8 watts. Unlike random errors, which may also be present, systematic errors can not be reduced by performing the same experiment many times to obtain the average value. Let me make sure I understand you correctly. You will show that Pout is larger than Pin by

the amount that is significantly larger than experimental errors. Is this correct?

25) Engineer:
Not exactly. I would do this if the difference between Pout and Pin were large, for example, larger than 10% of Pin. My approach is different. I will keep the Pin constant and compare the values of Pout for my two resistors. I will do the experiment a number of times with the non-magic resistor, and a number of times with the magic resistor. Then I will compare the results. This means that I am less concerned about the accuracy of my equipment. I am much more concerned about the stability. If my equipment has a systematic error, I want it to have the same systematic error for both resistors. My sensitivity to excess heat now depends on the stability and precision of my equipment (and on how small a change I introduce in switching resistors).

26) Teacher:
I think we should continue discussing your data, with that in mind. How large were the actual values of Pout and Pin for the two resistors?

27) Engineer:
I've kept Pin at 36.4 W for both resistors. Pout for the non-magic resistor turned out to be 35.76W. Pout for the magic resistor turned out to be 36.76W. To me it means that the difference between these two numbers, 1 W, represents the "excess power." It is the rate at which thermal energy is generated inside the calorimeter. I think that this happens through a new nuclear process. What else can it be?

28) Teacher:
I can think of many non-nuclear processes able to generate heat at the rate of 1W. But that is not the issue here. Our goal is to establish reality of excess heat (~ 1 W) and not its origin.

29) Another student, Kirk Shanahan, makes a comment:
I notice that for the experiment with the magic resistor, Pin is 36.4W and Pout is 36.76W. The difference is much less than 1W. In fact, it's 1/3W, less than 1%. I think you have no excess heat, just experimental error.

30) Teacher:
Mr. Shanahan raises a very good point, but before we get to it, I want to know a bit more about the experimental errors. Firstly, about accuracy. Is it possible that the difference of 1 W is nothing but an experimental error due to the limited accuracy of measurements? We know that the true value of Pout can be anywhere between 35.96 and 37.56 W. That is based on your 2% accuracy. Likewise, the true value of Pin can be anywhere between 36.0 and 36.8 W; this is based on your 1% accuracy. Thus, the difference between Pout and Pin, can be as small as -0.84 W or as large as 1.56 W. Do you understand my reservations?

31) Engineer:
Yes. I agree that my equipment could be giving inaccurate values of Pin and Pout. I expected it. That is why I compared a non-magic and magic resistor. If my equipment is reading Pin at 2% too low, I don't mind. I just want to be sure that the reading is 2% too low for both the non-magic and the magic resistor. My first concern was that it might read 2% too low in the morning for the non-magic resistor, and accurate in the afternoon for the magic resistor. So I repeated the run with the non-magic resistor many times, at different times of the day. I also removed and reinstalled the resistor and compared readings. I found that the Pout readings varied by 0.3% over all of these runs. Likewise, the Pin readings varied by no more than 0.1%.

32) Teacher:
From these repeats, you don't know if the readings are all about 2% too low. And as long as that inaccuracy is the same for both resistors, you don't care. Is this your main point?

33) Engineer:
Correct, as long as it is the same for both resistors, I don't need to know how far off I am.

34) Teacher:

So, after these runs, you found that your Pout readings were repeatable to 0.3%. Let's go into the details of the runs. In runs with the non-magic resistor, Pout was 35.7W: on average; an individual reading could have been as low as 35.5 and as high as 35.9W. Similarly, Pin readings could have been anywhere from 36.2 to 36.6W. So, the difference Pout - Pin was from -0.3 to -1.1W. Shouldn't it have averaged out to zero? This sounds like the non-magic resistor is not behaving perfectly.

36) Engineer:
Correct - the equipment is not perfect. I believe the non-magic resistor has Pout = Pin, but the calorimeter is imperfect. The Pout I measure is the power collected by the water from the resistor. Not all the power out of the resistor goes into the water. Some of the power is conducted up the resistor's metal leads into the room. Since it doesn't go into heating the water, I don't measure it. This is a parameter of all calorimeters called the heat collection efficiency. I measure my heat collection efficiency to be 98%. (Pout = 35.67W is 98% of Pin = 36.4W). I have to work within that practical limitation and remove any error it causes if possible.

37) Teacher:
OK, let's look at the magic resistor results. Pout was 36.76W and Pin was 36.4W. With 0.5% error, the range for Pout is then 36.58 to 36.94W. The range for Pin is still from 36.2 to 36.6W. This reminds me of Mr. Shanahan's comment. In this experiment, it could have been possible that Pout and Pin are both 36.6W. Then you would have Pout = Pin and no proof of excess heat.

38) Engineer:
True, based on the assumption that my calorimeter just achieved a heat collection efficiency of 100%. There are two problems with that assumption. One, a 100% efficient calorimeter is difficult, if not impossible, to build. Two, in my non-magic resistor experiments, I measured that my calorimeter is 98% efficient. In order for that assumption to be plausible, you would have to show that changing resistors changed my efficiency. This is not likely to occur; my resistors are essentially identical, as far as the geometry is concerned. I've made the two resistors as mechanically identical as possible and their location in the calorimeter does not change. The electric leads are of the same material and the same gauge. The heat escaping out should be the same. I've removed and replaced the non-magic resistor several times, and the heat collection efficiency did not change.

39) Student Shanahan:
I have another comment. Based on the heat capacity of water and the flow rate, I've calculated a theoretical value for the ratio Pout/Pin. I'm looking at your data for the various runs. I've calculated Pout/Pin for each run. I see that the ratios for the magic resistor runs are clustered near my theoretical value. This leads me to conclude you have no excess heat. I also see that the ratios for the non-magic resistor are clustered at a value 2% lower than my theoretical value. This leads me to conclude that the non-magic resistor experiments have a systematic flaw - I don't know what it is. Possibly all calorimeter experiments have this flaw.

42) Engineer:
Your theoretical value for the ratio Pout/Pin is calculated for equipment that is perfectly accurate and a calorimeter that has 100% heat collection efficiency. A conclusion based on such unrealistic assumption should not be taken more seriously than conclusions based on my experimental data. I did not idealized anything. Is it not obvious?

43) Teacher:
Yes. The theoretical calculation of Pout/Pin does not have a systematic experimental error. You can't compare the experimental ratio to the theoretical ratio on the 0.5% scale, only as a rough estimate of how large your systematic errors are (which we think is 2%). On the other hand, comparing the non-magic and the magic resistor experimental values to each other allows us to ignore systematic errors which are stable. Thus the 1W difference between the values of Pout seems to be real.

44) Engineer:
I'd like to add that, if my equipment is accurate, the fact that the non-magic resistor measurements are clustered at 2% lower is simply a repetition of my previous statement that the heat collection efficiency is 98%.

45) Teacher:
Since I get the last word, I'd like to say that an experiment which compares the test of the hypothesis against an almost-identical control (or calibration) test has a sensitivity higher than the accuracy limits. Shanahan's suggestion that the magic resistor results should only be compared to a theoretical value rather than to the non-magic resistor isn't correct. The key to making the experiment convincing is to make the difference between the two experiments as small as possible. In this case, the engineer has designed the resistor to be swapped out very easily, which is an advantage. As always, the findings must be confirmed by others. Would you be willing to reveal the compositions of your powders to other scientists?

46) Engineer:
I would, but first I must protect myself with a patent. I do have a family to support; the discovery may lead to practical applications.

47) Teacher:
I understand. This is an important social issue but I do not think that we should debate it here. For the time being I would say that your claim for the discovery of excess heat is credible. Process your patent as quickly as possible and reveal the composition of your "magic powder," preferably in a reputable scientific journal. This might become an important scientific contribution without leading to immediate practical applications.

P.S. Comments (by Ludwik Kowalski):

1) It is a pity that Richard Eskimos asked me not to show his real name. Will the pending DOE evaluation of cold fusion end the unhealthy situation in which discussing cold fusion is dangerous to young scientists? I hope it will. Declaring that cold fusion is not different from any other area of science, and that additional research in that area is welcome, is the first thing that the new DOE panel should do. In saying this I am assuming that no evidence of fraud, or incompetence (among the major players), will be discovered.

2) Let me compare the proposal presented by the inventor (Engineer) in Unit #117 with the proposal presented by him in Richard's essay. The goal, in both cases, is to measure x=A-B, where x is the excess power while A and B are two numbers much larger than x. In the unit #117 the value of A (output power) was determined calorimetrically while the value of B (input power) was determined electrically. This approach failed to demonstrate that x is positive because of large uncertainties (systematic errors) in the values of A and B. The approach would not fail if x was several times larger, or if more accurate instruments were used to measure A and B.

In Richard's story the inventor decided to take a different approach. The value of x is determined not by comparing A and B but by comparing two different values of A. One value, A', is determined by using the "magic" resistor while another, A", is determined by using several control resistors. All resistors are geometrically identical; each contains a tiny cavity at the center. In control resistors that cavity contains sand but in the active resistors it contains a sand-looking "magic powder." This implies that heat collection efficiencies (~98%) are practically the same in all experiments.

In this new context x=A'-A". A systematic error in measuring A' and A" can be tolerated, as long as it is practically the same for both A' and A". The issue, as stated in voice #25, is no longer the magnitude of the systematic error, the credibility of the discovery depends on the stability of errors. Here is how I would summarize the issue. If the calorimetric error (about 2%, as in unit #117) changes significantly, when the magic resistor is replaced by a control resistor, then Shanahan is right. If, on the other hand, such change is very unlikely. then Storms is right. Is this the crux of the controversy?

3) Let me end with an interesting observation that Richard made when he saw the draft of this document (without my comments at the end). That observation was mostly about unit #148 but will append it here. Richard wrote:

I think the file is ready for posting. Having said that, I do have some philosophical issues with this approach (this is just for discussion purposes - don't let it prevent you from posting the file). This teacher/engineer essay is self-contained, so it re-states the essences of Storms' and Shanahan's papers - that makes it too long. At the same time, there is only room

for the high points of each paper - so the essay leaves the field wide open for Shanahan's rebuttals. I don't know how to get around these two issues in a better way, if the essay is intended for a general audience.

In your previous topic, you lamented the fact that scientific papers are much more abstruse than they need to be. It's exactly for these two reasons that it happens: the authors want to cover all the possible future rebuttals, so as not to cause lengthy debates. That makes the paper long and detailed. To compensate for the extra length, they refer to previous papers and the jargon of their field in abbreviated form. That makes reading the paper laborious for anyone who is not familiar with the previous papers.

I think scientific paper writing needs to get into the computer age. A scientist wishing to be accessible could write the "main" paper as they do now, for other scientists. If she wrote it in html, she could incorporate hypetext links. Jargon could be linked to definitions/descriptions; citations to other papers could actually hyperlink to those papers (maybe even the relevant sentences in the paper); calculations (which are often "left for the student") and data files could be linked to, in all their gory detail.

(Comment: Storms' paper originally linked to his data files, and that is how Shanahan analyzed his paper so fully. I found the original data on the Wayback Machine. Without access to the data, Storms' paper is much harder to get a clear picture of.) I don't know why papers aren't written this way, except that most journals are in hardcopy form.

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152) Recent cold fusion claims: are they valid?

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Introduction

Cold fusion (CF), presumably discovered 15 years ago, is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutron sources, cosmic rays, alpha particles or stellar temperatures. In 1989, several months after the discovery was announced (through a press release at the University of Utah) a panel of scientists, appointed by the US Department of Energy (DOE), examined the evidence supporting the CF claims. That evidence was declared insufficient. But, as summarized in[1] "there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system."

CF became highly controversial and only several hundred researchers continued working on it, world-wide. Most scientists still think that cold fusion is pseudoscience. On that basis editors of many journals refuse to publish papers devoted to CF research. Only a small fraction of scientists is familiar with recent progress in the area. The purpose of this article is to objectively summarize recent findings[2] and to supply references with which I am familiar. The article was triggered by the reported initiative of DOE to review[3,4] cold fusion research. I will focus on four cold fusion claims which are, in my opinion, the most important. As a nuclear physicist, and a physics teacher, I examined many CF publications and attended one cold fusion conference[5].

Claim #1: unexpected emission of nuclear particles

In the early 1980's Steven Jones, working at the Los Alamos National Laboratory, explored muonic atoms of hydrogen and the unstable molecules they form[6]. Such molecules are about 200 times smaller than their stable electronic counterparts. According to a well verified theory of the so-called "tunneling effect," the proximity of hydrogen atoms in muonic molecules increases the probability of nuclear fusion by many orders of magnitude. This is associated with the release of energy, as in stellar interiors and hydrogen bombs. For nearly a decade the work on muonic atoms was supported by the US government as a possible path toward a new source of energy. The grant, however, was not renewed after it became clear that practical applications, if any, would not materialize in the immediate future.

These investigations led to the idea that cold fusion might be occurring at very high pressures inside planets, as described in[7,8]. The notion that guides Jones, of a large electron screening effect in the D-D fusion reaction for deuterons embedded in metals, has been independently confirmed by other experimental physicists[9,14,15]. Can screening be responsible for lowering of the D-D coulomb barrier in a metal? Recent observations of rare neutrons and charged particles, reported by Jones[10], give credence to such speculations. The rates of observed emission are usually very low but significantly higher than the background; in one experiment the rate of proton emission was 400 times higher than the background.

The particles, identified as 3 MeV protons, were emitted from spots inside thin titanium foils loaded with deuterium. To load hydrogen ions into the foils Jones placed them (for several hours) into a cylinder filled with deuterium gas at elevated temperature (500^{o} C) and pressure (40 psi). Emission of nuclear particles was subsequently recorded with scintillation and silicon detectors in the low-noise environment. An aluminum foil of 19 microns helped to identify charged particles as protons. Coincidences between protons and other charged particles (tentatively assumed to be ^{3}H) were observed with a set of two silicon detectors. It is worth adding that protons and alpha particles have also been

reported by A. Lipson[11], R. Oriani[12] and A. Karabut[13]. These researchers worked independently of each other; their methods of loading metals with ions, and their methods of particle detection, were very different from those used by Jones.

The process of emission of such particles remains to be interpreted. For the time being Jones favors the model according to which the 2.45 MeV neutrons and the 3.02 MeV protons are accompanied by 0.82 MeV ^{3}He and by 1.01 MeV ^{3}H, respectively, as in well known thermonuclear reactions. The probability of emission of these particles in a metallic environment, however, is much higher than in hot plasma. A recent paper, published by a team of German scientists[9], does show that the cross section of the D(d,p)t reaction, at very low energies (down to 5 keV), is about one order of magnitude larger in the deuterated Ta than in a gas target. Similar observations were made earlier in Japan[14,15]. Very recently, a team of researchers, from Russian Academy of Science[16], found a unique way of observing protons (presumably from the same reaction) down to the energy of 0.8 keV. The observed rate of emission, at the lowest energy, turned out to be nine orders of magnitude higher than predicted by an accepted theory. This seems to indicate that the theory which agrees with experimental data above the energy of 10 keV fails to account for what happens to the embedded ions in titanium at much lower energies. But arguments about a model (screening versus other possible explanations) should not be confused with arguments about the validity of experimental data (observing unexpected neutrons protons and tritons).

Claim #2: accumulation of 4He in excess heat experiments

The claim of excess heat was first made in a famous press conference, on March 23, 1989. That event, and its consequences, are described in several books about cold fusion[1,17,18,19,20]. Two scientists, M. Fleischmann and S. Pons, announced that they had been conducting research on highly unusual electrochemical cells for several years. These cells were said to be outputting more thermal energy than received in the form of electric energy. The authors wrote that chemical contributions to excess heat were found to be insignificant. On that basis they tentatively concluded that the origin of excess heat was nuclear. Rejecting this hypothesis the critics pointed out that rates of nuclear reactions accompanying excess heat, if the origin of that heat were nuclear, would be many orders of magnitude higher than what was observed. Fleischmann's research, prompted by theoretical considerations[21], was purely experimental. The arguments against it, however, were based on the theoretical model of tunneling effect. The critics assumed that nuclear reactions, presumably responsible for excess heat, should be the same as those in hot plasma. Difficulties reproducing the effect also contributed to initial skepticism.

Subsequent work in the area is described in two books[22,23] and in many papers, such as[24,25]. The authors of these references describe experiments in which excess heat was generated. But they do not always provide enough data to rule out the possibility that a large fraction of excess heat can be due to parasitic chemical reactions, or other non-nuclear processes. Furthermore, according to Shanahan[26], the excess heat claims are due to calibration errors. It is difficult to accept this accusation because similar results have been reported by a large number of highly qualified scientists in several countries. On the other hand, one should not ignore the possibility of experimental bias, mutual self-deception, or even fraud. The best evidence that the excess heat is nuclear would be to show the commensurate accumulation of byproducts of nuclear reactions, such as ^{4}He. This will be addressed in connection with claim #4 below.

The most recent contribution, in the area of excess heat, belongs to a group of Chinese scientists[27]. X. Li, a veteran of cold fusion research, did not use electrochemistry to load palladium with deuterium. The excess heat was generated when compressed gas was allowed to diffuse through a thin palladium wall. According to the authors, that heat could not be explained by the well known Joule-Thomson effect or by chemical reactions. They write: "this experiment has been repeated 6 times already in various configurations. The 'excess power' density in the Pd disk is more than 100 W per cubic centimeter, which is about the power density in a fuel rod of a thermal neutron fission reactor." Reproducible results on generation of excess heat, in a glow discharge chamber (another non-electrolytic method of loading metals with D^+ ions), were also reported by Russian scientists[13].

Generation of excess heat without producing radioactive material would certainly be desirable. But how can nuclear energy be released without commensurate amounts of radioactivity? According to some theoretical considerations[27],

deuterium ions embedded in crystals might be influenced by a large number of atoms able to supply and to remove energy "in unison". Theoretical modeling of natural phenomena, however, and attempts to validate these phenomena experimentally, are two different things. Arguments for or against models do not resolve disputes about validity of experimental data.

Claim #3: highly abnormal isotopic ratios

Chemical elements, not initially present, often accumulate in cold fusion setups, as reported in[13, 22, 29, 30]. Are new chemical elements due to nuclear reactions or are they impurities introduced during various manipulations? The most direct way of answering this question is to subject the residual chemicals to isotopic analysis. Such analysis shows that isotopic compositions of residuals are often very different from those expected from contamination. According to[13], for example, nickel found in a palladium cathode (after the cathode was used to generate excess heat) contained less than 1% of ^{58}Ni; the natural abundance of that isotope is nearly 68%. On the other hand, carbon found in the cathode contained 20% of ^{13}C, while the natural abundance of that isotope is about 1%. Similar results were reported by other researchers[22,30]. Such findings, if confirmed, would indicate that nuclear processes do take place in some CF setups.

Claim #4: nuclear transmutations

Absence of anticipated products (unjustifiably expected to be the same as in well known thermonuclear reactions) was often used as an argument against cold fusion. A thermonuclear fusion of two deuterons (^{2}H + ^{2}H), for example, nearly always results in production of either ^{3}He or ^{3}H (associated with the emission of neutrons and protons, respectively). Absence of commensurate amounts of neutrons and protons, in excess heat experiments, was often compared to fire without any ashes. That is why the claim of nuclear origin of excess heat was not taken seriously when it was first made in 1989. But several years later progressive accumulation of ^{4}He was reported by several investigators[31,32]. The authors found that helium generated via cold fusion is mainly ^{4}He; the ^{3}H and ^{3}He atoms are produced much less frequently. The situation is dramatically different from what happens in thermonuclear reactions taking place in ionized gasses. In these reactions the probability of the ^{2}D+^{2}D--> ^{4}He (releasing about 24 MeV of energy) is 10^{-6} while the probabilities of reactions producing ^{3}H and ^{3}He (releasing about 3 MeV of energy) are roughly 0.5 each.

Furthermore, the energy released in cold fusion (24 MeV per dominant cold fusion event) appears in the form of heat and not in the form of gamma rays (as in rare hot fusion events). How can such differences be explained? That is one of the many unanswered theoretical questions. At present, however, the main issue is experimental rather than theoretical. Is the accumulation of ^{4}He, at the rate of about one atom per 24 MeV of excess heat, real or apparent? Skeptics suspect that helium comes from the surrounding air and not from a totally unexpected nuclear reaction. The authors of the above mentioned reports, however, addressed this issue and ruled out the possibility of atmospheric contamination. If confirmed, such findings could become very significant. They would indicate that ^{4}He is the main "ash" of the mysterious CF "burning," at least in some cases. Those who objected to cold fusion claims in 1989 expected ^{3}H and ^{3}He to be dominant ashes; how would the discovery of cold fusion have been received if information about ^{4}He was available at that time?

As it turned out, ^{4}He, is not always a dominant "ash" accumulating in excess heat experiments. Production of elements heavier that helium, first reported and then withdrawn by Bockris, was later heralded by some investigators[13,22,33]. Results from a very extensive study are summarized in [34]. The most recent report in this disputed area was presented by Iwamura, from Advanced Technology Research Center, Mitsubishi Heavy Industries, Ltd., in Japan. Addressing the 10th international CF conference (August 2003) Iwamura described a fascinating setup[35] in which cesium was turned into praseodymium and strontium was turned into molybdenum. The paper describing these experiments[36] had already been published in the Japanese Journal of Applied Physics (JJAP). It is highly significant that the isotopic composition of Mo, produced from Sr, is drastically different from that found in nature. This seems to rule out the possibility of contamination (redistribution of impurities). Low energy transmutations in condensed matter, reported by Iwamura, have been repeated by scientists from Osaka University[37].

Final Comments

Cold fusion is not taken seriously by most scientists. But, according to my own informal survey, the opinion of many is still based on what was known in 1989 and not on recent findings. I think that the often repeated labels, such as "pseudoscience" and "fiasco of the century" were perhaps justifiable in 1989, when the first DOE review was conducted. But are such labels justifiable today? Most of us are not equipped to answer this question through laboratory investigations. That is why another official evaluation is desirable. Are the credentials of CF scientists doubtful or not? Are their ways of validation consistent with scientific methodologies? Is there any evidence of deliberate deception? Answers to such questions should help us decide what to think about the controversial field, and what to tell students when they ask questions about it.

Cold fusion claims have often been criticized for being in conflict with the existing theory of nuclear phenomena. Reacting to this one can say that in science a theory should guide while experiments should decide. That is a recognized principle of our scientific method in which theories are models of objective reality. Models often lead scientists to discoveries of new facts but a single confirmed fact contradicting a model is a good reason to modify a theory. On the other hand, an established theory, one that is not only logically consistent, but is also supported by a large number of experimental facts, has a special place in the arsenal of scientific tools. Leaning on such theory is like leaning on the experimental facts supporting it. A theory often protects us from drawing wrong conclusions from experimental data. The initial skepticism about cold fusion was mostly based on the accepted theory; it was not based on critical analysis of new data. The cold fusion controversy, no matter how it is resolved, offers us an insight into the delicate interplay between theoretical and experimental investigations.

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I have already written about rejections of cold fusion papers by editors (see item #88); this item describes my experience trying to publish a cold fusion review paper (item #152). What follows are dated entries and messages, as recorded during the Spring 2004 semester. I present them in the form of a diary; colors are used to identify e-mail messages. The editors of the following journals rejected the paper without sending it to referees and without offering any criticism:

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1) Physics Today, USA
2) American Scientist, USA
3) Nature, UK
4) New Scientist, UK
5) The Physics Teacher, USA
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THIS DIARY-LIKE ITEM IS UNREASONABLY LONG. READ THE ITEM #154 INSTEAD, UNLESS YOU ARE INTERESTED IN IRRELEVANT DETAILS.

153) Why was my review paper rejected?

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A SHORTER (BETTER) VERSION OF THIS IS IN ITEM 154

2/11/04
What to think about this e-mail message (in green, below)? It refers to what was described in my unit #100; the rejection of my Letter to the Editor of Physics I considered the rejection final. That is why I am so surprised. And what a coincidence; I just finished working on a paper objectively summarizing the situation in the cold fusion field and I am looking for a place to publish it. What would be a better place for it than Physics Today? Should I consider the message from Dr. Hanna to be an indication that this might be possible? Why did she write to me after the issue was essentially over about half a year ago? I do not know how to answer this question. Here is a message I received today:

Dear Dr. Kowalski,

In August of 2003, you submitted a Letter to the Editor commenting on our previous rejection of a letter you'd sent to us last January. That earlier letter commented on the story, "New APS Ethics Guidelines Address Research, Misconduct and Professional Responsibilities."

Please accept my sincere apologies that we have taken so long to get back to you. We did give both of your letters careful consideration.

After that consideration, it was decided that we would share with you the reviewers' comments on your first letter. Those comments are contained in the attached MS Word file. Please let me know if you have any problem opening or viewing the file.

Also please note that your second letter was not accepted for publication. While we do appreciate and carefully review letters about how we are doing our job, we consider them to be internal business.

If you have further concerns about this matter, you may contact me via e-mail at ptletter@aip.org or telephone at 301 209-3041. My fax number is 301 209-0842.

Thank you for writing and for your interest in Physics Today.

Sincerely, Marty Hanna, Letters Editor, Physics Today, College Park, Maryland.

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Here are the reviewer's comments from the attached file.

Reviewers' Comments on Ludwik Kowalski's Letter

Reviewer 1: I don't have a high opinion of this letter. The author is trying to use the ethics guidelines as a wedge to say that cold fusion scientists are doing real science and are being discriminated against by the science establishment. It is, at its base, a polite dance around the cold fusion conspiracy theory. He notes that the "new" work with cold fusion may not be fusion, but some other mystery that seems, despite all of the work, somehow beyond the understanding of the scientists. And even though it may not be fusion, or just a little bit fusion, the respected science journals discriminate against the work. I get the impression the author isn't a cold fusion scientist with an axe to grind, but it sounds like he's been to a few seminars with them. I recommend against publishing this as it does nothing but revive a conspiracy theory that is better off dead.

Reviewer 2: Reject. I'm actually a bit ambivalent — his concern as a teacher of students seems valid. If you want, get another opinion from our staff teacher.

Reviewer 3: I'm comfortable with the reject but, like Reviewer 2, see some merit in the Letter. I believe it raises a legitimate discussion point: what is the physics community's responsibility, if any, to sincere, competent workers who choose to explore "fringe" fields like cold fusion. Harold Birnbaum touched on this issue in his March 2002 article for us, when he noted his impression that nowadays, reviewers are less likely than previously to support a proposal that is not in accord with their own view. It could be argued that the question is an ethical one, so I find it reasonable that Kowalski's letter was inspired by Jim Dawson's ethical-guidelines report.

Kowalski's letter is not well focused, and as a result, Reviewers 1 and 2 and I each took something different from it. Kowalski's choice to limit his letter to cold fusion instead of addressing a larger picture was unfortunate, in my opinion. Reviewer 1 decided that the letter "does nothing but revive a conspiracy theory that is better off dead." I think the letter hints at more, but Reviewer 1's take is certainly reasonable, given what the letter actually says. In brief, if I were the first to look at this letter, I would consider working with the author (and it would take some work) to try to get the letter I think the author intended. However, as an "appeals judge," I see no reason to overturn the decision of the "lower court."

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Here is my immediate reply:
Dear Dr. Hanna:
I am very glad that you decided to share with me what reviewers wrote about the Letter to the Editor I wrote one year ago. Let me comment on what they wrote and then make a suggestion.

1) Yes, the first reviewer was correct that I was "trying to use the ethics guidelines." Referring to me he wrote: "it sounds like he's been to a few seminars with them ['cold fusion scientist with an axe to grind']." This was not true when my letter was written; at that time I was mostly interested in moral aspects of the cold fusion episode. But I did go to a cold fusion seminar last August and participated in the 10th International Cold Fusion Conference. I went there as an

open minded teacher and I learned a lot.

2) I am pleased that two of three reviewers did “see some merit in the letter.“ That shows that a conspiracy, if it exists, is not widespread. And your decision to share the comments with me is a confirmation of this.

3) It happened that I just finished writing a paper about basic recent claims made by leading cold fusion scientists. It is not a paper defending these claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers should be aware of what has been going on in the cold fusion field since it was officially evaluated, in 1989, by a panel of US experts. Some of the new claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of published alchemy reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable.

4) Anticipating that my article would be rejected in the US I considered publishing it abroad. But I would very much prefer to publish it in our journal, Physics Today. What I write is likely to generate interesting discussion and comments. A copy of that paper is attached. Please pass this message to the Editor-in-Chief and let me know if the journal is willing to send it to reviewers. Sincerely yours, Ludwik Kowalski

P.S. Last year I spent my entire sabbatical collecting cold fusion documents, and reading them. I did this for myself and for others who are not familiar with recent cold fusion claims. Many readers of Physics Today will probably appreciate my summary. What kind of harm can possibly result from publishing it?

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The manuscript that was attached can be seen in item #152 on this web site. The answer to my reply came two hours later, much faster than I expected. Here it is:

Dear Dr. Kowalski, Thank you so much for your feedback, and thank you for the article submission. Because the document you sent is beyond the limits of the Letters Department, I have forwarded it to our editorial staff for consideration as an article. Please send further inquiries about it to pt@aip.org, but expect that it could be several weeks before you hear from our editors. With gratitude and kind regards, Marty Hanna
Letters Editor, Physics Today, College Park, Maryland.

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What to think about all this? Why the apology, six month later? Will they give my paper a chance to be reviewed by unbiased people? I do not know how to answer these questions. I suspect that my registered letter to the Editor in Chief, mailed in September was finally read and acted upon. I can not locate the copy of what I wrote but I do remember complaining about not being able to see the comments made by reviewers. At the end of that letter I suggested that its content should be treated as a second Letter to the Editor. The Editor in Chief never replied to me. Perhaps he remembered my earlier complaints. What else could have triggered this new message from Dr. Hanna? If they publish my article I will be very happy, if they do not publish it then I will have another record of rejection. Such records are worth saving; I should be able to find an editor willing to accept my review. If not then the review will appear as an item on this web site. For the time being I will withhold this item; it is probably not right to make it available and try to publish it at the same time.

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2/14/04
During the weekend I discovered some imperfections in what has already been sent to Physics Today. Furthermore, Steven Jones, the only person who knows about my submission (because I asked him to comment on the submitted article, and to criticize it), sent me some comments. Because of this the article was revised and resubmitted today in the traditional way (via post office). Here is the letter I sent today with my printed article:

Department of Mathematical Sciences
Montclair State University
Upper Montclair, NJ, 07055

February 17, 2004

Dr. Stephan G. Benka, Editor-in-Chief
Physics Today, American Institute of Physics
Suite 1NO1, 2 Huntington Quadrangle
Melville, NY, 11747-4502

Dear Dr. Benka:

Last Friday, replying to an e-mail message from Dr. M. Hanna, I at once submitted the electronic version of a paper entitled “Recent cold fusion claims: are they valid?” This paper, I hope, can be published in Physics Today. As I wrote to Dr. Hanna, it is not a paper defending cold fusion claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers. Many physicists are probably not familiar with what has been going on in the area of cold fusion since it was formally evaluated in 1989.

Some of the new cold fusion claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. I hope that this suggestion will generate an interesting discussion and comments.

After sending the electronic version of my paper, I discovered a clerical mistake; three references appearing in the text were not listed at the end of the article. And not being sure that subscripts and superscripts would be printed properly by all printers I am sending the paper version of the article with additional minor corrections. Note that references have been renumbered to account for insertions. Please use the paper version of my article instead of the version e-mailed last week. Needless to say, I will be glad to react to suggestions made by your reviewers.

Sincerely yours, Ludwik Kowalski, Ph.D.
Enc.

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I still do not know what to think about this episode. What prompted Physics Today to contact me half a year after my letter to the editor was definitively rejected? Will the editor in-chief answer me? What is the chance that my article will be sent to unbiased referees? What is the chance that the reviewers will recommend the acceptance? I have no idea. The second physicist who knows about today's submission (beside Steven Jones) is my Ph.D. thesis adviser, Pierre Radvanyi. He did tell me, when we met in Paris last summer, that Cold Fusion is not worth spending time on. But I could not resist sending him the paper. Will he, whose second specialty is history of nuclear physics, say that I am wasting time? We will see. If the paper is published in Physics Today then one can be reasonably certain that a discussion about the second formal evaluation of cold fusion will develop, in the form of letters to the editor. And who knows, such discussion can lead to some changes of attitude about cold fusion topics. That would be the end of my present mission. After retiring next June I plan to become a cold fusion researcher. The goal will be to look for unexplained charged particles emitted from Steve's Ti foils using track detectors. (like Oriani, Lipson and Karabut).

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3/13/04

I finally received the answer from the editor of Physics Today. " Dear Dr. Kowalski: We received your article submission titled, "Recent Cold Fusion Claims: Are They Valid?," and appreciate your sending it to Physics Today. After reviewing it, however, we have concluded that it does not meet our editorial needs. Thank you for your interest in Physics Today. Sincerely, Stephen G. Benka Editor-in-Chief."

That is it. Not a single word about the content of the article. How can the phrase "does not meet our editorial needs" be interpreted? Why was the article not sent to reviewers? They do publish many field summaries each year. Why was my summary not given the same chance to be reviewed by experts? Was I writing about sociology, poetry, business or something else unconnected to physics? Are recent cold fusion claims described in the article already widely known to most physicists? Was my description of these claims erroneous? Was the article rejected because of its style, its limited scope, or its disregard for ethical standards?

Do I have right, as a member of AAPT, ask for an explanation of the rejection? Is the editor of Physics Today expected to briefly justify his decision to reject a paper? I do not know. Perhaps somebody will provide me with answers to such questions. For the time being I submitted the same paper to American Scientist, by e-mail. Will it be rejected in the same way or will they have the decency to send the article to unbiased reviewers? We will see.

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As indicated above, I might be able to perform cold fusion experiments myself. Suppose the results agree with a claim I am trying to verify? Will they, guardians of the gates, allow me to publish an article describing such results? In another field of science a claim of a discovery, made by a qualified investigator, would be published in a scientific journal. But in the field of cold fusion one should expect a rejection by a mainstream journal. That is what happened after Steven Jones submitted his highly convincing paper to Physical Review. Fortunately that paper is available over the Internet. I would not know about that paper if I hadn't attended the cold fusion conference last fall. Even if the "smoking gun" is found it is difficult to publish the result in a mainstream journal. That is sad; that is discouraging. That is why I would not advise a young researcher to work in the area of cold fusion. The field might die from absence of support and lack of natural continuity.

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DRAFT OF A REPLY (THAT WAS NOT SENT)
Dear Dr. Benka:

I received your message and, naturally, I accept your decision not to publish my paper. I would appreciate, however, some explanation. I spent an entire sabbatical year gathering the material and learning what is summarized in the rejected paper. Please let me know on what basis my paper was rejected. How does it differ from dozens of review papers that our journal publishes each year? Was it a matter of style? Was it a matter of not describing new findings correctly? Was it because I am primarily a physics teacher and not a research scientist? I am not going to argue with you. Please let me know why my manuscript was rejected.

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3/12/04
I am sending the rejected manuscript to another journal -- American Scientist.

3/21/04
Before describing the correspondence with American Scientist let me digress and write about some interesting events. Perhaps these events have something to do with the fact that reviewers' comments (on my Letter to the Editor) were shared with me after six months. Below are extracts from an e-mail message received today.

Eugene Mallove [who died tragically last spring] wrote**:**
1) The U.S. Department of Energy has made a startling reversal of its past refusal to evaluate with a fresh look the large body of experimental evidence that now supports highly anomalous non-chemical magnitude excess heat phenomena in some hydrogen systems, plus associated nuclear anomalies.

2) The confirmation of the DoE review came first in a draft article by Physics Today science journalist Toni Feder. This draft was circulated to several LENR scientists, critics, and others who gave input to Ms. Feder. New Energy Foundation provided input to Ms. Feder and welcomed receipt of the draft article from her. The article is to appear in Physics Today's April 2004 issue, which should be out by the first week of April.

3) The first popular journal to publish the news of the impending DoE review is, however, the UK-based New Scientist. In its March 20, 2004 issue, which was received in the mail today (3/20) at New Energy Foundation here in Concord, New Hampshire, freelance journalist Ben Daviss reports in a short article in the 'Upfront: News in perspective' section (p.6), that James Decker, deputy director of the DoE's Office of Science, 'has pledged to review evidence from the past 15 years of research in the controversial field.' Daviss also writes, 'The study could be completed by January 2005 and might open up the possibility of funding for cold fusion research
projects.' "

If Feder's article is ready to appear in Physics Today then why didn't the editor in chief (Dr. Benka) write to me that an article devoted to the same topic had already been accepted, when he rejected my article nine days ago? Instead he wrote "After reviewing it [your article], however, we have concluded that it does not meet our editorial needs." How can I argue with this? The editor in chief probably has the right to review an article internally instead of sending it to outside experts. An article which has nothing to do with physics could be handled in such way. But my article was devoted to physics only; I deliberately avoided social and historical issues. Somehow I suspect that what Mallowe wrote is not true. On the other hand, why would Eugene put his reputation at stake without being certain? Here's what I found about Toni Feder over the Internet: She is on the Editorial Staff of Physics Today. Was she among those who decided to reject my paper two weeks ago? She is also an Adjunct Assistant Professor at the Department of Physics, Duke University, Ph.D. 1993.

3/23/04
Message sent to an editor of American Scientist (3/23/2004)
Dear Dr. Schoolmaker: Did you receive my manuscript, entitled "Recent cold fusion claims: are they valid?" It was e-

mailed to you on March 12, 2004. I did not expect to be writing this message on the 15th anniversary of the famous cold fusion press release in Utah. That release occurred on March 23, 1989. Shouldn't this be a reason for publishing my review paper? I hope so. But there is another reason as well. According to the last issue of the UK-based New Scientist (March 20, 2004, page 6) the DOE decided to evaluate new research in the area of cold fusion. The text of that announcement is pasted below. This event, if officially confirmed, will naturally renew interest in cold fusion among many readers. Please note that my paper focuses only on scientific aspects of cold fusion. I deliberately avoided references to social aspects, which are interesting but highly controversial. I would be willing to add a paragraph or two devoted to social aspects of cold fusion, if you find this to be desirable. Sincerely yours, Ludwik Kowalski

After waiting more than a week for a confirmation that my submitted paper was received I e-mailed another message to the editor Of American Scientist: In that message I wrote:

. . . Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers. Many readers are probably not familiar with what has been going on in the area of cold fusion since it was formally evaluated in 1989. Some of the new cold fusion claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. I hope that this suggestion will generate an interesting discussion and comments. Sincerely yours, Ludwik Kowalski, Ph.D.

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3/26/04
After being ignored again I simply called American Scientist and obtained permission to contact the editor in chief Rosalind Reid. I sent her the paper; here is the reply:

Dear Dr. Kowalski:
Yes, we've received your original manuscript and the follow-up. I'm afraid we're not always able to acknowledge receipt immediately. I try to give a prospective author an idea of whether we'll be able to consider a manuscript, and sometimes it takes a little time to determine that. We have certain basic criteria for submissions. When a submission does not meet those criteria, I prefer to say that it cannot be considered rather than simply acknowledge receipt.

In the case of this submission, I'm unsure. We publish feature-length articles and commentaries based on original published research. The authors of American Scientist articles are the people who have done the work and therefore are in a position to survey their own field. I don't actually have evidence (in the form of cited publications or a c.v.) that you have done original research on the topic you propose to write about.

If you would like to publish a short commentary, we do have a department with different criteria, called "Macroscope." This is where we publish short essays conveying a scientist's point of view on a matter of personal or professional interest to scientists and engineers. The maximum word count is 1,500. If you would like us to consider publishing your piece in a short form, please let me know, and I'll share it with my colleagues and let you know the response. Sincerely, Rosalind Reid Editor, American Scientist

Responding to the above I wrote:

Dear Dr. Reid: Thank you for your prompt reply. I understand your hesitation. Protecting readers of American Scientist from people who are not qualified to write about science should be one of your tasks. To help you decide here is a little summary about myself.

I am an experimental nuclear physicist (Ph.D., 1963) with a large number of publications (mostly as coauthor) in that field. The attached abbreviated list of publications, spanning four decades, makes it clear that my teaching commitment has not prevented me from active participation in nuclear physics research. Like most scientists, I accepted the 1989 verdict about cold fusion. And you are correct, I have no publications about cold fusion. My new interest in this field was triggered in October 2002. I attended a nuclear conference in New Mexico and heard several scientists talking about cold fusion research. It was the beginning of my sabbatical year. I changed my anticipated literature research project and focused on cold fusion instead. The paper I submitted is the product of that work.

As you can see, my submitted paper focuses on experimental aspects of nuclear research. I think I am qualified to write about that. Several months ago I got involved in a project headed by Steven Jones. (As you might remember, he was one of the three scientists involved in the famous 1989 controversy.) Our work is in progress and we plan to publish results before the end of this year; most likely at the 12th International Conference on Cold Fusion in Marseilles, France. So I can now say that I am participating in cold fusion research. The goal is to confirm the emission of 3 MeV protons from the activated TiDx foil by using a detector which is very different from that used at BYU. I am sure that Steven Jones will be able to confirm our collaboration. You can contact him at <steven_jones@byu.edu>.

I hope your hesitation will not prevent you from sending my article to competent and unbiased reviewers. Please let me know what your decision will be. Meanwhile I would like to follow your suggestion about writing a short commentary on the anticipated review of cold fusion by the DOE; see the attached file. Thank you for your consideration. Sincerely yours, Ludwik Kowalski

Two files were attached one containing a list of selected publications and another containing the following short note to be considered. I hope that at least this note will be published. Here is its content:

SEEK NOT THE GOLDEN EGG, SEEK THE GOOSE

Ludwik Kowalski
Department of Mathematical Sciences
Montclair State University, Montclair, NJ, 07043

According to a recent article in The New York Times (3/25/2004) the US Department of Energy (DOE) is going to review the field of cold fusion this year. This is a significant event; the controversial field of cold fusion (CF) has often been called pseudoscience. If it were up to me I would suggest that the panel of DOE scientists focus on essential scientific questions and not on practical applications which are far away, at best. Promising too much, and too early, was one of the mistakes made fifteen years ago. In my opinion the six most important scientific questions are:

1) Are unexpected neutrons, protons, tritons and alpha particles emitted (at low rates) in some CF experiments?
2) Is generation of heat, in some CF experiments, linearly correlated with the accumulation of ^{4}He at the rate of 24 MeV per atom of ^{4}He?
3) Have highly unusual isotopic ratios been observed among the elements found in some CF systems?
4) Have radioactive isotopes been produced in some CF systems?
5) Has transmutation of elements occurred in some CF setups?

6) Are the ways of validating scientific findings in the areas of
CF research consistent with accepted methodologies in
other areas of science?

I think that a positive answer to even one of these six questions should be sufficient to justify an official declaration that "cold fusion, in light of recent data, should be treated as a legitimate area of research." The normal peer review mechanisms will then be used to separate valid claims from wishful thinking.

Several hours later I received this reply:
Dear Dr. Kowalski: We'll take a look at the short piece. Meanwhile, I should go just a little further in explaining the requirements for a feature article. Our articles are based entirely on published work; that is, we will be able to consider your feature-article manuscript *after* you've published substantial original research on the topic. Because we do not have our own peer-review process, we must rely on the community to referee original research. An American Scientist review always follows, rather than precedes, journal publication. I wish you the best of luck with your collaboration and hope you'll get back in touch after it has borne fruit. Please give us a couple of weeks to have a look at the short commentary. I'll share the whole package with my colleagues. Sincerely, Rosalind Reid Editor

My answer was:
Dear Dr Reid: Thank you for another prompt reply. I will try to publish the review article in another journal. I suggest that you contact Martin Fleischmann, the central cold fusion figure, or another acceptable author, and ask for a paper about cold fusion. The 15th anniversary, and the pending DOE evaluation of the CF field, justify such initiative, I think. And thank you for giving my short piece a chance of being accepted; the sooner the better. Have a good weekend. Ludwik Kowalski"

TO WHICH JOURNAL SHOULD I SEND MY LONGER PAPER? SHOULD IT BE SCIENTIFIC AMERICAN? WHY NOT? IT IS A DIFFERENT JOURNAL THAN IT USED TO BE IN THE 1960s. BUT I THINK THAT IT IS STILL A VERY APPROPRIATE TARGET FOR MY REVIEW.

3/27/04
ANOTHER MESSAGE TO DR. REID:
After reading our last messages again I see a good reason for sending you another one. In the last sentence you refer to the "whole package." Does it mean that there is a slight possibility that my long paper might still be accepted by American Scientist? Assuming (probably too optimistically) that this is the case I will wait another week before submitting my review paper to another journal. But please, decide sooner, if you can.

You wrote: "Because we do not have our own peer-review process, we must rely on the community to referee original research. An American Scientist review always follows, rather than precedes, journal publication." I respect that policy and think it is good. But what about review papers? It is not likely that you can find an author who actually worked in all fields of a broad area to be reviewed. The so-called "cold fusion" is an area of many different fields and, as far as I know, nobody worked in more than one of them. Each area is highly specialized in terms of expertise and instruments. The authors, as I found out, often disagree with each other and the only thing that unites them is rejection by "mainstream science." My paper, as you know, is a review of that controversial field.

I already mentioned two reasons making such review urgent: the 15th anniversary of the Utah announcement and the pending DOE investigation. In my opinion, by publishing my paper, or a review written by somebody else, you will contribute to something desirable. Nobody is happy with the unhealthy feud between a group of well motivated researchers and official representatives of "mainstream science." Most people are passive but those who do take extreme positions often use highly pejorative adjectives, such as "pathological", " stubborn, " misguided," and "fraudulent." Please do not miss an opportunity to contribute to ending this unnecessary feud. I would be happy to give you names and addresses of top people in five main areas of cold fusion.

Yesterday I wrote to you about my current cooperation with Steven Jones from BYU in Utah. But I forgot to mention another cold fusion project in which I participated during the 2002/2003 sabbatical year. I will not mention the name of

the man who accepted my suggestion to repeat his experiment together. The outcome was not positive. His very extravagant claim was not confirmed through experiments that we performed together. But it was not a case of fraud, it was a case of limited experience in a complex area. The reaction of that highly committed man was to recognize experimental data. A con artist would never allow an outsider to examine his "bag of tricks."

So now you have several excuses for bending a rule of your editorial policy. They are: a) the anniversary, b) the pending DOE investigation, c) my paper is a review describing (very objectively, and without accusations of any kind, as you probably noticed) several very different areas of a broad field, d) my background as an active nuclear physicist, and e) my unpublished research in two areas of cold fusion. You are certainly aware how difficult it is to publish cold fusion research papers in important scientific journals. Will the situation change after the pending DOE investigation of cold fusion? I hope so. Please help to contribute to this cause.

If you decide to approach Fleischmann, be aware that he is an electrochemist; I do not consider him to be an expert in nuclear physics. This became clear in 1989 and contributed heavily to the cold fusion controversy. One can only imagine what would happen if Fleischmann and Pons, who are chemists, refused to participate in the infamous press release, organized by the administrators of the University of Utah, and decided to work with Steven Jones, who is a physicist. A year or two later they would publish a peer reviewed paper and . . . But I refuse to speculate; my goal is heal the wound by focusing on purely scientific topics and by ignoring stupid things people said or wrote before. Please help me. I think that cold fusion, no matter what the final verdict will be, is a highly significant episode in the history of science. Let your journal be a part of that history.

P.S. (dated 3/28/04)
Do you want an author with a much better record of publishing cold-fusion-related papers than me? If so then let me make some suggestions. I already mentioned Martin Fleischmann. Here are some other names:

1) George Miley, University of Illinois
g-miley@uiuc.edu

2) Peter Hagelstein, MIT
phagelstein@aol.com

3) Edmund Storms, retired from LANL
storms2@ix.netcom.com

4) John Dash, Portland State University
dashj@pdx.edu

5) Steven Jones, Brighman Young University.
steven_jones@byu.edu

Actually Jones should be on top of this list because he was probably the first to use the term "cold fusion." This was in the title of his 1987 article in Scientific American; he was working under the DOE grant at that time.

As you know, I am now working on Jones' project and he corrected two serious mistakes in the draft of the paper I want to publish. Would you publish this paper if I convinced Jones to become a coauthor? If so I would be glad to ask him. Please be aware that, except Jones, none of the above knows about my initiative. But I know these people and you can say that the name was suggested by me. Best regards again, Ludwik Kowalski

3/29/04
REPLY FROM Dr. REID:
Dear Dr. Kowalski: By saying I'd share "the whole package," I meant that your longer manuscript (and the information you've provided in your messages) will provide good background for my colleagues in considering your proposal for a short essay. Thank you for the list of prospective authors provided in your "post scriptum." We'll consider that separately. Rosalind Reid

[Today is 6/28/04. I never heard from American Scientist again. Will they tell me about their decision to publish or not publish the short piece? I do not think so.]

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3/31/04 (Submitting paper to Scientific American):
Dear Dr. Rennie: I am sure that you are aware of the DOE move to review the cold fusion field, as reported in The New York Times (3/25/04). Attached is a review article which, I hope, can be published in Scientific American. The title is "Recent cold fusion claims: are they valid?" It is not a paper defending cold fusion claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers.

Some of these claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. The subject is interesting no matter what the final verdict of the second DOE evaluation will be.

Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. In writing this I was not aware of the pending DOE investigation.

I deliberately avoided references to social aspects, which are interesting but highly controversial. I am a physics teacher at Montclair State University. Studying cold fusion was my 2003/2004 sabbatical project, which resulted in the attached manuscript Sincerely yours, Ludwik Kowalski, Ph.D. Professor of Physics, Montclair State University, Montclair, NJ

[The answer came much later than I expected (6/14/04). here it is:]

Dr. Kowalski: Thank you for your offer to contribute to SCIENTIFIC AMERICAN. After much consideration, I regret to say that the piece you propose is not suited to our somewhat limited editorial needs. We appreciate your interest in SCIENTIFIC AMERICAN. Regards, Jacob Lasky Editorial Administrator. Did Dr Rennie, to whom my first message was addressed, retire?]

After giving up on Scientific American and tried to publish my paper in Nature. Instead of sending the article to them I decided to follow the presubmission path. The most impressive part of the path was that the negative reply came about ten hours later. The process of presubmission consists of filling two text boxes on their web site. The first box was for the letter about my article; I wrote essentially the same as what I wrote to the editor of Scientific American (see above). The second box was for the first paragraph of my paper, and for the references used in it. I wrote:

"Cold fusion (CF), presumably discovered 15 years ago, is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutron sources or stellar temperatures. In 1989, several months after the discovery was announced (through a press release at the University of Utah) a panel of scientists, appointed by the US Department of Energy (DOE), examined the evidence supporting the CF claims. That evidence was declared insufficient. But, as summarized in (1) 'there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system.'

CF became highly controversial and only several hundred researchers continued working on it, world-wide. Most scientists still think that cold fusion is pseudoscience. On that basis editors of many journals refuse to publish papers

devoted to CF research. Only a small fraction of scientists is familiar with recent progress in that area. The purpose of this article is to objectively summarize recent findings (2) and to supply references with which I am familiar. The article was triggered by the reported initiative of DOE to review (3,4) cold fusion research. I will focus on four cold fusion claims which are, in my opinion, the most important. As a nuclear physicist, and a physics teacher, I examined some of CF publications and attended one cold fusion conference (5).

1. Huizenga, J. *Cold Fusion: the Scientific Fiasco of the Century*. Oxford University Press, 2nd edition, Oxford, 1993.
2. Cold fusion papers are usually published at specialized scientific conferences. Many of them are downloadable from the library at http://www.lenr-canr.org.
3. Daviss, B. "No Cold Shoulder." *New Scientist*, March 20, 2004, p 6.
4. Feder, T. "DOE Warms to Cold Fusion," *Physics Today*, April 2004, page 27.
5. The Tenth International Conference on Cold Fusion was held in Cambridge, Massachusetts 24 - 29 August 2003. Conference proceedings, in the form of Pdf files, can be downloaded from http://www.lenr-canr.org/iccf10/iccf10.htm "

The reply was short and clear;
"Thank you for your inquiry about submitting your paper entitled 'Cold fusion 15 years later' to Nature. I regret that the paper that you describe seems unlikely to prove suitable for publication in Nature, and we accordingly suggest that you pursue publication elsewhere. I am sorry that we cannot respond more positively on this occasion. Yours sincerely Dr Karen Southwell, Senior Editor." I was aware, from browsing their web site, that the rate of acceptance in Nature is about 1 out of 10. On that basis I should have expected a rejection.

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Corresponding with Brian D. Josephson [a Nobel laureate]
Cavendish Lab, Cambridge CB3 0HE, U.K.
Dear Dr. Brian Josephson
I am one of many to whom the Physics Today article (about cold fusion) was distributed today by Eugene Mallove. At first I thought it was an April Fool's joke. Seeing your name on the list of e-mail destinations reminded me that you wrote about negative aspects of the cold fusion situation in the past. May I suggest that you write a Letter to the Editor of Physics Today with comments on the upcoming DOE evaluation. Considering your reputation that letter may become important. Please write what, in your opinion, the task of a new panel should be.

And here is his reply:
Dear Ludwik, Thanks for your e-mail. Steve Krevit and Nadine Winocur, editors of New Energy Times, have sent in a very good letter to PT, and I don't think there's very much worth adding to the combination of that and the original article, so probably I should leave it at that. But it was a good idea of yours! Actually New Scientist reported the situation last week (i.e. in March!), and those in the know had heard about the PT article before then. Brian J.

In a subsequent message Josephson added: ". . . Nothing's impossible in this domain -- Nature often does not publish letters from me. Anyway, I've sent in an endorsement of the other letter which I mentioned." Hmm, a paper from a Nobel laureate being rejected? This is hard to believe.

4/2/04
Letter to The Editor of Physics Today. (Responding to Ferder's article that was sent to me over the internet by Mallowe):

Dear Dr. Hanna: Assuming the Toni Feder's article distributed yesterday over the Internet was not an April Fool's joke,

I am sending you my reaction to it. Please publish it as a Letter to the Editor of Physics Today. Respectfully yours, Ludwik Kowalski

Here is the content my letter:

Cold fusion; science or pseudoscience?
I agree with Toni Feder (Physics Today, April 2004, page 27) that "skepticism about the credibility and reproducibility of cold fusion remains widespread." As a physics teacher who is not certain how students' questions about cold fusion should be answered, I welcome the upcoming DOE investigation of recent claims in this controversial area. Here are questions which I would like to see answered by the appointed investigators: **a)** Is it true that unexpected emission of neutrons, protons, tritons and alpha particles (at low rate) has been observed in several cold fusion experiments? **b)** Is it true that accumulation of ^{4}He, at the rate of about one atom per 24 MeV of excess heat, has been confirmed by many scientists, as reported by McKubre? **c)** Is it true that highly abnormal isotopic ratios have been found in some cold fusion setups? **d)** Is there any indication that leading cold fusion scientists are incompetent in the areas they investigate? **e)** Is there any indication that their data are fraudulent? **f)** Is the research methodology used by them different from the methodology used in other areas of physical science?

Answers to these questions will help me decide what to think about cold fusion and what to tell students about it. Speculations about practical applications of new findings, in my opinion, should be de-emphasized at this time. They will emerge naturally when basic scientific claims are recognized as valid, and when cold fusion researchers are no longer treated as if they were con artists and charlatans. The "chilling effect," mentioned by Randall Heckman, prevents young scientists from entering the area of cold fusion research. I agree with Allen Bard that being able to reproduce experimental results is not "good enough;" it is only a prelimanary step. But is it not true that poor reproducibility was the central point of criticism when cold fusion was first investigated fifteen years ago?

4/12/04
The editor wrote:
Dear Dr. Kowalski: Your letter has not yet gone to reviewers. There are quite a few in the queue ahead of it, and my reviewers are staff editors who are all busy in current assignments to get Physics Today's May issue out the door. I'll be happy to include your definition of cold fusion in the folder with your letter as it travels through the review process. That process should begin later this week. Thank you for your patience. Kind regards, Marty Hanna Letters Editor, Physics Today.

My immediate reply was:

Dear Dr. Hanna: Thank you for giving my letter to the editor a chance. I also hope that Feder's article will result in a lively discussion. If I had to write my letter again I would insert the opening sentence: "Cold fusion is any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutrons, cosmic rays, alpha particles or stellar temperatures." It is probably too late to introduce this change; I do not want to delay the letter.

Feel free to use my definition in any way you wish. I think that either you, or Tony Feder, should define cold fusion before the discussion begins. Unfortunately the term "cold fusion" means different things to different people. It is certainly premature to define cold fusion as a practically unlimited energy resource. But that is what most people say when asked to describe CF. I suspect that many disagreements about CF would disappear if its definition were accepted by all antagonists.

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4/17/04
Frustrated that my timely review of the Cold Fusion field is being delayed I decided to send it to another UK journal, New Scientist. I wrote:

Dear Editors: I have a 3048 word article entitled "Cold fusion 15 years later" which has just been rejected by a research-oriented journal. I would be happy to rewrite it (preferably with one of your science journalists as coauthor) for your readers. Below are three short descriptions of it: one for the general public; one for scientists; and one showing what I wrote to editors of the journal that rejected my review. I would be glad to e-mail you the article if you are interested.

Let me add that I am a physics professor and a researcher with many nuclear science publications. The paper I wrote is a summary of my 2002/2003 sabbatical project. I attended the 2003 International Cold Fusion Conference and have two photos which may be worth adding. Both Martin Fleischmann and Steven Jones (whose CF research triggered the 1989 controversy) gave me signed permission to publish the photos.

Description for general readers
Cold fusion (CF) has been a highly controversial area in science due to: the way in which the discovery was initially announced (via a press release in 1989), difficulties reproducing critical CF experiments, and the highly unusual nature of CF claims. The article describes four such claims and provides references for additional learning. The US Department of Energy (DOE) will soon review the CF field. Are CF claims valid? If they are then practical applications might be developed on the basis of new discoveries. The article focuses on basic science and not on potential applications.

Description for scientists
Cold fusion is defined as "any process in which a nuclear reaction is produced without relying on traditional means, such as particle accelerators, neutron sources, stellar temperatures, cosmic rays or alpha particles. " This article is a review of recent papers describes four cold fusion processes: a) emission of nuclear particles, such as neutrons and protons, b) generation of excess heat of nuclear origin, c) accumulation of reaction products with highly unusual isotopic ratios, and d) nuclear transmutation of elements. The purpose is to explain cold fusion and to provide references. The article was prompted by the pending review of the field in the US.

Description for editors
I am sure that you are aware of the DOE move to review the cold fusion field. This event prompted me to submit a review article which, I hope, can be published. It is not an article defending cold fusion claims; it is an article describing the most important cold fusion reports objectively. Some of the claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the 1989 claims made by Pons and Fleischmann. The strange thing is that authors of described reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article, containing 37 references, does not try to answer these questions; its purpose is to present a summary of what has been reported in the last ten years, without taking sides. The subject is interesting no matter what the final verdict of the second DOE review will be.

5/3/04
Neither Scientific American nor New Scientist confirmed that my submitted paper was received. I have no indication from Physics Today that my letter to the editor will be published. Should I ask for the confirmation of this? No, I will wait another two or three weeks. Also no words from American Scientist (about the Golden Egg piece that they said will be considered). Facing this situation I submitted the article (slightly modified) to The Physics Teacher today. Here was the letter to Dr. Mamola to which the manuscript was attached.

Dear Dr. Mamola: As you probably remember, the manuscript on Cold Fusion that I submitted about two years ago was rejected by your reviewers. My letter to the editor, however, was published last summer. I was pleased by this. The topic, as you know (see the "DOE WARMS to Cold Fusion" article in last April issue of Physics Today), is likely to be of great interest in the near future. With this in mind I wrote a **new article** on Cold Fusion and I hope that you will be able to publish it next fall. As you will see, I am simply describing controversial claims, I am not defending them. An extensive list of references is provided for those teachers who might wish to familiarize themselves with recent papers. The length is 3302 words, including 37 references. If necessary I can shorten the article, and reduce the list of references. But I prefer not to do this because I believe that everything is important.

I think I already wrote to you that my renewed interested in Cold Fusion was triggered by an accidental encounter, and

that my 2001/2003 sabbatical year was devoted to literature research in that field. I am still undecided about validity of cold fusion claims but I think that they should be known to physics teachers. Unfortunately, most of them are, as I was before September 2001, not familiar with experimental data gathered in the last ten years. The pending evaluation of the field by the DOE is likely to be publicized in the media and lead to student interest and questions. Hopefully, my paper will help teachers deal with the renewed interest in the "forbidden field." The manuscript, in MS Word format, is attached.

The following confirmation from the journal arrived in less than two hours.

The Physics Teacher magazine acknowledges receipt of your manuscript "Cold Fusion 15 Years Later" on 5/3/04. Your manuscript has been assigned the number: 420502. Our editorial office will appreciate it if you refer to this number in any future correspondence regarding the submission. After the editor has reviewed your manuscript, we will contact you with his comments and/or recommendation. Thank you for your interest in TPT.

6/ 9/04
More that a month later I received the following rejection: "Dear Professor Kowalski: We have reviewed your manuscript "Cold Fusion 15 Years Later" in the light of the recent Physics Today article "DOE Warms to Cold Fusion." While a paper in TPT on this subject may be warranted, we do not believe there is any great urgency to publish one immediately. After all, according to the Physics Today piece, DOE Deputy Director Decker says that their "review of cold fusion will begin in the next month or so [that was back in April]" and it "won't take a long time –- it's a matter of weeks or months." We believe that it would be premature to publish a cold fusion paper in TPT before the results of the DOE review are announced. Were we to do so, a follow-up piece would almost certainly be required later, regardless of how that review turns out, and we don't feel that two papers on the subject are warranted. We will consider your paper again (along with any revisions induced by the DOE report) after the report is made public."

My immediate reply was: "Dear Dr. Mamola: Was my manuscript examined by referees? I would very much like to see what they had to say about its content. Thanks in advance."

6/28/04
I am still waiting for referee's comments. What should I do? I will wait a little longer and then post the review article as item #152 on my web site. How many people will read it? i do not know. I will also turn this summary into a poster for the next Cold Fusion Conference. The fight for a better climate in the area of Cold Fusion is going to continue. Will the pending DOE review end the unhealthy feud about cold fusion? Will it result in elimination of administrative barriers (such as rejection of articles without the peer review process)? What motivates defenders of the status quo? Who benefits from it? Yes these questions belong to the realm of social sciences. But that does not mean they should remain unanswered.

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154) History of attempts to publish a paper

Ludwik Kowalski (6/29/04)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

My 2004 paper, reviewing recent cold fusion claim, has been rejected (without sending it to referees and without offering any criticism) by editors of six journals:

1) Physics Today, USA
2) American Scientist, USA
3) Scientific American, USA
4) Nature, UK
5 New Scientist, UK
6) The Physics Teacher, USA
7) Science, USA

This poster traces histories of rejections. The paper itself can be seen as items #152 at my web site Montclair State University:

http://csam.montclair.edu/~kowalski/cf/

- -

1) February 12, 2004
Here is how my paper was introduced to the editor of one of the above journals. Other accompanied letters were similar.

I am sure that you are aware of the DOE move to review the cold fusion field, as reported in The New York Times (3/25/04). Attached is a review article which, I hope, can be published in Scientific American. The title is "Recent cold fusion claims: are they valid?" It is not a paper defending cold fusion claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers.

Some of these claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. The subject is interesting no matter what the final verdict of the second DOE evaluation will be.

Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. In writing this I was not aware of the pending DOE investigation. I deliberately avoided references to social aspects, which are interesting but highly controversial. I am a physics teacher at Montclair State University. Studying cold fusion was my 2003/2004 sabbatical project, which resulted in the attached manuscript

In rejecting my paper the editor of **Physics Today** wrote: " Dear Dr. Kowalski: We received your article submission titled, "Recent Cold Fusion Claims: Are They Valid?," and appreciate your sending it to Physics Today. After reviewing

it, however, we have concluded that it does not meet our editorial needs. Thank you for your interest in Physics Today. Sincerely, Stephen G. Benka Editor-in-Chief."

That is it. Not a single word about the content of the article. How can the phrase "does not meet our editorial needs" be interpreted? Why was the article not sent to referees? They do publish many field summaries each year. Why was my summary not given the same chance to be reviewed by experts? Was I writing about sociology, poetry, business or something else unconnected to physics? Are recent cold fusion claims described in the article already widely known to most physicists? Was my description of these claims erroneous? Was the article rejected because of its style, its limited scope, or its disregard for ethical standards?

And here how the editor of **American Scientist** responded to my submission:
Dear Dr. Kowalski: Yes, we've received your original manuscript and the follow-up. I'm afraid we're not always able to acknowledge receipt immediately. I try to give a prospective author an idea of whether we'll be able to consider a manuscript, and sometimes it takes a little time to determine that. We have certain basic criteria for submissions. When a submission does not meet those criteria, I prefer to say that it cannot be considered rather than simply acknowledge receipt.

In the case of this submission, I'm unsure. We publish feature-length articles and commentaries based on original published research. The authors of American Scientist articles are the people who have done the work and therefore are in a position to survey their own field. I don't actually have evidence (in the form of cited publications or a c.v.) that you have done original research on the topic you propose to write about.

If you would like to publish a short commentary, we do have a department with different criteria, called "Macroscope." This is where we publish short essays conveying a scientist's point of view on a matter of personal or professional interest to scientists and engineers. The maximum word count is 1,500. If you would like us to consider publishing your piece in a short form, please let me know, and I'll share it with my colleagues and let you know the response. Sincerely, Rosalind Reid Editor, American Scientist

Responding to the above I wrote:
Dear Dr. Reid: Thank you for your prompt reply. I understand your hesitation. Protecting readers of American Scientist from people who are not qualified to write about science should be one of your tasks. To help you decide here is a little summary about myself.

I am an experimental nuclear physicist (Ph.D., 1963) with a large number of publications (mostly as coauthor) in that field. The attached abbreviated list of publications, spanning four decades, makes it clear that my teaching commitment has not prevented me from active participation in nuclear physics research. Like most scientists, I accepted the 1989 verdict about cold fusion. And you are correct, I have no publications about cold fusion. My new interest in this field was triggered in October 2002. I attended a nuclear conference in New Mexico and heard several scientists talking about cold fusion research. It was the beginning of my sabbatical year. The paper I submitted is the product of that work.

I hope your hesitation will not prevent you from sending my article to competent and unbiased reviewers. Please let me know what your decision will be. Meanwhile I would like to follow your suggestion about writing a short commentary on the anticipated review of cold fusion by the DOE; see the attached file. Thank you for your consideration. Sincerely yours, Ludwik Kowalski"

A list of my selected publications, and a file containing my "short piece" (see below), were attached.

SEEK NOT THE GOLDEN EGG, SEEK THE GOOSE

According to a recent article in The New York Times (3/25/2004) the US Department of Energy (DOE) is going to review the field of cold fusion this year. This is a significant event; the controversial field of cold fusion (CF) has often

been called pseudoscience. If it were up to me I would suggest that the panel of DOE scientists focus on essential scientific questions and not on practical applications which are far away, at best. Promising too much, and too early, was one of the mistakes made fifteen years ago. In my opinion the six most important scientific questions are:

1) Are unexpected neutrons, protons, tritons and alpha particles emitted (at low rates) in some CF experiments?
2) Is generation of heat, in some CF experiments, linearly correlated with the accumulation of ^{4}He at the rate of 24 MeV per atom of ^{4}He?
3) Have highly unusual isotopic ratios been observed among the elements found in some CF systems?
4) Have radioactive isotopes been produced in some CF systems?
5) Has transmutation of elements occurred in some CF setups?
6) Are the ways of validating scientific findings in the areas of CF research consistent with accepted methodologies in other areas of science?

I think that a positive answer to even one of these six questions should be sufficient to justify an official declaration that "cold fusion, in light of recent data, should be treated as a legitimate area of research." The normal peer review mechanisms will then be used to separate valid claims from wishful thinking.

In a subsequent reply I wrote: " ... I already mentioned two reasons making such review urgent: the 15th anniversary of the Utah announcement and the pending DOE investigation. In my opinion, by publishing my paper, or a review written by somebody else, you will contribute to something desirable. Nobody is happy with the unhealthy feud between a group of well motivated researchers and official representatives of "mainstream science." Most people are passive but those who do take extreme positions often use highly pejorative adjectives, such as "pathological", " stubborn, " misguided," and "fraudulent." Please do not miss an opportunity to contribute to ending this unnecessary feud. I would be happy to give you names and addresses of top people in five main areas of cold fusion. . . .

So now you have several excuses for bending a rule of your editorial policy. They are: a) the anniversary, b) the pending DOE investigation, c) my paper is a review describing (very objectively, and without accusations of any kind, as you probably noticed) several very different areas of a broad field, d) my background as an active nuclear physicist, and e) my unpublished research in two areas of cold fusion. You are certainly aware how difficult it is to publish cold fusion research papers in important scientific journals. Will the situation change after the pending DOE investigation of cold fusion? I hope so. Please help to contribute to this cause.

If you decide to approach Fleischmann, be aware that he is an electrochemist; I do not consider him to be an expert in nuclear physics. This became clear in 1989 and contributed heavily to the cold fusion controversy. One can only imagine what would happen if Fleischmann and Pons, who are chemists, refused to participate in the infamous press release, organized by the administrators of the University of Utah, and decided to work with Steven Jones, who is a physicist. A year or two later they would publish a peer reviewed paper and ..... But I refuse to speculate; my goal is heal the wound by focusing on purely scientific topics and by ignoring stupid things people said or wrote before. Please help me. I think that cold fusion, no matter what the final verdict will be, is a highly significant episode in the history of science. Let your journal be a part of that history. . .

I also gave Dr. Reid names and e-mail addresses of five people (who are certainly much more knowledgeable than myself) suggested that she contacts one of them to write a longer review paper of the journal. Steven Jones, Martin Fleicshmann and George Miley were among the scientists I selected. I did not hear never from Dr. Reid again. Will she accept my "short piece?" Probably not. The "long piece" was submitted to **Scientific American.** Here is the reply that came after a long delay:

"Dr. Kowalski: Thank you for your offer to contribute to SCIENTIFIC AMERICAN. After much consideration, I regret to say that the piece you propose is not suited to our somewhat limited editorial needs. We appreciate your interest in SCIENTIFIC AMERICAN. Regards, Jacob Lasky Editorial Administrator."

After giving up on Scientific American and tried to publish my paper in **Nature**. Instead of sending the article to them I decided to follow the presubmission path. The most impressive part of the path was that the negative reply came about ten hours later. The process of presubmission consists of filling two text boxes on their web site. The first box was for the letter about my article; I wrote essentially the same as what I wrote to other editors. The second box was for the first paragraph of my paper, and for the references used in it. The reply was short and clear:

"Thank you for your inquiry about submitting your paper entitled 'Cold fusion 15 years later' to Nature. I regret that the paper that you describe seems unlikely to prove suitable for publication in Nature, and we accordingly suggest that you pursue publication elsewhere. I am sorry that we cannot respond more positively on this occasion. Yours sincerely Dr Karen Southwell, Senior Editor."

I was aware, from browsing their web site, that the rate of acceptance in Nature is about 1 out of 10. On that basis I should have expected a rejection. Frustrated that my timely review of the Cold Fusion field is being delayed I decided to send it to another UK journal, **New Scientist**. But they never responded. After waiting about a month the article was submitted to **The Physics Teacher**, a journal in which several of my teacher-oriented review papers were published in the past. In submitting the article I wrote:

Dear Dr. Mamola: As you probably remember, the manuscript on Cold Fusion that I submitted about two years ago was rejected by your reviewers. My letter to the editor, however, was published last summer. I was pleased by this. The topic, as you know (see the "DOE WARMS to Cold Fusion" article in last April issue of Physics Today), is likely to be of great interest in the near future. With this in mind I wrote a **new article** on Cold Fusion and I hope that you will be able to publish it next fall. As you will see, I am simply describing controversial claims, I am not defending them. An extensive list of references is provided for those teachers who might wish to familiarize themselves with recent papers. The length is 3302 words, including 37 references. If necessary I can shorten the article, and reduce the list of references. But I prefer not to do this because I believe that everything is important.

. . . I am still undecided about validity of cold fusion claims but I think that they should be known to physics teachers. Unfortunately, most of them are not familiar with experimental data gathered in the last ten years. The pending evaluation of the field by the DOE is likely to be publicized in the media; this will lead to student interest and questions. Hopefully, my paper will help teachers deal with the renewed interest in the "forbidden field."

More that a month later I received the following rejection:
"Dear Professor Kowalski: We have reviewed your manuscript "Cold Fusion 15 Years Later" in the light of the recent Physics Today article "DOE Warms to Cold Fusion." While a paper in TPT on this subject may be warranted, we do not believe there is any great urgency to publish one immediately. After all, according to the Physics Today piece, DOE Deputy Director Decker says that their "review of cold fusion will begin in the next month or so [that was back in April]" and it "won't take a long time –- it's a matter of weeks or months." We believe that it would be premature to publish a cold fusion paper in TPT before the results of the DOE review are announced. Were we to do so, a follow-up piece would almost certainly be required later, regardless of how that review turns out, and we don't feel that two papers on the subject are warranted. We will consider your paper again (along with any revisions induced by the DOE report) after the report is made public."

My immediate reply was: "Dear Dr. Mamola: Was my manuscript examined by referees? I would very much like to see what they had to say about its content. Thanks in advance." This message has not yet been answered. Will I see the referee's comments? Probably not.

Will the pending DOE review end the unhealthy feud about cold fusion? Will it result in elimination of administrative barriers (such as rejection of articles without the peer review process)? What motivates defenders of the status quo? Who benefits from it? Yes these questions belong to the realm of social sciences. But that does not mean they should remain unanswered.

This afternoon the review manuscript was submitted to the Editor in Chief of **Science**, Donald Kennedy. Here is the reply received next day, Staurday afternoon: " I've consulted with our editorial staff in the physical sciences. Unfortunately, we don't think this topic is an appropriate one for review in Science at this time. Thanks for thinking of Science. Sincerely yours. Donald Kennedy." Hmm, very efficient; they had only couple of hours on Friday to read the manuscript. It was rejected because the topic is not appropriate. Why is it not appropriate? Aren't the described claims scientific?

A more detailed description of correspondence with the editors can be found in item #153 on my website dedicated to cold fusion:

http://csam.montclair.edu/~kowalski/cf/

P.S. Novenber 30, 2004 (One day before the DOE report was released)

Dear Dr. Mamola:

As you might recall (see the message quoted below) I wanted to publish a paper "Cold Fusion 15 Years Later," several months ago. A that time I was essentially an uncommitted observer of the field. My review paper is still waiting to be modified after the DOE panel report, as you suggested. The purpose of this message is to inform you that I have recently replicated two "cold fusion" experiments of Dr. Oriani, at the University of Minnesota. The label "cold fusion" is not appropriate here because neither heavy water nor palladium were used in the setup. And we do not believe that two isolated atomic nuclei could fuse at a low temperature to produce an observable effect. The probability of fusion, due to the tunneling effect, is too small for this. The only link between the concept of "cold fusion" and what we observed is a totally unexpected nuclear effect caused by chemical activity in an experimental setup. The Oriani effect was replicated by me twice (out of two attempt), as described at:

http://csam.montclair.edu/~kowalski/cf/188oriani.html

I know that most nuclear physicists are convinced that chemical processes cannot produce nuclear effects. But I also believe that experiments have priority in Physics. A task of trying to prove, or disprove, a controversial claim, made by a reputable scientist (such as Dr. Richard Oriani), can be turned into an educational project. What can be a better way to expose students to the excitement of scientific research? Please let me know if we should submit a paper, based on the above webpage, for a possible publication in The Physics Teacher. I am now trying to get ready for one more experiment, with a totally new cell, with new chemicals and at our university laboratory. The article will be submitted if the number of tracks is again much larger than the background.

The purpose would be to inform teachers about Oriani effects, and to encourage them to test one of them. Many high schools and universities are equipped to perform the simple experiment described in my webpage. Hopefully, some of them will attempt to replicate it. Please reply as soon as possible; we hope you will give our paper a chance of being published in TPT. The cold fusion, and the controversy about it, will not even be mentioned in our draft.
Sincerely yours,
Ludwik Kowalski

> Dear Professor Kowalski:
> We have reviewed your manuscript "Cold Fusion 15 Years Later" in the
> light of the recent /Physics Today /article "DOE Warms to Cold Fusion."
> While a paper in /TPT/ on this subject may be warranted, we do not
> believe there is any great urgency to publish one immediately. After all, . .
The rest of this message can be seen above.

P.S. December 13, 2004

Dear Professor Kowalski:

Our editorial staff has examined the discussion given at the URL provided in your message. While the materials required to conduct the Oriani (c) experiment might be accessible to schools, we believe that too few of our readers would consider the exercise to be appropriate for inclusion in their introductory-level physics courses. Because of space constraints, we are now able to publish fewer than one-third of the submissions we receive. We must therefore take care to select those which we believe would be of the greatest benefit to our readers. While we would be willing to review a completed manuscript of the sort you describe, we are not optimistic that it would be accepted for publication.

Sincerely, Karl C. Mamola
Editor

Return to the clickable list of items

Return to the clickable list of items

155) A VERY GOOD ARTICLE OF EDMUND STORMS

Ludwik Kowalski (7/1/04)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

What follows is an excellent article; it should be read by those who are interested in excess power resulting from the so-called "cold fusion." The author, who I met at the 10th International Cold Fusion Conference (August 2003), is an authority in the area of excess heat. He uses the acronym LENR (low energy nuclear reactions) wher many usethe trem "cold fusion." The pdf file containing the article (with figures which I do not show) can be downloaded from:

http://www.lenr-canr.org

As in unit #148, I inserted many questions (capital letters in red) into Storms' paper. The purpose, as before, is to show things that were not obvious to me. Note that some of the questions asked in unit #148 were unexpectedly answered, by Richard Eskimos, in unit #150. Perhaps somebody will answer questions formulated in this unit. I also inserted some comments.

Calorimetry 101 for Cold Fusion; Methods, Problems and Errors

Edmund Storms
Energy K. Systems
Santa Fe, NM

(1***) WHY THERE IS NO DATE? (THE ACKNOWLEDGEMENT SHOWS 2003)

ABSTRACT
Application of calorimetry to cold fusion or LENR presents unique problems that have not been previously summarized. This paper discusses various calorimetric methods that have been applied to the subject and evaluates each in light of what has been discovered about their limitations and errors based on experimental studies. Such information is essential to a study of the effect and to evaluate the results.

I. INTRODUCTION

LENR (Low Energy Nuclear Reactions) is a new mechanism for initiating nuclear reactions within special solid materials without application of energy normally required to overcome the Coulomb barrier. When the mechanism is applied to the D-D fusion reaction, the better known term "Cold Fusion" is used. These reactions produce three kinds of evidence: electromagnetic radiation, nuclear products, and heat. The radiation is generally weak and most is absorbed before it can reach the detector. Nuclear products can be detected, but analysis is difficult because the products occur in such small amounts. Finally, the abnormally large amount of heat energy that is generated can be measured. This

technique requires use of calorimetry, as described here.

Calorimetry is the measurement of heat energy. Although this is an old science that underpins much of thermodynamics and modern chemistry, when such measure-ments are applied to LENR, several unique problems become immediately apparent. For the LENR effect to be initiated, normal energy is frequently required as electric power to produce higher temperature, to generate hydrogen ions, or to stimulate the process in various ways. Consequently, the small amount of anomalous energy being sought is frequently superimposed on a large heat flux resulting from normal processes. As a result, the calorimeter must be able to handle large heat flux while being sufficiently sensitive and stable to see the small amounts of anomalous energy. This requirement has severely limited calorimeter design and has complicated evaluation of results. However, as better calorimetry has been applied to the problem, sufficient anomalous power has been reported to completely overwhelm uncertainties in heat measurement. This situation continues to improve as ways are found to generate increasing amounts of anomalous power.

Heat flux can be measured either by noting the temperature drop across a thermal barrier or by measuring the temperature rise of a material, generally a liquid. The first relies on thermal conductivity and the second uses the heat capacity of relevant materials. The isoperibolic and Seebeck methods use the former method and flow calorimetry uses the latter. These three methods will be examined here in detail.

(2***) THESE THREE CALORIMETERS ARE DESIGNED TO MEASURE THE RATE AT WHICH HEAT IS ESCAPING FROM A VESSEL. IN A STEADY STATE IT IS ALSO THE RATE AT WHICH ENERGY IS GENERATED IN A DEVICE INSIDE THE VESSEL. THE CALORIMETERS WE USE IN SCHOOLS OFTEN MEASURE TOTAL AMOUNTS OF HEAT PRODUCED RATHER THAN THE RATE (FOR EXAMPLE WHEN ONE GRAM OF WOOD IS REACTING WITH OXYGEN). THE RATE AT WHICH THE CONSUMPTION OF FUEL TAKES PLACE CAN BE EITHER LARGE OR SLOW.

A calorimeter needs to be calibrated using a known source of energy, the most convenient being Joule heat created at a resistor located within the cell or heat generated during electrolysis of a “dead” cathode. (FOOTNOTE: Newly cleaned platinum does not produce anomalous energy when used as the cathode. However, if the platinum cathode is electrolyzed for sufficient time without cleaning, it may start to produce anomalous energy[1].) This energy must have the same effect on calorimeter behavior as does the process being studied. However, the calibration constant contains systematic errors in addition to normal random error. Both errors may be different from errors occurring when LENR heat is measured. Random error can be evaluated by noting the scatter in repeated calibration values, generally acquired over a range of applied calibration power. The much larger systematic error is more difficult to evaluate and depends on the calibration method and calorimeter design. Also, this error will slowly change with time as various uncontrolled variables change. As a result, data may appear to be very consistent yet be very wrong, sometimes in ways that are hard to evaluate.

This paper describes my experience designing and using three types of calorimeters and applying the designs to a search for heat generated by LENR in electrolytic cells, as pioneered by Pons and Fleischmann[2-4]. Rather than speculating about potential error, this paper examines experimental measurements of many real errors that must be considered. The sources of error found during these studies apply to all calorimetric methods used to study LENR.

II. DESCRIPTION

II.1
Isoperibolic Calorimetry:

(3***) WHAT DOES THE WORD PERIBOLIC MEAN?

Two general types of isoperibolic calorimetry have been applied to electrolytic cells in which LENR is proposed to occur. For the first, called here single wall isoperibolic calorimetry (SWIC), temperature is measured within the

electrolyte and this value is compared to the temperature outside the container, which is generally made of glass, as shown in Figure 1. This method is the least accurate, but the most commonly used of the methods that will be described here.

FIGURE 1 (calorimeter of the first kind)
Cartoon of a Single-Walled Isoperibolic Calorimeter. Dashed arrows show the direction of heat flow caused by bubble action. The top of the electrolyte is warmer than the bottom because warmed water rises from the cathode-anode assembly and energy is lost through the wall as convection causes flow from the top to the bottom near the wall. The measured inside temperature very much depends on where the detector is located.

(4***) WHAT IS “BUBBLE ACTION?”

The inside temperature is determined using thermistors or thermocouples, preferably multiple sensors at different depths and locations, while the outside temperature can be uniform and well known if the cell is contained in or surrounded by a constant-temperature environment. A drawing is shown in Figure 2 of an actual isoperibolic calorimetry. In this case, the cell is surrounded by a jacket through which water flows to maintain constant temperature. Other people have immersed their cells in a fluid bath or within constant-temperature air. The latter method is especially error prone because the true outside temperature at the thermal barrier is difficult to determine with sufficient accuracy.

FIGURE 2. Drawing of an isoperibolic calorimeter using an external cooling jacket. Notice that temperature is measured at two locations within the electrolyte in this cell. This design is typical of cells used to obtain gradient data described in this paper.

(5***) WHAT PRESSURE IS MEASURED BY THE GAUGE? FOR WHAT PURPOSE?

(6***) WHY IS THE ROLE OF THE RECOMBINER NOT EXPLAINED? I WOULD ANSWER THIS QUESTION HERE AND NOT MUCH LATER IN THE ARTICLE.

(7***) IS AIR SEALED HERMETICALLY IN THE DEWAR? I SUPPOSE IT CAN BE SEALED BECAUSE THE VOLUME OF WATER DOES NOT CHANGE (DUE TO THE RECOMBINER). P.S. THIS QUESTION IS ANSWERED IN SECTION **II.6** BELOW.

Accurate calibration and subsequent measurements require the actual average temperature drop across the thermal barrier be equal to the measured temperature drop. Because this ideal condition is seldom achieved, the temperature error must be at least independent of conditions within the calorimeter. Neither of these conditions is easy to achieve because random variations in thermal gradient cause unexpected fluctuations in delta T, even when heat flux is constant. This criticism was raised very early in the field’s history[5]. Figure 3 shows how the gradient within the electrolyte between the top and bottom changes as power is applied to electrolytic action or to an internal resistor.

FIGURE 3. Comparison between the gradient within the electrolyte produced by electrolysis or by power applied to an internal resistor (heater) that is used for calibration.

(8***) WHY ARE GRADIENTS INSIDE THE LIQUID IMPORTANT? MY IMPRESSION, BASED ON FIGURE 1, IS THAT TO CALIBRATE MEANS TO ESTABLISH A RELATION BETWEEN THE GRADIENT ACROSS THE WALL AND POWER.

Notice that the gradient is very small when electrolysis is used, because bubbles stir the liquid, but large when a heater is used. Application of even a small amount of electrolytic power along with heater power reduces the gradient to a value similar to that produced by electrolysis alone.

(9***) WHAT IS THE NATURE OF HEATING BY “ELECTROLYSIS?” IS THIS ONLY I^2*R, WHERE R IS THE RESISTANCE OF THE ELECTROLYTE?

FIGURE 4. Temperature gradients produced using power applied to a heater alone; to electrolysis alone; and when heater power is combined with electrolysis.

FIGURE 5. Effect of applying increasing electrolytic power to a fixed power applied using the heater. The temperature gradient is the difference between the top and bottom of the electrolyte. The heater power is about 10 watts.

(10***) THE X AXIS IN FIGURE 5 IS "ELECTROLYTIC CURRENT" WHILE IN FIGURES 4 AND 5 IT IS "APPLIED POWER." WHY IS IT SO? WAS THE VOLTAGE CHANCING FOR THE DATA IN FIGURE 5?

FIGURE 6. Effect of stirring on the gradient within a cell during electrolysis as a function of applied power.

Figure 4 shows what happens when electrolytic power is applied along with heater power. As shown in Fig. 5, applying even a small amount of electrolytic current can significantly reduce the gradient because bubble action reduces the gradient produced by the heater. This behavior was noted and utilized by Pons and Fleischmann during their early studies and it is why gradients were not important in their work, in spite of assumptions to the contrary[5, 6].

Stirring the electrolyte can reduce gradients, but introduces other problems. Stirring, as shown in Fig. 6, can even reduce the small gradient present during electrolysis. In addition, stirring makes the measurements more stable. Figure 7 compares calibration curves obtained using power applied to electrolysis alone and when power is applied to an internal resistor along with electrolysis. This figure shows that mechanical stirring will reduce but not eliminate the error introduced using resistor calibration.

(11***) WHAT DOES NEGATIVE GRADIENT MEAN IN FIGURE 5?

(12***) WHAT KIND OF ERROR IS BEING MENTIONED? HOW LARGE IS IT ACCORDING TO FIGURE 7?

Mechanical stirring, while reducing the difference between electrolytic and resistor calibration, introduces another error. Stirring reduces the thickness of stagnate fluid next to the barrier, which changes the overall thermal conductivity of the barrier. As a result, the calibration constant changes with stirring rate, as shown in Fig. 8. Consequently, when stirring is used, the rate must be held very constant. The gradient can also change gradually during the experiment causing apparent excess energy to develop. Figure 9 shows the gradient as a function of time. Such changes are very difficult to predict and can introduce serious systematic error if the cell is not frequently calibrated. Application of external magnetic fields will also change the gradient because the path of ions moving between the anode and cathode will be changed, thereby causing flow of heated electrolyte to change. These factors will change the temperature within the electrolyte at positions where temperature is measured.

FIGURE 7.
Calibration curves resulting from power applied to electrolysis or to an internal resistor plus electrolysis while the electrolyte is stirred or not stirred. Delta T is the temperature measured across the cell wall by averaging two thermistors located at the top and bottom of the electrolyte.

Pons and Fleischmann[7, 8] used a cell insulated by a silvered vacuum Dewar through which heat was lost mainly by radiation. Consequently, heat loss was proportional to ΔT^4 through part of the cell and proportional to ΔT through the lid. Energy was applied to a dead cathode in increasing steps and the resulting ΔT was used to calculate a single value for the effective thermal conductivity of the cell over a range of temperature. In addition, the cell was calibrated by applying a pulse of power to a resistor, which caused the temperature to rise and then fall in a manner determined by the heat capacity of the cell combined with heat loss from the cell. This calibration method had the advantage that it allowed sudden busts of anomalous energy to be measured using the rate of temperature change, without having to wait for the cell to stabilize. Several evaluations [9-11]of the method concluded that their procedure was sufficiently accurate to justify some, but not all, of their claims for anomalous energy. However, other studies have not used such a complex calorimeter or used this adiabatic method for calibration.

(13***) WHAT IS THE ADIABATIC METHOD OF CALIBRATION?

Instead, steady-state measurements, where sufficient time is allowed for generated power and the resulting temperature to become constant, give results easier to evaluate.

(14***) I AM STILL CONFUSED ABOUT THE GRADIENTS OF TEMPERATURE. IN FIGURE 1 IT IS BETWEEN INSIDE AND OUTSIDE; IN ALL OTHER FIGURES IT IS BETWEEN DIFFERENT POINTS IN THE SOLUTION.

FIGURE 8.
Effect of stirring rate on the calibration constant. The average temperature across the cell wall was based on the average of two thermistors located at the top and bottom of the electrolyte. The cell was stirred using a Teflon covered cross-shaped bar driven by a rotating external magnet. The calibration constant does not become constant at any practical stirring rate.

(15***) THE LABEL ALONG THE AXIS IS "CONDUCTION CONSTANT" BUT THE FIGURE CAPTION REFERS TO THE "CALIBRATION CONSTANT." WHY IS IT SO?

The outside temperature is generally uniform and well known, except when a cell is air-cooled. Air-cooling introduces additional uncertainty because the temperature immediately next to the container surface is not known and is subject to unstable convection currents even when a fan is used. As a result, loss of heat through the thermal barrier can be very unstable even though temperature of the surrounding air is constant.

Because these errors and ones described below are sensitive to uncontrolled variables even under the best of conditions, the method as commonly used can not be trusted to give absolute values to an accuracy better than ±200 mW, although apparent changes in power at the 20 mw level can be detected. These characteristics make the method poor for making absolute measurements, but adequate for revealing small changes in anomalous power, provided they are part of a consistent pattern using many measurements and if the calorimeter is frequently calibrated using proper techniques. Fortunately, anomalous power is initially absent and grows relative to an initial constant background. Consequently, absolute values for background power are not required as long as the background is stable.

(16***) THAT IS WHAT MY FRIEND ESKIMOS WAS SAYING IN UNIT #151.

Even so, claims for anomalous heat based on this method cannot be trusted if they are less than about 100 mW.

FIGURE 9.
Observed changes in the gradient as a function of time. In this case, a thermistor was located at the cathode in addition to ones at the top and bottom of the electrolyte.

The second method, called double wall isoperibolic calorimetry (DWIC) avoids the gradient problem by using a second thermal barrier outside the cell. A cartoon is shown in Figure 10. Temperature is measured on the inside of this barrier, but outside of the container in which liquid electrolyte is located. Consequently, thermal gradients within the electrolyte have much less effect on measured temperature. As a result, this method is much more accurate than the former and can be made very sensitive, as described by Miles[13] and Huggins et al.[14, 15]. Only the former method (SWIC) is discussed here because its limitations are greater and its use is more common than the DWIC design, even though the latter design is much better. In addition, such cells tend to run hotter for the same applied power compared to the SWIC design, a condition that appears to improve energy production. Details about the design and characteristics can be found in the cited papers.

FIGURE 10.
Cartoon of a double wall isoperibolic calorimeter. Arrows show the direction of heat flow caused by bubble action. The temperature shown as "Inside T" needs to be the average temperature of the cell, which can be obtained using several detectors located in a conducting metal jacket made of copper or aluminum.

II.2

Flow Calorimetry:

Flow calorimetry is more accurate and reliable than is SWIC because it is based on measuring heat energy used to raise the temperature of a known amount of fluid, generally water. Only three quantities need be measured; these being the temperature of fluid entering the calorimeter, fluid temperature leaving the apparatus, and the flow rate. Applied power is then plotted against the flow rate, F (g/min), times temperature increase of the fluid, ΔJ (°C). Corrections must be made for the small amount of heat energy that leaves without being added to the fluid, generally from the cell lid. Generally, this loss is less than 2% of the total amount of energy being produced within the calorimeter and can be determined by applying a known amount of electrical energy to the cell, The stability of such calorimeters is excellent provided they are located within a constant-temperature environment. Because the calibration constant is independent of where heat is produced within the calorimeter, gradients within the electrolytic cell are unimportant. This assertion is easily demonstrated when heat, supplied from a resistor, is compared to heat generated by electrolysis, as shown in Fig. 11. These two methods generate heat in different locations because during electrolysis, bubbles reduce the gradient and heat is generated at the recombiner in addition to at the electrodes. In contrast, a resistor produces large gradients and no heat is released at the recombiner or at the electrodes. Yet, the calibration constant obtained from the two methods is virtually identical.

FIGURE 11. Comparison between values obtained by using electrolysis and/or Joule power. The before and after values were obtained during a study lasting hundreds of hours during which time a cathode produced excess energy. The electrolytic power was based on using a dead cathode.

(17***) I SUPPOSE THE “CALIBRATION CONSTANT” IS THE SLOPE OF THE LINE SHOWN IN FIGURE 11. IS THIS THE VALUE TO WHICH THE CAPTION OF FIGURE 8 REFERRED? HOW IS THE CALIBRATION CONSTANT RELATED TO CONDUCTION CONSTANT PLOTTED ALONG THE Y AXIS IN FIGURE 8?

(18***) FIGURE 11 IS CONFUSING. WHAT IS ^{2}J ALONG THE X AXIS? WHY IS IT NOT DT (DIFFERENCE OF TEMPERATURES)? WHICH TWO TEMPERATURES?

A picture of a flow-type calorimeter is shown in Fig. 12 along with a drawing showing how the parts are related in Fig. 13. Constant-temperature cooling fluid is provided at a location near the input of the calorimeter by pipes containing rapidly flowing water, as shown in Fig. 14. A very small amount of this water is extracted by a precision pump2 which is drawn through the calorimeter.

FIGURE 12. Picture of flow calorimeter. When the Dewar is raised, the stirrer is slipped under it, the top is sealed with an insulating lid, and the surrounding box is closed. As a result, the cell is thermally isolated and the whole apparatus is contained in an environment having a stability of ±0.01°C.

FIGURE 13. Drawing of the flow calorimeter.

FIGURE 14. Diagram showing water flow. By using the valves “A” and “B”, water can be pumped by the bath directly through the cooling jacket to rapidly achieve a constant temperature or flow at a fixed rate determined by the constant rate pump shown near the top of the drawing. The rate is measured by collecting all water passing through the cell in the container located on a balance. This container periodically siphons back into the constant-temperature reservoir. The cell design shown here is different from the one shown in Figs 12 and 13.

Temperature is measured as water enters the jacket and again when it leaves using calibrated thermistors. With valve “B” closed and valve “A” open, full flow water is provide through the jacket to allow the calorimeter to be used in the isoperibolic mode. Flow calorimetry is done when valve “B” is open and valve “A” is closed. Water flow is measured after leaving the constant- flow pump by filling a container located on a balance. Weight is measured periodically to determine the flow rate. The largest error in this method results from variations in flow rate. The calorimeter is calibrated by supplying power either to an internal resistor or performing electrolysis of a “dead” electrode. Figure 11 shows typical calibration values.

(19***) IS IT NOT JUST ONE CALIBRATION VALUE (THE SLOPE OF THE CURVE)?

Experience has shown that the points generally differ from the least squares line by about ±50 mW, generally because of variations in flow rate. In addition, variations in room temperature can change the amount of heat lost through the lid. For this reason, maximum accuracy is achieved when the calorimeter is contained in a constant-temperature environment, as was used in this design.

II.3
Seebeck Calorimetry:

Seebeck calorimetry uses thermoelectric converters within the calorimeter wall to sense the average temperature difference across the wall. The voltage generated by this difference is proportional to the amount of heat passing through the wall, provided the thermal conductivity of the wall is uniform and constant. The major error associated with this method occurs when the walls are not uniform with respect to thermal conductivity or to generated voltage for the same ΔT. If this is the case, the calibration constant will be sensitive to where the cell is located within the enclosure and to random factors that affect convection of enclosed air. Placing a fan within the Seebeck enclosure to force circulation of air can reduce this problem.

An additional problem is introduced if the outside of the wall is not uniformly cooled so that outside temperature is not independent of the amount of heat flowing through each part of the wall. This defect will effectively cause the wall to be nonuniform in its response to heat flow and be sensitive to external temperatures.

A commercial Seebeck calorimeter is available, shown in Fig. 15, which has been used by several laboratories with success. However, this device requires internal fans and a constant temperature environment to realize its highest accuracy. The uncertainty is ±10 mW when the calorimeter is immediately calibrated using a resistor within the electrolytic cell. If the cell is placed as close as possible to the position used for previous calibrations and an average calibration equation is used, the uncertainty increases to ±20 mW. In other words, even slight variations in the cell location will affect the calibration constant, albeit by a small amount.

A homemade calorimeter is shown in Fig. 16. This device uses PVC as a thermal barrier in which 1000 Fe-Constantan thermal couples are inserted and connected in series. The outside of the thermal barrier is in contact with flowing, constant-temperature water. This design is as accurate as the commercial device and is completely immune to changes in room temperature. Similar designs have been used with up to 50 watts of applied power.

Another variation based the on use of thermoelectric panels is shown in Figs. 17 and 18. This calorimeter is three times as sensitive compared to either the commercial device or the homemade example. Figure 19 shows a typical calibration with and without the use of an internal fan. The data show a scatter of less than ±10 mW.

FIGURE 15. Picture of a Thermonetics Seebeck calorimeter3. The active enclosure contains three fans and is closed by the green lid sitting off to the back. After the Seebeck is closed, the surrounding box is closed by another lid, which allows the environment to be maintained at the same temperature (20°±0.02°) as the cooling water provided to the Seebeck enclosure.

FIGURE 16. Photo of a homemade Seebeck calorimeter based on use of thermocouples. A total of 1000 Type J wire thermocouples were placed in the thermal wall made of PVC tubing. The wires were butt welded on the inside and lead soldered on the outside. The outside surface was waterproofed so that constant temperature water could flow in the surrounding jacket. A muffin fan circulates air within the calorimeter when the lid is closed.

FIGURE 17. Photo of a homemade Seebeck calorimeter using 16 commercial thermoelectric panels.

FIGURE 18. Open view of the Seebeck calorimeter made from thermoelectric panels. The top of the electrolytic cell is visible. Next to it is a small fan. Because the chamber is completely surrounded by flowing water, the device is completely inert to changes in room temperature. The active surface of a panel can be seen within the enclosure.

Although the Seebeck calorimeter is insensitive to many errors suffered by the other designs, one problem shown by the

Seebeck also applies to other designs. Because power is normally applied to a cell either as constant current or constant voltage, if the cell resistance changes, the amount of applied power will also change. If this change occurs sufficiently rapidly, with a time short compared to the thermal time constant of the device, an indication of [AN APPARENT] excess power may be seen. This effect occurs because applied power is measured immediately, while the loss of power from the cell takes time to be detected. When applied power, determined by the product of cell voltage and current, is subtracted from power loss, determined by the Seebeck voltage, the apparent difference will not be correct. Figure 20 shows what appears to be [BUT IS NOT] excess power caused by the applied power change shown in Fig. 21. The corresponding negative power associated with a return of applied power to its initial value might be missed if applied power changed at a sufficiently slow rate.

FIGURE 19. Typical calibration for the Seebeck shown in the previous pictures. The curves are not linear because voltage output is not linear with temperature. Use of a fan results in a greater voltage because all panels are not alike in their behavior.

As a result, only the positive component of the change would be seen and attributed to an exothermic reaction. Rapid changes in cooling water temperature can produce similar false indications of excess power. Because these and the other sources of error, false data can occur without warning. A person reporting such data must demonstrate awareness that such processes might be operating at the time of the study.

(21***) TO “DEMONSTRATE AWARENESS” IS NOT ENOUGH. THAT PARSON MUST DEMONSTRATE THAT EXCESS HEAT IS REAL AND NOT ARTIFICIAL.

(22***) ACCORDING TO FIG 19 THE SIGNAL IS IS ONLY ONE MILLIVOLT WHEN THE EXCESS POWER IN ~0.1W (AS IN FIG 20). HOW IS SUCH SIGNAL MEASURED?

FIGURE 20. Apparent excess power created by an artifact.

II.4
Ways Calorimetric Data are used to Calculate Excess Heat Production:

It is important to understand that calorimetry is not an absolute measurement. All values are relative to the behavior when a known amount of power is applied to the device. Consequently, the only requirement for accuracy is for the behavior to remain constant between calibration and during the actual measurement. In other words, energy loss to the environment is not important as long as it is constant, and the value is known based on a suitable calibration.

Calorimetric behavior can be completely analyzed by applying very complex equations[3,6], as a number of authors have done. Such detailed analysis is useful to understand some of the variables expected to affect the results. However, most of the variables revealed by such analysis can be ignored when a suitable design is used. In other words, rather than attempt to correct for deficiencies using correction factors based on mathematical formulation, a better method is to design a device that does not exhibit these deficiencies. As an example of a correction for a deficiency, corrections can be made for variations caused by changes in room temperature by calculating a loss rate through that part of the apparatus that is not at constant temperature, generally the lid through which the wires pass. This method is not as satisfactory as placing the entire calorimeter in a constant temperature environment, including the wires. Such an arrangement insures that the heat loss rate is constant, hence has no effect on the results.

As another example, the generated gas can be allowed to leave the cell and corrections made as described later in the paper. This deficiency and potential error can be easily eliminated by placing a catalyst within the cell to cause all gas to form D_2O before leaving the cell. Worries that such materials would introduce harmful impurities into the cell have been groundless. This approach also eliminates changes in behavior caused by changes in liquid level. When all such deficiencies have been corrected in the design, excess power can be calculated using the simple equations shown below. Although a calorimeter is expected to show linear behavior, the various ignored variables result in nonlinear behavior that can be fit very well by a quadratic equation. Although the resulting constants in these equations have arbitrary

meaning, accurate results require only that the constants not change between conditions existing during calibration and during the search for excess energy, a condition that is easy to evaluate and control.

1. SWIC and DWIC: Excess power = $A + B*\Delta T + C*\Delta T^2 - V * I$, where A, B, and C are constants obtained from the calibration process, ΔT is the temperature drop across the thermal barrier, V (volt) is the voltage applied to the cell at the thermal boundary of the calorimeter, and I (ampere) is the current passing through the cell.

2. Flow-Type: Excess power = $A + B*F*\Delta T + C*(F*\Delta T)^2 - V * I$ where A, B, and C are constants obtained from the calibration process, ΔT is the temperature increase of the flowing fluid, F (g/min) is the flow rate, V (volt) is the voltage applied to the cell at the thermal boundary of the calorimeter, and I (ampere) is the current passing through the cell.

3. Seebeck: Excess power = $A + B*S + C*S^2 - V * I$ where A, B, and C are constants obtained from the calibration process, S (volt) is the voltage generated by the thermoelectric converters, V (volt) is the voltage applied to the cell at the thermal boundary of the calorimeter, and I (ampere) is the current passing through the cell. If a fan is used, power supplied to it also needs to be subtracted.

(23***) IS “THERMAL BOUNDARY” ALWAYS A SOLID WALL? PROBABLY YES.

In all cases, excess power is detected when more heat leaves the calorimeter than is applied to the electrolytic cell.

(24***) I WOULD SAY “EXCESS POWER IS REAL WHEN THE RATE OF LOOSING HEAT IS LARGER THAN THE RATE OF RECEIVING ELECTRIC ENERGY.” THAT IS WHAT THE AUTHOR HAD IN MIND, OBVIOUSLY. THE TYPES OF CALORIMETERS DESCRIBED MEASURE RATES OF LOOSING HEAT, NOT THE AMOUNTS OF LOST HEAT.

(25***) WHAT IS PLOTTED ALONG THE Y AXIS IN FIGURE 20 ABOVE? ARE THESE INSTANTANEOUS VALUES (CALCULATED AS ABOVE) OR AVERAGE VALUES OF THE EXCESS HEAT FOR SHORT TIME INTERVALS? IS IT CORRECT TO ASSUME THAT EACH POINT IS AN AVERAGE FROM TEN OR MORE INSTANTANEOUS VALUES OF EXCESS POWER?

These equations assume that DC power is being applied to the cell. The problem becomes more complex when AC power is applied. Because bubble action rapidly changes the resistance of the electrolyte, a small AC component is introduced into the DC output of the power supply, which does not occur when a calibration resistor is used.

(26***) AHA, THAT WHAT THE “BUBBLE EFFECT” IS? HOW COULD I GUESS THIS WHILE READING THE CAPTION OF FIGURE 1? (TO AVOID SUCH CONCEPTUAL PITFALLS I WOULD ELIMINATE THE REFERENCE TO THE BUBBLING EFFECT FROM THE CAPTION OF FIGURE 1; THERE IS NO NEED OF IT. (YES, THIS IS PEDAGOGY, NOT PHYSICS. KEEP IN MIND,HOWEVER, THAT I AM A TEACHER.)

Experience shows that modern power supplies generate no more than 50 mV of AC superimposed on 15 V and 3 A of DC, which introduces a trivial error when a resistor is used for calibration. If calibration is done using a dead cathode, which generates a similar AC component, this potential error cancels.

II.5
Effect Calorimeter type on the Observed Behavior:

The choice of calorimeter used for such studies determines the temperature experienced by the LENR environment, the time required to achieve steady-state, and the smallest amount of anomalous energy that can be believed. Additional considerations are cost and ease of use.

The amount of heat produced by a LENR reaction is sensitive to temperature at the spot where the reaction occurs. In an electrolytic cell, this location is the surface of the cathode. Each of the calorimeter types removes heat from the cell with different efficiency. Therefore, each type will show a different relationship between applied electrolytic power, resulting

cathode temperature, and the observed excess energy. Because the LENR reaction is sensitive to temperature, the observed behavior will be different, depending on the kind of calorimeter used.

A calorimeter should respond quickly to changes in power so that bursts of anomalous power can be detected, in addition to requiring less time before data can be taken after changes are made. Generally, the smaller the cell, the more quickly the calorimeter will respond.

FIGURE 21. Example of a sudden change in applied power that produced an excess power artifact.

II.6
Consequences of Using Thermodynamic Open Cells:

In order to measure anomalous energy correctly, all energy produced in the calorimeter must leave the device only as heat. When the LENR effect is produced using electrolysis, D_2 (H_2) and O_2 gases are generated. Unless these are recombined back to D_2O (H_2O) within the calorimeter, they will carry chemical energy out of the device. Only if all generated gas is known to leave the cell can the amount of lost energy be calculated and added to the measured value. Otherwise, any unexpected recombination will be interpreted as excess power. Therefore, evaluating the error this effect introduces rests on knowing exactly how much, if any, gas leaves the cell. This problem is eliminated when a catalyst is placed in the cell. Such a design is called a thermodynamically closed system, which is frequently sealed from the atmosphere as well. The problem can also be solved by measuring the exact amount of D2O that is lost during a known period of time at a know applied current.

The energy lost by H_2 and O_2 leaving the cell is calculated as follows. When electrolysis occurs in a cell containing water, H_2 is produced at the cathode and O_2 is produced at the anode. The rate at which these gases are produced is determined by applied current (A) and is calculated using the following equation:

Rate (moles of H_2O decomposed/sec) = A / (2* 96489).

In other words, a current flow of 53.605 Ampere-hours or 53.605 Coulombs will decompose 1 mole of H_2O to give one mole of H_2 and 1/2 mole of O_2.

(27***) THE QUANTITY EXPRESSED IN A*HRS SHOULD NOT BE CALLED “CURRENT FLOW” IT SHOULD BE CALLED ELECTRIC CHARGE. THE SYMBOL I (INSTEAD OF A) I WOULD BE MORE APPROPRIATE FOR THE CURRENT. (YES, THIS IS PEDAGOGY, NOT PHYSICS. KEEP IN MIND,HOWEVER, THAT I AM A TEACHER.)

The energy required to generate these gases is equal to the enthalpy of formation of water (ΔH=68,300 kcal/mole) times the number of moles of H_2O consumed.

Consequently, the energy (E) used to create these gases can be calculated using the following equation:

$E = \Delta H * A * t / (96489 * 2)$ ---------(1)

where E = kcal of energy used during the time “t” (sec). The amount of lost energy (power in watts being lost) can be calculated another way as follows:

$P = E * 4.189 / t$ ----------(2)

Watts = volt * current -------(3)

Substitution of Equation 1 into Equation 2 and the result into Equation 3 gives, upon rearrangement, the following equation expressed in volts.

Volt = (68300 * 4.189)/(2*96489) = 1.48 volts

When current applied to the cell is multiplied by this voltage, power being used to decompose water can be calculated for standard state conditions (STP). In other words, Equation 1 has been rearranged to allow the calculation to be made by a single multiplication. The voltage has no meaning except as a constant that results from combining Equations 1, 2 and 3. If the conditions do not correspond to STP, small corrections will be required. A similar calculation can be made for D_2O to give a value of 1.54 V.

(28***) GREAT; I HAVE OFTEN SEEN THESE NUMBERS BUT HAD NO IDEA HOW CAN THEY BE JUSTIFIED. THANKS, ED.

For convenience, this value is called the "neutral potential" for water decomposition because it is the theoretical voltage required to produce a current flow through a cell that results in decomposition of water. Real cells will require different voltages to initiate current flow, depending on conditions existing in the cell. Such measured voltages cannot be used to calculate the amount of energy lost from the cell unless the exact nature of the chemical reactions is known.

(29***) WHY IS IT SO; MY IMPRESSION WAS THAT WHAT IS MEASURED IS MORE RELIABLE THAN WHAT IS THEORETICALLY EXPECTED.

Unless the cell contains a catalyst known to completely convert all gas to liquid water, the amount of gas that leaves a cell can be variable and related to applied current, as shown in Fig. 19 [FIG 22]. Small areas of metal exposed to the gas can cause some recombination in excess of values shown here. Nevertheless, application of current above about 100 mA reduces the amount of recombination within the cell to insignificant values.

(30***) WHY IS THE "SPONTANEOUS" RECOMBINATION REDUCED AT LARGE CURRENTS?

FIGURE 19. Relationship between applied current and amount of recombination within a cell in the absence of a catalyst.

(31***) IT IS REALLY FIGURE 22 (NOT 19)

In my work, the cells communicated to an oil reservoir, as shown in Fig. 20 [Fig 22]. This arrangement allows the D/Pd ratio of the palladium cathode to be determined because accumulation of orphaned oxygen, produced when D2 reacts with the Pd cathode, transfers mineral oil to the balance.

(32***) THIS IS NOT EXPLAINED CLEARLY ENOUGH FOR ME.

Because this arrangement gives about 4200g of oil for each mole of D retained by the Pd, it provides a very sensitive method to determine the D/Pd ratio. (This number depends on atmospheric pressure. The quoted value is for average pressure at 8300 feet, the elevation of my laboratory.)

(33***) IS g EQUAL TO 9.8 m/s^2? I WOULD PREFER TO SEE METERS FOR THE ELEVATION, NOT FEET.

Failure of the recombiner is immediately revealed by a steady increase in displaced oil even after the Pd has reached its maximum D/Pd ratio. The data (Storms) shown in Figure 19 [Fig 21] was obtained by removing the catalyst and measuring the amount of generated gas using this arrangement.

FIGURE 20. Drawing of the oil storage and weighing system. Any gas generated in the cell displaces oil onto the balance. Suitable calibration allows this method to measure the D/Pd ratio of the cathode to ±0.001, to determine the amount of recombination in the absence of a catalyst Fig. 19 [Fig 21], and to demonstrate that the catalyst is working properly.

(34***) IT IS REALLY FIGURE 23 (NOT 20)

II.7
Consequence of Prosaic Processes Occurring Within the Calorimeter:

A number of prosaic processes have been suggested that have been previously evaluated by Storms[16, 17]. If real, these processes, as well as others that have not yet been suggested, have the potential effect of producing either extra energy that is interpreted as energy resulting from the proposed cold fusion processes, or using energy, which is frequently interpreted as an obvious error.

(35***) WHAT IS "USING ENERGY" IN THIS CONTEXT? IT IS USE OF ENERGY STORED IN THE SYSTEM, AS EXPLAINED IN THE NEXT SENTENCE. I DO NOT KNOW WHY THE TERM "PROSAIC PROCESS" WAS INTRODUCED. THE TERM "STORAGE PROCESS" IS BETTER, I THINK. (YES, THIS IS PEDAGOGY, NOT PHYSICS. KEEP IN MIND,HOWEVER, THAT I AM A TEACHER.)

A problem is created if the prosaic process absorbs energy for a while, hence is ignored, and then releases this energy, which is then interpreted as evidence for an anomalous effect. However, for such a process to operate, a chemical or physical mechanism must be present that can store and then re-lease large amounts of energy. No one has yet identified such a mechanism when only Pyrex, D_2O, Pt, and Pd are present, the main constituents of a Pons-Fleischmann cell.

(36***) IN FIGURE 2 I ALSO SEE TELON AND RUBBER (O-RING). WHY ARE THESE SUBSTANCES NOT MENTIONED?

Other methods, such as gas loading or ion bombardment, found to produce anomalous heat using different materials, would each have to be consistent with the proposed prosaic processes for the mechanism to be a plausible explanation. In addition, Rejection of the results based on prosaic processes requires that many different prosaic processes are operating, all giving the same result, or the same prosaic process is operating under many different conditions. So far, none of the suggested prosaic processes meet these conditions.

III. SUMMARY

Calorimetry applied to the LENR phenomenon has evolved since 1989 and is now close to the state of the art for measurement of energy production. The Seebeck method provides the most stable and most accurate method, as suggested previously by Bush[18]. Such calorimeters can now be made easily with very little cost or purchased from commercial sources. Nevertheless, even the simple single wall isoperibolic (SWIC) method can be made useful if factors that affect behavior are well understood and are taken into account when the results are analyzed. Unfortunately, reasons for early rejection of such data were based on a very limited and generally incorrect understanding of calorimeter behavior.

(37***) MANY PEOPLE ARE NOT AWARE OF THIS IMPORTANT FACT. THEY THINK THAT CALORIMETRY WAS ALREADY CLOSE TO "THE STATE OF ART" ONE HUNDRED YEARS AGO. (TINY EXCESS HEAT FROM RADIUM WAS DISCOVERED BY P. CURIE IN 2003). NUANCES DESCRIBED BY STORMS BECOME IMPORTANT IN A CONTEXT IN WHICH EXCESS POWER, EP = (B - A), IS A SMALL NUMBER (0.1 to 1 W) IN COMPARISON WITH NEARLY EQUAL (AND MUCH LARGER) NUMBERS, B AND A. NOTE THAT B REFERS TO THE THERMAL OUTPUT POWER WHILE A REFERS TO THE ELECTRIC INPUT POWER.

This study has resulted in the following issues and conclusions:

1. The SWIC method cannot be calibrated using an internal resistor to apply Joule heat. Accurate calibration can only be obtained using a dead cathode and passing current through the electrolyte. Stirring is beneficial.

2. Flow-type and Seebeck calorimeters can be calibrated using an internal resistor within the cell because the results are

independent of where heat is produced within the electrolytic cell.

3. The major error in flow calorimeter involves achieving a uniform and known flow rate.
4. The major error in Seebeck calorimeter involves maintaining a uniform temperature at the outside of the thermal barrier and good circulation of air within the enclosure. Air-cooling of the outside cannot be relied on to give stable results.

5. A constant temperature environment, which is able to effectively carry the heat away, must completely enclose all calorimeters. Use of air-cooling as the major method to remove heat is not satisfactory and will not give stable results.

6. In general, the simpler the method and the less complex the data analysis, the more easily the results can be believed.

Numerous laboratories, using three different types of calorimeters having a variety of designs and characteristics, have studied the LENR effect and report the same anomalous behavior. This kind of reproducibility is much more significant than the ability of a particular researcher to make the effect work on demand, as many skeptics demand. Indeed, as conditions needed to create anomalous effects have become better understood, researchers who use this knowledge now produce the effect with good success.

(38***) THE DIFFICULTIES WOULD BY REDUCED IF THE EXCESS POWER WERE A LARGER FRACTION OF THE ELECTRIC INPUT POWER (FOR EXAMPLE 10%). THE CALORIMETRIC PROBLEMS DESCRIBED IN THIS ARTICLE HAVE NOTHING TO DO WITH MECHANISMS THROUGH WHICH THE EXCESS POWER IS GENERATED IN AN ELECTROLYTIC CELL. THE MECHANISM CAN BE COLD FUSION OR SOMETHING ELSE.

Acknowledgements
Many people have contributed to my studies of calorimetry. In the past, Charles Entenmann, Jed Rothwell, Dave Nagel, and Gene Mallove provided financial support at various times without restrictions. Their selfless support of cold fusion has made possi-ble much of the understanding described in this paper. Since 2003, Lattice Energy, LLC has exclusively supported my work in LENR. My wife Carol has been most patient and provided food and drink when I was too engrossed to get it for myself.

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156) Two papers of Lipson et al.

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According to Karabut (1), who used a Pd target in a glow discharge chamber, the rate of emission of energetic particles (alphas of 14 MeV) was about 1 per second. The glow discharge parameters were about 1000 volts and 450 mA. Lipson, however (2), using a Ti target in a similar apparatus (at the 2175 volts and 250 mA), reports the rate of 0.12 (plus or minus 0.02) per second. Does this big difference result from the use of different discharge parameters or from differences between the two cathodes? My guess is that Pd is a much better target than Ti, in terms of the yield. Why was this not discussed by Lipson? Would it be reasonable to work with Pd to get better statistics?

In another paper Lipson et al. (3) focus on the effect of the glow discharge voltage on the rate of cold fusion from Ti. Their figure 6, for example, shows that the yield of 3 MeV protons, increases from about 50 units at 805 V to 250 units at 2175 V. The units are tracks per cm^2, at the same geometry and same current. The absolute yields, according to Figure 5, range from 1300 and 1550 protons per second. This seems to indicate that protons are dominant particles; the rate of emission of more energetic charged particles, according to 2, is only 0.01 per second, as indicated above. This can be compared to the protons/alphas ratio in experiments based on electrolysis. According to (2) that ratio is 6:1. Why is the protons/alphas ratio in the glow discharge (equal 1450/0.012) much larger than in the electrolysis? Why was this question not addressed by Lipson et al.?

The authors of quoted papers represent prestigeous research centers: (a) University of Illinois at Urbana-Champaign, Department of Nuclear, Plasma and Radiological Engineering, Urbana, IL, (b) Lebedev Physics Institute, of the Russian Academy of Sciences, c) Institute of Physical Chemistry, of the Russian Academy of Sciences, and (d) Lutch in Podolsk, Moskow Region..

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1) A.B. Karabut, “Generation of heat, long-living atomic levels in ...”; see item #13 on my web site. This paper can be downloaded from the www.lenr-canr.org web site.
2) A. G. Lipson, A.S. Roussetski, G.H. Miley and E.I. Saunin, “Phenomenon of an Energetic Charged Particle Emission From Hydrogen/Deuterium Loaded Metals,” as presented at the 10th International Conference on Cold Fusion, Cambridge, MA, 2003. This paper can be downloaded from the www.lenr-canr.org web site.
3) A.G. Lipson, A.S. Roussetski, A.B. Karabut and G.H. Miley, “Strong Enhancement of DD-reaction Accompanied by X-ray Generation in a Pulsed Low Voltage High-Current Deuterium Glow Discharge with a Ti-Cathode.” as presented at the 10th International Conference on Cold Fusion, Cambridge, MA, 2003. This paper can also be downloaded from the www.lenr-canr.org web site.

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157) Comments and questions

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In unit #150 I posted comments and questions related to a paper on calorimetry. I was prompted by a message from Richard Eskimos. In this unit I decided to do the same about a paper on unexpected charged particles. In planning to start a research project in that area, using CR-39 detectors, I selected a paper published by Andrei Lipson, and his coworkers. They are experts in using track detectors of charged particles in the area of cold fusion. The title of that paper is **"Phenomenon of an Energetic Charged Particle Emission From Hydrogen/Deuterium Loaded Metals;"** it can be downloadable from the www.lenr-canr.org website.

That paper, like the one chosen to ask questions about excess heat, was presented to experts attending the 10th International Cold Fusion Conference. It is not surprising that somebody who has not read earlier papers has difficulties with understanding numerous detail. As in unit #150 I want to ask questions illustrating difficulties encountered by non-experts in reading conference papers.

What follows is the text part of the article with questions and comments (in red). The four authors of this paper are A.G. Lipson, A.S. Roussetski, G.H. Miley and E.I. Aaunin. They represent prestigeous research centers: (a) University of Illinois at Urbana-Champaign, Department of Nuclear, Plasma and Radiological Engineering, Urbana, IL, (b) Lebedev Physics Institute, of the Russian Academy of Sciences and, c) Institute of Physical Chemistry, of the Russian Academy of Sciences. The text of the paper (without figures) is in black while my comments and questions are in red. Will the answers to my questions be provided by some readers? I hope so. It will not be difficult to incorporate such input into this text by using a different color.

Abstract
The new phenomenon of energetic alpha (up to 16.0 MeV) and proton (~1.7 MeV) emissions has been discovered from a metal surface possessing a large affinity for hydrogen and loaded/excited by electrolysis, glow discharge or powerful laser. These various experiments on charged particle detection show a remarkable feature, namely all exhibit a similar specific energy yield of long-range alphas (1 alpha particle per 10-15 eV input energy/Pd(Ti) target atom) independent of the excitation power of delivering method (electrolysis, glow discharge or laser irradiation). This result suggests the mechanism of energy transfer causing the energetic particle emissions in hydrogen loaded metal targets is similar despite the seemingly dissimilar excitation techniques.

I. Introduction
Charged particle emissions from the surface of Pd and Ti deuterides have been studied since the beginning of experiments on Low Energy Nuclear Reactions (LENR) product detection in metal deuterides. First experiments on this charged particle detection were mainly, referenced to confirm generation of DD-reaction products (3.0 MeV protons and 1.0 MeV tritons) in metal deuterium system. However, alongside with charged DD-reaction products, there were appeared reports concerning detection of more energetic particles than would be expected from DD-reaction [1-4], especially during hundred-keV accelerator deuteron

bombardment of Ti and Pd targets.

Earlier we just reported that during exothermic deuterium or hydrogen desorption we found high energy alpha-particle emission in the energy range 8-14 MeV, that could not be ascribed to known natural alpha -- emitters. Indeed, the typical charged particle spectrum taken in vacuum using SSB -- detector during about 8 days shows no counts beyond a 8.0 MeV energy (Fig. 1 [5]).

(1***) The SSB probably stands for the “Solid State Barrier,” a common Si detector.

Figure 1.

That is not surprise because maximal alpha energy of radon series is about 7.8 MeV, while energy of cosmic induced protons rarely exceeds 2.0 MeV. In contrast to Background, the dE-E spectra detected from Au/Pd/PdO:D(H)x samples during exothermic deuterium desorption clear showed presence of high energy alpha-component (Fig.2 [6]).

(2***) What are the horizontal segments in the second bin (0.5 to 1 MeV)? I am referring to Figure 1.

Figure 2

New insight was recently obtained from the use of CR-39 track detectors to the study energetic particle emissions from the surface of Pd/Ti loaded with hydrogen/deuterium [7]. Experimental runs with CR-39 to detect long-range alpha-particles *in-situ* during electrolysis of Pd/dielectric substrate cathodes showed energetic alphas ($9 < E_a < 16$ MeV) yield $N_a \sim (2\text{-}5)*10^{-4}$ s^{-1} -cm^{-2} Pd in 4π-str accompanied by even more intensive emission of ~ 1.7 MeV protons.

The objective of present research was focused to study energetic charged particle emission (ECPE) at various power loading and/or excitation conditions applied to the metals with large affinity to hydrogen/deuterium and determine a specific yield of ECPE, depending on excitation power applied to the sample.

II. Experimental Technique

For charged particle detection the purified “Radtrack” CR-39 plastic track detectors (with the size 2.0*1.0 cm^2) by Landauer Inc. AND Fukuvi Chemical have been used. Especial purification procedure utilized for detector manufacturing as well as hermetic saving condition allow to minimize initial alpha track density of these CR-39 to less than 20 cm^{-2}.

(3***) In other words less than 40 tracks per detector. A useful number to know.

Figure 3a and Figure 3b

(4***) Good to know that Radtrack and Fukuvi detectors are not different. Is it true that Fukuvi detectors are no longer commercially available?

These detectors were calibrated with alpha-sources (in the range of 1.6 – 7.7 MeV) and by monoenergetic cyclotron alpha-beams (in the energy range of 10.0 – 30.0 MeV) as well as by proton beams with energy ranging of 2.0-3.0 MeV. (**Fig.3a,b**). For energetic proton detection the detectors were also calibrated with Van DeGraaf accelerator by monoenergetic proton beams (energy ranging from $0.75 \le E_p \le 3.0$ MeV), (Fig.2). After the beam exposure detectors were etched in 6N-NaOH at t=70°C during 7 hrs. and investigated with optic microscope. Typical view of alpha-track picture is presented in Fig 4. As seen, at normal interaction

of monoenergetic cyclotron beam with detector, the tracks observed after the etching have almost ideal circle-like shape. These nuclear tracks can be easy distinguished from the defects of CR-39 subsurface structure.

Figure 4

The efficiency of CR-39 detection with respect to different energy alphas and protons were estimated in accordance with their critical angles q_c being determined by formula [4]: q_c= sin-1{[1-(dE/2h)2]/[1+(dE/2h)2]}, where dE-is the track diameter produced by charged particle with energy E (Fig 1,2), and h = 9.1μm is the depth of etched layer in CR-39 at our etching condition. Knowledge of the critical angles calculated from above formula allow to determine the efficiency e of the charged particle detection as:

e = 0.5 * (1-sin q_c)

(5***) This is wrong; replace sin by cos. Efficiency is defined as the ratio of the solid angle over 4*p. I suppose they reprint the same mistake from the 2002 paper (ICCF9 in China). Am I the first one who noticed it? I do not think that the reference 1 has anything to do with the efficiency of CR-39 detectors.

In electrolysis experiments the freshly opened CR-39 detector chips were attached either to the Pd thin film cathode(Foreground)

(6***) It is not clear to me how to apply CR-39 to a cathode during the electrolysis. They used a very thin cathode. Somehow (according to their 2002 paper at ICCF9 in China) they placed the Cr-39 detectors (open and shielded) inside the electrolyte, next to the thin cathode. This if not at all obvious from reading this paper. Why there is no illustration? Why there is no reference to their earlier paper at this place?

or to the substrate side or/and immersed in electrolyte in the cell (Background).

(7***) This formulation (with two “or”) is confusing. I would measure the background by placing the CR-39 detectors next to an identical cathode that has not been loaded with hydrogen, for example, at zero current. I will assume they did this.

Background experiments showed proportional growth of track density vs. time for CR-39 immersed in electrolyte (Fig.4). At large Background duration [long exposure] it is possible to observe two separate alpha-peaks with track diameters located at 8.0 and 9.0 μm, respectively (Fig.5). The energy positions of these peaks are in good agreement with conventional alpha-Background and are normally corresponded to about 7.0 MeV radon (8.0 μm) and 5.0 MeV (9.0 μm) thoron series of natural alpha-nuclides.

(8***) In other words, the electrolyte or cathode (or both) contained traces of emitters of natural alpha particles.

Figure 5

(9***) According to this figure they can distinguish diameters differing by only 0.1 m.

In order to separate high-energy alphas and low-energy protons that could be possibly emitted during electrolysis runs, the thin Cu-foils (25 μm thick) were inserted between the cathode metallic coating and the CR-39 surface. 25 μm Cu coating is completely absorbs all alpha-particles and protons with energies below 9.0 and 2.3 MeV, respectively.

(10***) I suppose that a separate experiment was conducted to show that the foil itself is not alpha radioactive (at a very low level). Why is nothing said about this? Alpha particles from the surface of the foil, interpreted as if they were traversing the foil, could be assigned more energy than they really have.

Background measurements in experiments with Cu-covered CR-39 were performed similarly to that with open detectors. As expected, these background experiments showed significant reduction(~2 times) in the total track density compared to that obtained with open CR-39 detectors.

(11***) Why “as expected?” On what basis could one expect that one half of all tracks recorded with the open detector (without the Cu foil) would be due to low energy alpha particles and protons? I suppose this was based on their earlier data. How else would one predict the percentage of low energy particles (such as 50%)?

(12***) How large were these densities? What about errors of judgment in deciding which tracks to count and which to attribute to surface defects on CR-39? Based on my limited experience, I suspect that the adjective “typical”, used above to describe Figure #4, might be an exaggeration.

In experiments with glow discharge and laser irradiation for particles identification and their energy estimation, we used CR-39 Fukuvi detectors covered with Al or Cu foils with the thickness in the range of 11-66 or 25-50 μm, respectively. The experimental set ups used for deuterium glow discharge Ti-cathode bombardment and for powerful picosecond laser irradiation of Ti and TiDx targets are described elsewhere []. Power densities applied to the loaded/excited Pd or Ti samples (with respect to the total sample volume) during the electrolysis experiment, in glow discharge (GD) bombardment and in Laser irradiation are estimated as (2-5)x10^2, 10^5-10^6 and ~10^{21} W/cm^3, respectively.

(13***) Why per cubic cm and not per square cm?

In series of experiments with GD the detectors covered with 11-33 μm Al foils were placed behind the holes drilled in the Mo anode at the distance of 3.0 cm from the surface of Ti cathode.

In the laser experiment detectors, shielded with 11-66 μm of Al or 25-50 μm of Cu were placed at different angles toward the target (20 cm from the front of the target perpendicular to the beam direction and 4 cm distance behind the target)

Utilization of shielding foils of various thickness alongside with CR-39 calibration data allow identify and roughly reconstruct energy spectra of emitted alpha particles and protons.

III. Experimental Results

a. Electrolysis

The Foreground runs with electrolysis of Pd-thin film cathodes the exposed CR-39 detectors (t~2.0-30 days) showed the appearance of unusual diameter tracks that were not observed in Background detectors exposed in the same electrolytic cell. Indeed, in the track diameters distribution N(d), two significant peaks located at 7.0m and 6.0m observed in the Foreground runs (with electrolysis) with opened CR-39 detectors (Fig 6).

Figure 6

At the same time, almost no counts for tracks with d < 7.5m was found in the corresponding Background runs for detectors exposed in the same electrolytic cells.

(14***) I replaced mm by microns (mm instead of mm was an obvious typing error).

The low diameter tracks appeared to accompany an electrolysis of the thin Pd-film and Pd-black cathodes. The intensity of charged particle emissions and ratio between 6.0 and 7.0 μm peaks during electrolysis are strongly depended upon the cathode history and structure (Table 1). It should be noted that generation of charged particle emissions during the electrolysis of thin Pd cathodes has a good reproducibility (in contrast to the irreproducible emission of DD-products in Pd-D systems).

(15***) According to Karabut, emission of charged particles is highly reproducible when Pd is bombarded with D (glow discharge). Is it a contradiction?

In the Foreground runs with the same cathode being carried out with 25 μm Cu-film shielded CR-39 chips, the 7.0 and 6.0 mm peaks disappeared. But the other maximums ranging from 7.5 to 11.4 mm have appeared that were not found in Cu-shielded Background detectors (Fig 7).

(16***) This discussion of differences between Figures 6 and 7 is not clear to me.

Figure 7

The experiments with Cu-shielded detectors and a knowledge of CR-39 calibration curves (Fig 1,2) allowed to identify the energy and type of particles emitted in the Foreground runs with open CR-39 detectors. Taking into account stopping powers and ranges of 25 μm Cu-film with respect to the alphas and protons with different energies, the initial energies of emitted particles were also calculated. Comparison of pictures obtained with opened and shielded detectors shows that 6 μm peak is completely disappeared while a broad near 7.0 μm peak in Fig. 6 shifted to the larger track diameters and split at least by 3 narrow peaks (Fig 7). Disappearance of 6.0 μm peak in a shielded detector indicates to the low MeV proton nature of this peak. In accordance with our calibration data the estimated proton energy would be within 1.5-1.7 MeV (Fig 8a, b [7]).

Fig 8 a Fig. 8 b

(17***) Figure 8a has a line saying “39 detectors;” it is confusing. The “open Cr-39 detectors” should be in one line. And what is “25 mcmCu/CR” below that line?

(18***) Why are figures 8a and 8b not labeled consistently? Their legends are in different places.

(19***) These two figures show that 20 alpha particle tracks and 120 proton tracks were recorded on 3 cm^2 of Cr-39? Can this 1:6 ratio be interpreted in terms the higher Coulomb barrier penetrability of protons with respect to alpha particles?

(20***) How long did it take to accumulate so many tracks? A person attempting to reproduce the experiment would need to know this. In other words, what was the average emission rate? Information about the electrolytic cell should also be provided.

In contrast to 6.0 μm peak, the 7.0 μm maximum, accordingly to its shift and splitting after crossing the Cu-shield should be ascribed to a broad 11-16 MeV alpha-peak. Indeed, the stopping range of alphas ranging from 11-16 MeV (for open Cr-39) would be consistent with observed narrow bands with energies 11.6, 12.5 and 14-16 MeV, respectively for Cu-covered detectors (Fig 8a). Due to higher resolution of CR-39 alpha-tracks for the lower energy particles (Fig 1) the broad 11-16 MeV alpha band could be observed as the single individual maximums after these particles crossed the Cu-foil. Therefore, we showed that electrochemical loading of Pd thin film cathodes on dielectric substrates unambiguously produce high-energy charged particles: 1.5-1.7 MeV protons and 11-16 MeV alphas.

b. Glow Discharge

Let us consider in details new results on ECPE obtained for more powerful loading process during low energy deuteron bombardment of Ti-cathode. Typical spectra of charged particles emitted in such GD obtained with 11 and 33 μm shielded CR-39 detectors are shown in Fig 9.

Figure 9

(21***) This figure shows ~80 tracks/cm^2 in 7 hrs (50+20+10=80). It also shows that diameter become wider (lowering of energy) when a thicker shield is inserted. But why didn't they show diameters (and numbers of tracks) detected with the unshielded detectors? Does it mean that using unshielded detectors is not possible in the case of their glow discharge experiments?

As seen, the spectrum of charged particles of 11 μm shielded CR-39 contains 3 characteristic peaks with track diameters 5.2, 6.2 and 7.2 μm, respectively. In accordance with Fukuvi Cr-39 calibration those peaks have to be corresponded to 3.0 MeV protons (5.2 μm), 1.4 MeV/2.8 MeV protons /deuterons (6.2 μm) and 13.0 ± 2.0 MeV alphas (7.2 μm). Indeed, increase in Al shielding thickness from 11 to 33 μm leads to corresponding increase in track diameters for all three peaks observed at for 11 μm Al-covered Cr-39 detectors.

(22***) The above text is too condensed, for my taste. They interpreting say the peaks with track diameters 5.2, 6.2 and 7.2 μm. The first is due to protons of 3 MeV, the second is due either to to 1.4 MeV protons or 2.8 MeV deuterons, and the third is due to 13 MeV alphas. The last sentence is confusing, unless the phrase "observed at for 11 mm Al-covered Cr-39 detectors" is removed. They probably say that all red peaks (even single counts) shift to the right (diameters become larger).

The shifts of these peaks (5.2 →5.6 μm; 6.2 → 6.6+6.8 μm (spitting) and 7.2 →7.6 μm) is really corresponded to the energy losses of 3.0 MeV protons (from DD-reaction), 2.8 MeV deuterons and 13.0 MeV alphas, in accordance with the stopping ranges of these energetic particles in Al. In Fig. 10 the more detailed picture of alpha emission is presented and compared to the background in GD chamber.

Figure 10

As seen from the Fig. 10 the energetic alpha emission in GD at given discharge parameters exceeds the background counts in the range of 7.2 μm track diameters about 25 times.

(23***) I can not see this from Figure 10. By the way, why some blue background bars indicate negative negative densities? But I do see that this time they refer to Al shielding (while Figure 9 referred to Cu shielding). I suppose it was the same experiment in which some detectors were covered with Cu and others with Al. It looks that they had about 45 tracks/cm^2 in 7 hrs.

At the same time the usual alpha-background (d ≥ 8.0 μm) in presence of GD is not significantly distinguished from that in absence of glow discharge voltage. The yield of energetic alphas after background subtracting was found to be Nα = 0.12 ± 0.02 a/s in 4π ster.

(24***) But according to Karabut, who used the Pd target (not Ti target) the rate of emission of energetic alphas (at 1000 volts) was about 1 per second. This is nearly ten times more than the above number. Why is this not discussed? I would use Pd to increase the yield?

This yield of energetic alphas is about 2-3 orders of magnitude high than that detected in electrolysis experiment.

(25`***) Aha, here is the answer to my previous question; for the electrolysis the rate was about 0.001 to 0.0001 per second.

The yield of 2.8 MeV deuterons in GD experiment is about 2-3 times larger than for alphas.

(26***) Can this ratio also be interpreted in terms the higher Coulomb barrier penetrability of deuterons with respect to alpha particles? What I have in mind is a barrier lowered by screening.

In Fig. 11 the spectra of charged particles obtained for two different GD voltages are presented.

Figure 11

As it is expected the 3.0 MeV peak from DD-reaction in Ti under deuteron bombardment is strongly depended on GD voltage. Meanwhile, the intensities of 2.8 MeV deuteron and 13 MeV alpha peaks are depended on the GD voltage much weaker.

(27***) On what basis is this expected? Why should the effect of discharge voltage on the probability of emission of DD protons be much more pronounced than on the probabilities of emission of more more energetic particles?

In Fig 12a and 12b the more detail pictures of dependencies shows This fact indicate to absence of direct connection between energetic charged particle emission and DD-reaction. The detailed yield dependencies for 3.0 MeV protons, 2.8 MeV deuterons and 13.0 MeV alphas on discharge voltages in the range of 0.8-2.45 kV are presented in Fig 12 a, b.

Figure 12 a and Figure 12 b.

As seen, the yields of energetic deuterons and alpha particles normalized to the effective discharge power almost independent on deuteron energy Ed, while DD-reaction yield of 3.0 MeV protons tends to grow exponentially with increase in Ed. The experiments showed that the yields of energetic deuterons and alpha particles in Glow discharge are roughly proportional to GD power applied to the Ti cathode (Fig. 13)

Figure 13

(28***) Zeros below the x axis should be replaced by numbers of watts.

The fact of near linear dependence of energetic charged particle emission yields on effective power applied to the Ti cathode leads to a simple assumption that the yield of ECPE from the surface of metals with large affinity to hydrogen could be further increase with increase in specific power applied to these metals.

c. Laser Experiment

(29***) Their laser induced fusion is probably not cold. This is the end of my questions and comments.

In order to check the assumption of ECPE yield increase with applied specific power we have performed a search of energetic alphas in ultra-high specific power applied experiments on picosecond laser excitation of Ti/TiHx and TiDx targets. The details of this experiments, including set up are described in another report published in this Proceedings [8]. We note here that powerful laser irradiation of solid targets (polymers and

some metals including Al , Pb LiD and so on) was studied during last 10 years to induce intense MeV protons and heavy ion emissions as well as to the purposes of inertial DD-fusion and isotope separation (see review article [9]). Here in experiments with powerful laser we, probably, first use targets possessing large affinity to hydrogen/deuterium (Ti and TiDx foils of 30 μm-thick). The parameters of laser were: power density $P=2x10^{18}$ W/cm^2, pulse duration $\tau = 1.5x10^{-12}$ s and wave length $\lambda = 1.054$ μm. To compare our results with usually employed targets (to produce intense proton beams) we also used polyethylene (PE) film targets of 35 μm-thick.
The main charged particle component emitted on laser shots with both Ti(TiDx) and PE targets was found to be protons (deuterons) with energies Ep ≤ 1.0 MeV (Np ~ 10^{11}/pulse). The heavy ion component with mass A > 4 was also detected. However, besides these species during the shots on Ti and TiDx targets we found we found also energetic alpha particles that were not detected in experiments with PE (Fig.) As seen the laser shots on Ti-target produce the same tracks at the CR-39 detectors covered with 11 μm of Al (d=7.2μm) that were detected for of electrolysis and GD loading experiments. The similar shots on PE target showed possible alpha emission level comparable with background in vacuum chamber of target installation.

Figure 14

In Fig. 15 the spectra of alpha particles detected in the same laser shot by CR-39 detectors covered with various shielding and placed between 0-30 ° with respect to TiDx- target are presented.

Figure 15

The main peak in Fig 15 corresponding to 13.0 ± 2.0 MeV alphas is shifted to larger track diameters for 33 and 66 μm Al shielded detectors, well in accordance with 13 MeV alphas stopping range in Al. For 50 μm Cu covered detector this peak is completely disappeared because the 50 μm thick Cu shielding will stop all alpha particles with energy E ≤ 15.0 MeV.
The same energy alpha-particle emission, but about 30 times less in intensity was detected from the opposite side of Ti/TiDx target (the angle between the detector and target • = 180°) (Fig 16.) Here as in Fig 15. a track diameter corresponding to alpha peak (d=9.9 μm) is also in good agreement with stopping range of 13.0 MeV alphas in the sandwich consisting of Cu-shielding and Ti-foil [25μm Cu(shielding) +30 μm Ti(sample thickness)]. This fact allow assume that alphas were originated mainly at the front side of the Ti target. The estimated average intensities of alpha emission from the front and opposite sides of Ti/TiDx targets (taking into account geometrical efficiency of measurement) was found to be If ~ $2x10^4$/pulse-sr-1 sr. and Io ~$7x10^2$/pulse- sr^{-1}, respectively.

Figure 16

In Fig 17 the results of alpha-spectra reconstruction from CR-39 data obtained for various shielding type and thickness (taking into account cyclotron calibration) are presented. The results of such reconstruction for Ti and TiDx targets within experimental error are close one to another. In the Fig. 18 a comparison of alpha spectra obtained from laser experiment with Ti-target and Pd-glass electrolysis are shown. As seen from this figure, the spectra of laser experiment and electrolysis detection look quite similar within the measurement error (determined by standard deviations of alpha-calibrations and errors of track diameter measurements) and both spread in the energy range of 10^{-17} MeV.

Figure 17 and Figure 18

IV Discussions and Conclusions
Thus, in three independent studies of ECPE during hydrogen(deuterium) loading of Pd and Ti targets or excitation of their hydrides/deuterides, the similar energetic alpha particles and protons/deuterons are found

to be emitted, despite the seemingly dissimilar loading/excitation techniques. We found that the absolute intensity of ECPE is roughly proportional to the specific power applied to the metal target during its loading or excitation (Fig 19). There are three different power density areas corresponding to the studied ranges of specific power applied in experiments on ECPE detection with hydrogen/deuterium loading/excitation of Ti and Pd: electrolysis -((2-6)x10^2 W/cm^3); GD -(10^5-10^6 W/cm^3) and laser irradiation - (~ 10^{22} W/cm^3) are presented in this graph. On the other hand, the specific energy required to emit one alpha particle (E > 8.0 MeV) in all these loading /excitation cases was found to be Es = 17 ± 8 eV/at.Ti(Pd) and practically independent on excitation power applied to the target (Fig. 20).

The discovered property of the metals with large affinity to hydrogen to emit energetic charged particles is probably concerned to peculiarities of these metals. On one side, during hydrogen loading the metals such as Pd and Ti are subjected to strong plastic deformation accompanied by massive generation of non-equilibrium phonons. On the other side, the metals with large affinity to hydrogen may capture high amount of helium (from surrounding atmosphere or during the loading alongside with hydrogen), which have tend to segregate in the site of high internal strain. We assume that loading or excitation procedure produce non-equilibrium phonons in the near-surface layer of Pd or Ti. These phonons would be focused (concentrated) in the some specific sites with high internal strain near the surface. If mechanisms of energy transfer from the concentrated optic phonon modes (with high amplitude and frequency) to the atoms captured in the sites with high internal strain is really existed [10], then such energy transfer can lead to effective acceleration of the light atoms captured in the sites of high internal strain (hydrogen, deuterium helium).

Figure 19 and Figure 20

Regardless of mechanisms involved in ECPE phenomenon in Pd and Ti, we emphasize that some peculiarities of LENR could be more clear, taking into account similarities between processes observed in electrolytic loading of these metals (LENR effects) and powerful laser irradiation effects. These similarities include:

-Neutron spectra for deuterated targets in laser and LENR experiments : besides 2.45 MeV peak high energy neutrons up to 10 MeV (compare: P.A. Norreys et al., Plasma Phys. Control. Fusion, **40**, 175(1998) and A.G. Lipson et al., Fusion Tech., **38**, 238 (2000)). High energy component of neutron spectra in laser case is explained by presence of MeV deuterons (Probably similar effect in LENR case).

-Charged particle emissions, including their energy ranges

-Isotope separations and exotic transmutations

-Production of craters at the target surface.

-Possible generation of isomeric states of nuclei in solids (after-emissions of gamma and X-rays)

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158) An interesting article from MIT

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Here is an interesting extract from an article about cold fusion published recently (April 23, 2004) in the MIT magazine "Technology Review:" The title of the article "Is Cold Fusion Heating Up?," and the introduction, indicate that it was prompted by the pending evaluation of cold fusion by DOE.

". . . The evidence for 'new physics' has been building for years, says Peter Hagelstein, associate professor of electrical engineering and computer science at MIT, who chaired the tenth International Conference on Cold Fusion in Cambridge last August. Experiments performed under properly controlled conditions reliably produce more heat than standard theory predicts. Nuclear products show up in about the right amounts to account for this excess heat. Patterns have emerged that explain previous anomalies. When Hagelstein saw how pieces of the puzzle were fitting together at the August meeting, he urged the Department of Energy to reconsider a field that had been cast out of orthodox science soon after its birth.

Over the past 15 years, enthusiasts have generated some 3,000 manuscripts on cold fusion, but very few were ever published in scientific journals. Many results evaporated under outside examination, and promoters pushed 'free energy' schemes that sounded more like perpetual motion than physics. Most of those manuscripts 'are not helpful,' says Hagelstein, a theorist with wide-ranging interests in optics, energy, and nuclear physics. But some 50 do show interesting, reproducible effects. 'The heat effect has been replicated many times,' Hagelstein. It works only when deuterium is loaded into palladium cells, and never when normal hydrogen is used instead of the heavy isotope. Exacting measurements with heat-measurement instruments have answered criticisms of the original experiments. Excess heat has been measured beyond what Hagelstein considers any reasonable doubt. Experiments that produce excess heat also have yielded helium-4, one potential product of the fusion of two deuterium nuclei, in amounts that correlate with the excess heat. Theory predicts that the fusion reaction should generate 24 million electron volts (MeV) of energy per helium-4 nucleus. An analysis by Michael McKubre of SRI International detected energy of 31 MeV— a match within the experimental uncertainty of plus or minus 13 MeV. Skeptics had doubted the reaction was possible, but Hagelstein says McKubre's analysis of the experiments, reported at last year's cold fusion meeting, shows that fusion of two deuterium to yield helium-4 'is not as nutty as it initially seemed.'

McKubre has also found that the seeming inconsistency in experimental heat production arose from differences in the amount of deuterium packed into the palladium electrode. Whenever the number of deuterium atoms loaded into the metal matched or exceeded the number of palladium atoms, excess heat was generated. Palladium loaded with slightly less deuterium produced inconsistent results, and if the deuterium level was reduced by a great amount, then no excess heat at all was produced. Deuterium loading was hard to control and limited by the strength of the metal. Unfortunately, palladium strength is difficult to predict or control, and is not improved by purification; indeed, the purest palladium ruptured at lower loadings, and the highest strength was seen only in one impure batch. ..... "

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159) From another review of cold fusion

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Unlike my review of recent cold fusion claims (see unit #152) the paper presented by George Miley (1) was not prepared by an uncommitted outsider. George is the author of numerous research papers; as the editor of a scientific journal devoted to nuclear fusion he has been a leader of that field from the year in which the controversy started. Let me begin with an attempt to categorize cold fusion research. The first subdivision of the field is obvious: some research topics are nuclear while others are not. Research on emission of unexplained neutrons and protons (via counters of such particles), or research on changes in isotopic ratios, belong to the first category while research on the unexplained excess heat (via calorimetry) belongs to the second. Seminal work of Steven Jones was nuclear, seminal work of Fleischmann and Pons was not.

George Miley offers an interesting further categorization of nuclear topics. He divides them according to reactions that take place. The first subgroup consists of DD reactions while the second consists of so-called "lattice reactions." The DD reactions are:

D + D --> ^{3}H + p
D + D --> ^{3}He + n
D + D --> ^{4}He + g

The first two are named "normal DD fusion" (some people call it "hot fusion") while the last is named "P-F type Cold Fusion." The P-F stands for Pound and Fleischmann who speculated that excess heat might have nuclear origin.

The so-called "Lattice Reactions," on which Miley focuses in the review, are symbolically described as "p/D + metal." That means they are initiated by fusion of ^{1}H ions (or ^{2}H ions) with much heavier atomic nuclei in crystal lattices. These reactions are called transmutations. Collectively P-F Cold Fusion and Transmutations are referred to as LENR (Low Energy Nuclear Reactions).

There are two kinds of transmutations reactions, those in which compound nuclei are formed and those which are called "direct." That characterization of nuclear reactions is not new, it can be traced to late 1930s (Niels Bohr) and to numerous textbooks published in late 1940's. Formation of a compound nucleus is a process of complete fusion of two atomic nuclei followed by either fission (typically into two fragments) or emission of charged particles and neutrons. Direct reactions produce specific products, for example, transformation of ^{88}Sr into ^{96}Mo described by Iwamura (see item 152 on my web site). Compound nucleus reactions, on the other hand, result in a large number of products (see Figure 4 of Miley's review).

I think that this categorization of the cold fusion field can be useful. Let me make some comments.

1) The term "cold fusion" was used by Steven Jones long before Fleischmann and Pons offered a speculation that excess heat might have nuclear origin.

2) Rates at which emission of nuclear particles is observed in Jones' experiments

(typically one per hour or less) are too small to generate measurable excess heat.

3) According to Karabut (see item #13) alpha particles of 14 MeV are emitted when Pd is bombarded by ^{2}H ions of ~1keV in a glow discharge apparatus. Such rates are also too low to generate measurable excess heat.

4) Transmutations, on the other hand (according to Miley and others who observe them), take place at much higher rates, typically above 10^{11} reactions per second per cubic centimeter. This is illustrated in Figure 4 of Milley's review paper. Transmutation reactions have been studied in 14 separate laboratories, worldwide, as listed by Miley.

5) Accumulation of ^{4}He, reported by several excess heat researchers, worldwide, is also occurring at very high rates. Generation of heat at the rate of 1 W, for example, is associated with generation of about $3*10^{12}$ atoms of helium. In other words, production of each atom of helium (often called "an ash resulting from nuclear burning") is associated with the liberation of about 23 MeV of energy. That number is in very good agreement with generation of helium in thermonuclear reactions (sun and atom bomb). Let me end with a puzzling question about generation of helium: "why is it relatively rare in thermonuclear setups (one out of one million reactions in hot plasma) and dominant in some cold fusion setups?"

P.S.
According to Milley, "Excess heat production was also observed simultaneously with transmutations in various experiments..... The product production rate correlates with the excess power measured within the experimental accuracy." This is important because one of the most convincing argument against the reality of excess heat in cold fusion (in the 1989 DOE evaluation of that field) was the lack of evidence for the "commensurate reaction products." The excess energy claim was compared to fire without any byproducts of burning. If the reported accumulation of ^{4}He, and the accumulation of transmutation products, are real (not due to migration of impurities, or errors of measurements) then the 1989 argument is no longer valid. Will the upcoming DOE evaluation of cold fusion address this essential issue?

References:
1) G.H. Miley and P.J. Shrestha, "Review of Transmutation Reactions in Solids." This paper, presented at the 10th International Cold fusion Conference (August 2003), can be downloaded from the www.lenr-canr.org website. The exact URL is http://www.lenr-canr.org/Collections/ICCF10.htm#Proceedings

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161) A set of messages about cold fusion

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This thread on PHYS-L, the Internet discussion list of physics teachers was triggered by this message from Teacher 1 (myself). Other contributors will be identified only as Teacher 2, Teacher 3, etc.

Teacher 1:
An interesting piece from Brian Josephson (Nobel prize in 1973 for the discovery of a superconducting electronic switch) about pseudoscience.

www.lindau-nobel.de/images/ock/media/downloads/Media_1703187544.htm

It is his lecture at the last annual meeting of Nobel winners in Landau, Germany. According to Haiko Lietz, a science reporter who attended the gathering: "the Lindau conference was very interesting, as one can imagine. Brian Josephson created a big stir. He spoke extensively on cold fusion, and how unscientific attitudes and a publication policy prohibited the field from becoming known in the scientific community. He was the talk of the day. Many students were asking him questions, they were open and interested. Reactions by the Nobel laureates were twofold: Some seemed to be surprised that cold fusion is still around, while others remained silent."

The immediate reply from Teacher 2 was:
"Earthshaking shaking claims require earthshaking evidence. Are you saying that such evidence is being suppressed and HAS been for lo these many years ??? This stand wouldn't be giving the 'physics community' much credit. Has EVERYONE missed out on the shaking of the earth? How long was required for 'polywater' to become a lost cause? or does that research continue as well?"

Referring to the above, Teacher 1 wrote:
"1) In my opinion dangers of degeneration of science are very real. And I have no doubt that pseudoscientists exist. But I see no evidence that major cold fusion claims, described in item #152 on my website: are examples of pseudoscience. Some of these claims might become part of accepted science, others might be rejected as invalid (due to errors etc.). That is normal.

2) Let me give you an example of what Brian Josephson calls the "denunciation mode" of operation (by leaders of intellectual activities, such as editors of our journals). The example is based on personal experience (see item #154 on the above website). An objective summary of major cold fusion claims (see item #152) was consecutively submitted to editors of five journals. The manuscript was rejected by all of them, without being sent to referees. Each editor answered differently but the bottom line was the same: your manuscript does not correspond to our current needs. My review, as you can verify, does not defend cold fusion claims; it tries to describe them objectively. Why isn't the review sent to referees? Why couldn't it be published, provided, of course, that its accuracy is not challenged by referees? This is not new; the intellectual establishment has often operated in the denunciation mode. How can critical thinkers be protected from bureaucrats? How do bureaucrats benefit from acts of suppression and oppression of cold fusion?

3) I just became a retired teacher and this allows me to participate in the 11th Cold Fusion Conference in France (October 31-November 5). It is very unlikely that my own research (just started) will produce something worth reporting. I do see something but it needs more attention. Therefore my presentation, during the public relations session,

will be based on what I wrote above. In that context your input, under this thread, will be highly appreciated. Or write to me in private, if you prefer.

4) In the message that started this thread I forgot to mention that I would not know about Josephson's lecture if Steven Krivit did not send me 'New Energy Times Newsletter' (dated July 15, 2004). Krivit, a journalist specializing in science, is the editor of that newsletter. To subscribe write to: newsletter@newenergytimes.com "

Shortly after the above was posted I received a private message from another teacher. It was a set of general observations about intellectual suppression.

Teacher 3 wrote: "Here's something you might find interesting.

COMPLAINTS ABOUT INTELLECTUAL SUPPRESSION ARE NOT "CONSPIRACY THEORIES.

Intellectual suppression in any community's publications certainly exists, since the beneficial purpose is "triage." Triage is there to prevent idiotic troublemakers from wasting our time. For example, if I submit rambling drunken poetry and nude photographs to the journal Nature, they will be discarded. That's intellectual suppression! And when we try to filter e-mail lists so spammers can't fill them with advertising, that's suppression of publication. Nothing wrong with it. And it's very, very common.

Inappropriate suppression also exists in the form of nasty back room politics, when for example the governmental/industrial leaders try to prevent whistle blowers from publicizing embarrassing mistakes and criminal acts. In other words, intellectual suppression often means the same as "coverup." Dr. Brian Martin maintains an entire website on the problems of political intellectual suppression in the sciences, see http://www.uow.edu.au/arts/sts/bmartin/

Intellectual suppression has a long history involving revolutionary science. Researchers who later created entire new fields of science discovered that they could not get their research published for years, often for decades. They eventually succeeded, but only after a major fight with suppression. No journal editors conspired to silence the revolutionary researchers. They simply assumed that their papers were misguided, or were outright crackpotism. Here's a small list:

Ridiculed, vindicated discoverers
http://amasci.com/weird/vindac.html

. . . There's no question that inappropriate intellectual suppression is a real problem, nor that appropriate suppression is essential.

In recent years I've noticed a strange group-think phenomenon among skeptical scientists. In online forums, crackpots start discussing topics like antigravity, perpetual motion, psychic phenomenon, etc., and they complain that science journals won't publish their research. The
crackpot's opponents then sneer, insisting that the crackpots complaints are nothing but a conspiracy theory.

Um. What? Let's get this straight. Someone wants physics journals to publish their papers where they prove Einstein was wrong... the papers are rejected everywhere... and if the crackpots complain about this, it means that the crackpots are CONSPIRACY THEORISTS? But... but... all the physics journals REALLY DO reject those papers. The crackpots really are being suppressed; their publications are being blocked from all legitimate journals (obviously with good reason.) I thought that the skeptics might be joking, but they're not. They really insist that anyone who complains about intellectual suppression is a conspiracy theorist who should be ignored.

Over the years I've found that this strange reasoning is very widespread among the online scientific community. I find it ridiculous, and I feel embarrassed when I try to point out the flaws to those making the argument. (And I feel very

confused when my observations are rejected, and the skeptics making these arguments continue to do so time and again.)

Just to make things perfectly clear once more: intellectual suppression is very real, and is a valid part of the science culture, so when an author complains of suppression, he/she is complaining about something genuine. Journal editors need not "conspire" together before rejecting my nude photographs, or rejecting papers about Bigfoot or UFO abductions or Cold Fusion. Those editors INDIVIDUALLY are disbelievers. That's why they reject the articles out of hand.

Here's something that may shed light on the proceedings. In marriage counseling I found out about a very common human foible: "Invalidation." If someone doesn't want to deal with their spouse's complaints, they can choose to "not hear" those complaints via the process of "invalidation," by declaring the complaints to be ingenuous (perhaps triggered by vengeance or other low motives.) Rather than taking the complaints seriously, the ears are blocked and the complainer is essentially silenced.

When a skeptic declares a crackpot's complaints to be "conspiracy theories", this is a clear example of invalidation: it's a psychological ploy whose most likely purpose is to excuse the skeptic from taking the crackpot's complaints seriously (or from even hearing them at all.) We need not even listen to conspiracy theorists, so declaring a noisy crackpot to be a conspiracy theorist gives us even more reason not to listen.

But when someone complains of suppression, their complaint is almost always genuine. And note well: they never complained about any conspiracy. It was the skeptic, the person supposedly in support of reason and rational argument, who put those words in the crackpot's mouth. Hmmm. Since this phenomenon is so common, perhaps it needs its own name. "Suppression complaints are conspiracy theories" is a bit wordy. Which class of logical fallacy does it fall under?

P.S. Here is a possible answer to the last question; it is pasted from:

http://www.nizkor.org/features/fallacies/straw-man.html

The Straw Man fallacy is committed when a person simply ignores a person's actual position and substitutes a distorted, exaggerated or misrepresented version of that position. This sort of "reasoning" has the following pattern:

1. Person A has position X.
2. Person B presents position Y (which is a distorted version of X).
3. Person B attacks position Y.
4. Therefore X is false/incorrect/flawed.

Teacher 1:
XX sent me a private message about dishonest skeptics (pasted below). I am responding to this message publicly because it deserves to be shared. Let me replace the term "skeptics" by "editors" and not use the term "theorists" to describe crackpots. In the context of this thread I am thinking about editors of journals who reject cold fusion manuscripts without sending them to referees. The bottom line, if I understand XX correctly, is that editors use negative labels to dehumanize crackpots in the eyes of the public . This allows editors to act with impunity. It might appear that editors conspire against crackpots but this is not true, they do this independently of each other because they individually believe that crackpots are not legitimate scientists. Editors, incorrectly, place cold fusion in the same category as astrology and UFOlogy. Is this a reasonable summary?

Decisions of what to accept and what to reject (without any further consideration) must be made by editors; that is part of their difficult job. A good editor, however, should be a critical thinker. Did five editors who rejected my paper think that I am a crackpot? Yes, I submitted a review of recent claims made by crackpots (see item #152 on my cold fusion website:

http://blake.montclair.edu/~kowalskil/cf/).

But I am not defending these claims; I am only describing them objectively.

[In the e-mail piece the message from Teacher 3 was pasted here]

Teacher 4:
Josephson apparently says there is something to telepathy as well. "Nobel laureate, Cambridge physicist Brian Josephson 'Yes, I think telepathy exists,' he told The
Observer, 'and I think quantum physics will help us understand its basic properties.' "

Teacher 1:
Josephson's speculative 1991 paper on telepathy can be read at his website:

http://www.tcm.phy.cam.ac.uk/~bdj10/

Should this paper disqualify him? I know very little about topics discussed by him. Also see:

http://www.tcm.phy.cam.ac.uk/~bdj10/stamps/today.html

A message from a friend:
After posting the above reply I received a private message from a friend who is not a teacher. In the first sentence he wrote: "I am convinced that cold fusion is our best hope for a pollution-free source of energy to sustain this planet."

Here is my reply:
Yes, many people believe in this. Same of them, when asked "what do you know about cold fusion?" reply in the same way. In my opinion, however, this is still "science fiction." The main issue today is to determine whether or not scientific claims made by leading cold fusion researchers are valid. That should be decided by a panel of experts recently appointed (?) by the DOE. In my opinion suggesting practical applications might be counterproductive, at this time. A panel, if asked to evaluate practical uses of unrecognized discoveries, would most likely produce a negative verdict. But a panel of honest experts asked to evaluate validity of major scientific claims, in the area of cold fusion, would probably produce a positive verdict. After all, new data supporting such claims are very strong. Will the DOE declare that cold fusion should no longer be treated as pseudoscience? It will depend on the panel's report. Such declaration will not happen unless the report is positive.

The two most important claims, as explained in item #152 at my website, are:

http://blake.montclair.edu/~kowalskil/cf/

a) Unexplained emission of charged particles and neutrons from metals loaded with deuterium.

b) Generation of excess heat associated with accumulation of helium at the rate of one atom per 23 MeV of heat energy.

I am selecting these two claims because they are supported by research of several groups in different countries. What is needed is a set of clear answers to the following three questions: (a) Were the authors of these claims qualified scientists? (b) Were their methods of validation scientific? (c) Was there any evidence of fraud?

On the basis of answers to these questions the panel might recommend that damaging labels, such "voodoo science, pseudoscince or fiasco of the century" should be declared invalid by the DOE. Elimination of such pejorative labels will most likely turn cold fusion into science able to defend itself against honest scientific criticism. Sooner or later that might lead to some kind of practical applications. It is possible that formally legitimized cold fusion research will eventually give us pollution-free sources of energy. But that is not at all obvious today. The situation might change dramatically after at least one cold fusion experiment becomes 100% reproducible (by competent scientists, of course), and after the underlying phenomena are understood.

Teacher 1:

According to Teacher 2, “Earthshaking claims require earthshaking evidence.” Brian Josephson also commented on this. He thinks that this dictum is too strong. It implies that critics of cold fusion are able reject new claims indefinitely, as explained at:

http://www.tcm.phy.cam.ac.uk/~bdj10/

I agree with Teacher 2, provided the “earth-shaking evidence” does not refer to something unreasonable or impossible. Instead of the “earthshaking evidence” I would say “very strong evidence.” A researcher in the area of cold fusion should consider all possible objections and address them, one after another. Fleischmann and Pons, for example, should have anticipated requests for the evidence that excess heat is nuclear before allowing the university to release the results of findings.

They should also have waited before the reproducibility dilemma were solved or understood. Allowing a press release to go ahead without a peer reviewed publication also weaken their case enormously. Such press release might have been appropriate in a well established area but not in an area known to raise many objections. One kind of experiment might be sufficient in a well established area, several kinds of experiments are necessary when new results are in conflict with what is already in our textbooks. Yes, I know that the situation at the University of Utah was very complex; as usual, things are much clearer in retrospect. In sum, a discovery challenging an accepted paradigm must be announced more carefully than a discovery which does not challenge it.

Responding to the above the friend referred to the straw man man fallacy used by opponents of cold fusion. (The insertion added below the message from Teacher 3 was inspired by his reply.)

The friend also wrote:
“. . . ANY scientist who is honest, open-minded, AND skeptical can look at the data and conclude that the Cold Fusion effect is AS REAL as any other new science is or was in its early stages. The DOE matter is sheer politics and money, but a spin-off from a positive vote will be extra incentive to private industry and hopefully, God-willing, a change in policy at the US Patent Office which is a major block to progress.” . . .

Teacher 1:
I am not sure which block (the editors who reject research papers without sending them to unbiased referees or clerks in our patent office) is more effective at this time.

Teacher 5: (another private message)
PATHOLOGICAL DISBELIEF (from today's UnderNews)

http://www.lindau-nobel.de/images/ock/media/downloads/Media_1703187544.htm

"Pathological Disbelief" was the title of a lecture by 1973 Nobel Prize winner Brian D. Josephson, who teaches physics at the University of Cambridge, delivered at the 2004 Lindau meeting of Nobel Laureates. It describes a problem for science but also one for journalism which has over the past few decades moved from ubiquitous skepticism to ubiquitous condemnation of skepticism, most popularly expressed in labeling the skeptic
a "conspiracy theorist."

Josephson, incidentally, cites the treatment of cold fusion as an example. Some readers may recall that the Review is one of a tiny number of publications that has treated research into cold fusion as newsworthy ? Not because this research will necessarily pan out but because the suppression of this research by both science and journalism violated the objective principles of both trades.]

BRIAN D. JOSEPHSON - This talk mirrors "Pathological Science", a lecture given by Chemistry Laureate Irving Langmuir. Langmuir discussed cases where scientists, on the basis of invalid processes, claimed the validity of phenomena that were unreal. My interest is in the counter-pathology involving cases where phenomena that are almost certainly real are rejected by the scientific community, for reasons that are just as invalid as those of the cases described by Langmuir.

Alfred Wegener's continental drift proposal provides a good example, being simply dismissed by most scientists at the time, despite the overwhelming evidence in its favour. In such situations incredulity, expressed strongly by the disbelievers, frequently takes over: no longer is the question that of the truth or falsity of the claims; instead, the agenda centers on denunciation of the claims. . . In this "denunciation mode", the usual scientific care is absent; pseudo-arguments often take the place of
scientific ones. . .

Other popular forms of attack are "if X were true we would have to start over again" (as we of course had to do with relativity and quantum theory, and so the argument proves nothing), and then there is the dictum "extraordinary claims require extraordinary evidence", which has the marvelous feature of allowing the requirements for acceptable proof to be stretched indefinitely as more and more support for a contested claim comes in. Its originator, the late Marcello Truzzi, later decided that his comment was 'a non sequitur, meaningless and question-begging', and had planned to write a debunking of his own creation.

"Cold fusion" appears to be the modern equivalent to continental drift, starting with the controversial claim, made by Pons and Fleischmann in 1989, to have generated in an electrochemical cell heat considerably in excess of anything explicable in conventional terms. This provoked hostile reaction: ignoring the possibility that an aggregate of ions in a condensed matter matrix may behave differently to a collection of freely moving ones, it was asserted that nuclear fusion could not be responsible for the claimed excess heat.

Then came 'failure to replicate' by a number of groups, equated with the non-existence of the phenomenon, ignoring the fact that if different groups get different results there can be two explanations, one that the people who see some effects are bad experimenters, and the other that they were in fact better at creating the precise conditions needed for an effect to be seen.

Usually in such cases time tells which side is right, but here the steadily mounting evidence that there was a real effect was suppressed through the publication policies of the major journals. Consequently, these apparently supportive results are not known to most scientists, who simply take it for granted that the Pons-Fleischmann claims have been disproved.

In an attempt to promote proper discussion of the issue, I tried in 2002 to upload a survey by Storms to the preprint server arxiv.org, the natural place for facilitating such discussion, but the moderators frustrated this intent by deleting the review, declaring it "inappropriate" (chemists, being a more robust species than physicists, were permitted to see it on their own server chemweb.com).

A breath of fresh air has been introduced into the situation now, with the recent decision of the US Department of Energy to review the research; if the reviewers simply look at some of the research going on they will almost inevitably conclude that fusion can take place at ordinary temperatures, with a yield far in excess of the 'almost undetectable level' referred to in Langmuir's lecture.

The overall situation seems profoundly unsatisfactory. The system built up over the years to promote scientific advance has become one that narrow-minded people can use to block any advance that they deem unacceptable. This demands urgent review: otherwise, just as astronomy became fixated on the reasonably accurate, but wrong, Ptolemaic model, science will become fixated in a respectable, but inaccurate, view of reality.

Teacher 1:
1) Let me observe that Teacher 5, who is usually very vocal on the Phys-L list, preferred not to share personal thoughts. He quotes the UnderNews, he quotes the summary of Josephson’s lecture. I am not surprised; most physics teachers are likely to be undecided. They are open-minded people but the flow of information about cold fusion is not normal. Many of them are old enough to remember the 1989 euphoria and their opinion on the subject was formed at that time. I was also convinced, until about two years ago, that cold fusion claims should be rejected.

2) In reading the quoted summary of the lecture again I notice that Josephson refers to Marcello Truzzi, the originator of the "extraordinary claims require extraordinary evidence" phrase. Who was this man? According to the website devoted

to him Truzzi made “contributions to sociology, the history of juggling, magic, and the study of the paranormal.” Aha, study of the paranormal again! Should cold fusion still be considered paranormal or should this label be applied to the “suppression of research [in a targeted field] by both science and journalism”?

3) I find it interesting that Josephson, who is not a cold fusion researcher, refers to “the possibility that . . .nuclear fusion could not be responsible for the claimed excess heat.” He probably thinks that what is observed in “an aggregate of ions in a condensed matter matrix” might be very different from what is known to occur in hot plasma. Wasn’t this also the main observation made by J. Schwinger, another theoretically inclined Nobel Laureate who was trying to make sense out of cold fusion? Are they referring to the lowering of the Coulomb barrier in condensed matter, for example, via some kind of screening effect? That is what Steven Jones thinks, according to his papers at the last cold fusion conference (see item # 113 at my cold fusion website). Some theoretical papers presented at the conference also addressed this topic.

Teacher 1:
Guess what? Messages from this thread, plus additional comments, became unit #161 on my website. See it at:

http://blake.montclair.edu/~kowalskil/cf/161josephson.html

I might append other messages and comments later, if they materialize.Share what is worth thinking about cold fusion social issues, about its scientific aspects and, above all, what should we tell students about the controversial cold fusion topics? I suspect that questions of that nature will be asked this fall, after the DOE review process is completed.

During my retirement party I gave a short description of current cold fusion claims. It was based on unit #152 on my website. I also mentioned that I might become a cold fusion researcher. The audience consisted of faculty and some students. Recently I was contacted by a student who was present, not one of my students. In an e-mail message she introduced herself and asked if she could assist me in this research. We met several days later and I tried to discourage her. This will not help you professionally, I said. On the contrary, this can hurt you. Many will think that you are a crackpot, like those who take astrology or UFO seriously. Finding financial support for such research is likely to be impossible (I am going to pay for the necessary expenses from my own pocket). Read about the unhealthy situation at my website and think again. I will not be disappointed if you change your mind. About a week later I received this reply:

"I wanted to inform you that I have carefully thought about the pros and cons and I feel that working with you would be very beneficial to me. Hopefully you will allow me to assist you in the fall."

Do I have a moral right to take advantage of this offer? She is a sophomore with excellent grades, majoring in both physics and chemistry.

Teacher 3 (in private again):
In order to convince her that very severe career-damage is guaranteed, you could write up a simple contract where she promises to keep her involvement in this a secret from anyone in the science biz (and keep the stuff OFF her resume, especially off her grad-school application.)

Cold fusion suppression is a conspiracy theory? Yeah, suuuure. Just let any non-tenured faculty try seriously performing some CF experiments, and see how fast they're booted out of a job. With suppression at such a high level, and with so-called "Skeptics" ready to start back-room proceedings against anyone who doesn't toe the line re. conventional physics, only retired physicists and physics-hobbyists can risk working in the field. And that's really shameful. What does it say about the physics community? It says that the equivalent of bicycle-shop owners will make the breakthrough if there are any breakthroughs to be made. (Or perhaps like the situation with Robert Goddard, it will be some 3rd World dictator who will do the equivalent of dropping heretical impossible ICBMs on London in 1944.)

Remember that J. OM. Bockris of Texas A & M, who literally wrote the book on electrochemistry, published some CF/transmutation papers and was attacked by colleagues who circulated a secret petition to get him kicked out of the university. Even with his enormous academic stature he almost didn't make it. It's my belief that he was saved only

because he was on the verge of retirement, and for that reason the group trying to remove him didn't have the resolve.

Teacher 5:
She's an adult, correct? She's had full disclosure, right? She's an undergrad. where few, if any, do research assisting? She may chose to report this work or not, where non-reporting won't signal anything? She may stop anytime or continue till graduation or beyond?

Teacher 6:
Over the weekend I attended a conference on teaching non-majors introductory astronomy classes. In passing, one of the speakers asked, "How do we handle alternative theories and gaps in our understanding of the universe?" This (and the cold fusion thread) caused me to wonder: At what point is it appropriate to bring up theories that are hard to handle, are probably wrong but may be true, and just aren't mature?

Specific Problems:

*Should MOND[Modified Newtonian Mechanics] ever be discussed? I have seen a grad student burned at the stake for giving a journal review talk on MOND, and I've also seen a leading astronomer with his face in his hands worried that MOND may have some validity. I usually rely on Greg Bothun's site: < http://zebu.uoregon.edu/special/mond.html > What do you do?

*How do you answer the question: "What was there before the Big Bang?" I typically say, "Here be dragons" and use magazine articles and popular books to discuss some of the theories. Do you have problems with religion creeping into your classroom with this topic?

*How many dimensions do you say the universe has? *Are string theory or super symmetric particles mentioned? I know I don't have more than a very surface understanding of these topics. What should/can we hope for our students to understand? Is this best taught as a "Why we should spend money to build accelerators and bury detectors?" and then test their knowledge of FERMI and CERN, and Super Kamiokande?

*Does Cold Fusion come up when you discuss nuclear energy? *What other discussions are best dropped or skirted around? . . .

Teacher 7:
. . . New, and especially controversial or speculative, developments almost never need to be discussed in a text. If it is appropriate, the instructors can bring them up in class (this assumes that the instructors are keeping up with the subjects they are teaching).

Teacher 8:
. . . [Students should] be exposed to the idea that scientists and educated people could be biased such as Galileo's detractors who refused to look through a telescope because the "images were not real". Or consider how Einstein persisted in raising objections to QM, how Boltzmann committed suicide before his theory became scientific canon, or how MDs continued to wear bloodstained wool coats long after there was clear evidence for antiseptic operating conditions. Paradigm change takes time. . . .

Teacher 1:
I often see the term paradigm in connection with cold fusion. But I am not ay all convinced that the validation of experimental cold fusion facts would lead to a change of paradigm. Why should one take it for granted that the existing paradigm can not possibly accommodate theoretical models of cold fusion phenomena? Discovery of nuclear fission (in 1939), as far as I know, did not make the liquid model of nuclear reactions invalid. Fission of uranium, after being recognized, was at once explained in terms of this model. Who knows what will happen, and how soon, after the experimental cold fusion claims are recognized as valid by the mainstream scientific community.

Here are some well known details. The paper of Hahn and Strassmann, announcing the discovery of fission, contained an interesting phrase “it makes no sense to us.” Hahn was referring to strange transmutations, such as production of

barium. The explanation, first by Lise Meitner and then by Niels Bohr, appeared about one year later. It was based on the liquid drop model that was used since 1936 to make sense of nuclear reactions.

Teacher 1:
After posting the above I found (on the Internet) the following abstract of a paper in the Czechoslovak Journal of Physics (**49** (6): 985-992, June 1999). The author is **Xing Z. Li** (*Department of Physics, Tsinghua University, Beijing 100084, China) and the title is: "***Overcoming of the Gamow tunneling insufficiencies by maximizing the damp-matching resonant tunneling."** This does not sound like a suggestion for a departure from Gamow's theory of tunneling; it sounds like an attempt to use it creatively. But my knowledge of the theoretical physics is very limited. Perhaps somebody will write a summary of current attempts "to make sense of cold fusion data" for introductory physics teachers. That could become a very useful item on my website.

"The resonant quantum tunneling current through the barrier between two wells may be maximized when the damp (absorption) in one well matches the barrier parameters. The maximum resonant tunneling current is much greater than the conventional expectation by a factor of 1/TET^2 (1/TET^2 is the Gamow tunneling factor). It is shown that with all the established quantum mechanics, very much higher reaction probabilities between nuclei in contrary to the Gamow theory can be explained in agreement with experiments. Particularly, the resonance will select the sub-barrier fusion with a suitable fusion rate which matches the barrier parameters. This selective resonant tunneling model is able to explain both the hot fusion data (e.g. the width of resonance in 11B(p, alpha)2alphas reaction) and the cold fusion data (e.g. "excess heat" without any commensurate neutron and gamma."

Teacher 9:
Way back at the beginning of this thread, Teacher 6 asked a two-part question: "How do we handle alternative theories and gaps in our understanding of the universe?" And mostly "alternative theories" is a codeword for crackpot theories ... but I would like to nudge the discussion toward the second part of the question: IMHO there ought to be more discussion of non-crackpot unanswered questions. Lists of important open questions in physics are easy to find. One of my favorites is John Baez's list

* * * * <http://math.ucr.edu/home/baez/physics/General/open_questions.html>

Another is David Mermin's list of "alternate" questions to ask a visitor from the future:

* * * * <http://www.qub.ac.uk/mp/questions/alternate.html>

Naturally you can find lots of other lists:

* * * * < http://www.google.com/search?q=important+open-problems+physics>

and it is also interesting to look at the open problems
in allied fields (math, computing, fluid dynamics, ...)

Also in this context I suppose one is obliged to mention "Physics in a New Era" .... the view from the National Academy:

* * * * <http://www.nap.edu/openbook/0309073421/html/>

but I find it to have more detail than imagination. Tangential remark: There is a whole lot of good physics and math stuff on Baez's site:

* * * * <http://math.ucr.edu/home/baez/>

Well worth exploring.

Teacher 1:
The observation about , "a codeword for crackpot theories," is likely to be correct; especially when such theories are

developed by obscure people publishing in obscure journals. Credentials and professional associations do not guarantee anything but they should be looked upon very seriously when it comes to alternative theories, or earth-shaking experimental data.

I believe that exposing crackpot theories is a worthwhile exercise, provided one is knowledgeable enough. In exploring the Internet I often encounter claims that there is free energy from torsion fields. That is far above my head, as far as mathematics is concerned. But I will address this topic in the next unit.

Return to the clickable list of items

Return to the clickable list of items

162) Torsion effect: does it explain cold fusion?

Ludwik Kowalski (7/23/04)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

In searching for cold-fusion-related topics I often encounter claims about “torsion field” and about “excess energy from vacuum.” The term “zero-point energy” is often used in this context, as one can check by browsing the Internet. An e-mail message from China, received on July 16, made me think about this topic again. The author wrote:

Dear Dr. Kowalski:
. . . A model called "Vortex Dynamics" is proposed by us to give an explanation for cold fusion. This model was mentioned by Thomas Dolan, IAEA Officer(1995-2001) for fusion in his speech as five majoring models for CF mechanism, in the summary session at ICCF-9, Tsinghua Univ. May,2002, Beijing. I would like to introduce this model to you for your reference. Thank you for your attention, and awaiting your comments. With best regards.

Prof. Xingliu Jiang, Physics Department of
Bejing University of Aeron. and Astron.
jiangxl@buaa.edu.cn

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The attached file (shown at the end of this unit) did not convince me that the authors found an “explanation of cold fusion.” The impression was the opposite; it added to confusion in my mind. What do Zero-Point Energy and Vortex Dynamics speculations have to do with cold fusion? Did they develop a “theory of everything?” Can that theory be used to “explain” burning of wood, reality of quarks and the assassination of Julius Caesar? I did not reply because I do not like to express opinion about topics belonging to unfamiliar areas of physics. My reaction to Zero-Point Energy claims (seen on the Internet) and to Hydrinos (explained in an earlier unit) are essentially the same -- wait till somebody else helps me to accept them.

This unit was prompted by the ongoing thread on Phys-L, an Internet list for physics teachers. The name of the thread is “Teaching science on the edge of knowledge.” An astronomy teacher asked: “How do we handle alternative theories and gaps in our understanding of the universe?” A physics teacher responded “mostly ‘alternative theories’ is a codeword for crackpot.” Then he elaborated on “the gaps in our understanding.” In doing this he made a reference to John Baez's website:

http://math.ucr.edu/home/baez/physics/General/open_questions.html

At that site (see http://math.ucr.edu/home/baez/vacuum.html) I found the following description. How does the Vortex Dynamics theory of Xing-liu Jiang differ from the topic described by Baez? Unable to answer this question let me turn to what Baez wrote about the energy of the vacuum.

What's the Energy Density of the Vacuum?

John Baez

August 25, 1999

People talk a lot about "vacuum energy" or "zero-point energy" - that is, the energy density of empty space. In cosmology, people also call this quantity the "cosmological constant", or "dark energy". Sometimes kooky people get really excited about the idea that if we could only *use* this energy somehow, all our problems would be solved. But first things first! Does this energy really exist? And if so, how much of it is there?

Once upon a time, someone named Amw wrote:

>I have heard widely varying numbers for so called "zero point
>energy", some as low as practically zero and some as high as
>astronomical. It gets to the point I am not sure what to think.

To which I replied:

Yes, one hears lots of conflicting stuff about this. However, you've come to the right place to get to the bottom of it all.

Here's the deal. We have two fundamental theories of physics: quantum field theory and general relativity. Quantum field theory takes quantum mechanics and special relativity into account, and it's a great theory of all the forces and particles *except* gravity, but it ignores gravity. General relativity is a great theory of gravity, but it ignores quantum mechanics. Nobody knows how to reconcile these theories yet. That's what people working on "quantum gravity" are trying to do.

Now, the reason I'm telling you this is that quantum field theory and general relativity have really different attitudes towards the energy density of the vacuum. The reason is that quantum field theory only cares about energy *differences*. If you can only measure energy differences, you can't determine the energy density of the vacuum - it's just a matter of convention. As far as we know, you can only determine the energy density of the vacuum by experiments that involve general relativity - namely, by measuring the curvature of spacetime.

So, when you ask about the energy density of the vacuum, you get different answers, depending on whether the person answering you is basing their answer on general relativity or quantum field theory. Let me run through the 5 most common answers, explaining how people reach these different answers:

1. We can *measure* the energy density of the vacuum through astronomical observations that determine the curvature of spacetime. These measurements say the energy density is VERY CLOSE TO ZERO. We're not really sure if it's positive, negative or zero. Some recent observations suggest that it's positive, but this is not yet certain. All we really know is an upper bound: in terms of mass density, its absolute value is less than 10-29 grams per cubic centimeter. In terms of energy density, this is about 10-9 joules per cubic meter.

To believe these measurements are right, one must have some faith in general relativity, because that's the theory which we use to relate spacetime curvature to energy density. The more accurate measurements attempt to determine an actual *value* for the energy density of spacetime, or at least its *sign*. These require more faith in general relativity, and also other assumptions about cosmology. However, the basic fact that the energy density of spacetime is very close to zero is almost inarguable: for it to be false, general relativity would have to be *very* wrong.

2. We can try to *calculate* the energy density of the vacuum using quantum field theory. If we calculate the lowest possible energy of a harmonic oscillator, we get a bigger answer when we use quantum mechanics than when we use

classical mechanics. The difference is called the "zero-point energy". The zero-point energy of a harmonic oscillator is 1/2 Planck's constant times its frequency. Naively we can try calculating the energy density of the vacuum by simply summing up the zero-point energies of all the vibrational modes of the quantum fields we are considering (e.g. the electromagnetic field and various other fields for other forces and particles). Vibrational modes with shorter wavelengths have higher frequencies and contribute more vacuum energy density. If we assume spacetime is a continuum, we have modes with arbitrarily short wavelengths, so we get INFINITY as the vacuum energy density. But there are problems with this calculation....

3. A slightly less naive way to calculate the vacuum energy in quantum field theory is to admit that we don't know spacetime is a continuum, and only sum the zero-point energies for vibrational modes having wavelengths bigger than, say, the Planck length (about 10-35 meters). This gives an ENORMOUS BUT FINITE vacuum energy density: about 1093 grams per cubic centimeter! But there are problems with this calculation, too....

One problem is that treating the vibrational modes of our fields as harmonic oscillators is only valid for "free field theories" - those in which there are no *interactions* between modes. This is not physically realistic. However, while taking interactions into account changes the precise answer, we are still left with an enormous energy density. And there's an even bigger problem, too....

4. Quantum field theory as it is ordinarily done ignores gravity. But as long as one is ignoring gravity, one can add any constant to ones definition of energy density without changing the predictions for anything you can experimentally measure. The reason is that without measuring the curvature of spacetime, one can only measure energy *differences*. The big problem with calculations 2 and 3 is that they ignore this fact. If we take advantage of this fact we are free to redefine energy density by subtracting off the zero-point energy, leaving an energy density of ZERO. In fact this is what is ordinarily done in quantum field theory.

5. An even less naive way to think about the vacuum energy density in quantum field theory is the following. In quantum field theory we are neglecting gravity. This means we are free to add any constant whatsoever to our definition of energy density. As long as we are free to do this, we can't really say what the vacuum energy density "really is". In other words, if we only consider quantum field theory and not general relativity, the vacuum energy density is NOT DETERMINED.

So, I've given you 5 answers to the same question:

1. VERY CLOSE TO ZERO
2. INFINITY
3. ENORMOUS BUT FINITE
4. ZERO
5. NOT DETERMINED

Which should you believe? I believe 1) because it is based on experiment and fairly conservative assumptions about general relativity and astronomy. Answers 2)-4) are based on somewhat naive theoretical calculations. Answer 5) is the best that quantum field theory can do right now. Reconciling answers 1) and 5) is one of the big tasks of any good theory of quantum gravity.

The moral is: for a question like this, you need to know not just the answer but also *the assumptions and reasoning that went into the answer*. Otherwise you can't make sense of why different people give different answers. For more on the zero-point energy of the harmonic oscillator try this and this. These require more mathematical sophistication.

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What follows is a description the dark energy found on the Internet. It is an interesting but I see no refernce to cold fusion in it. Is dark energy the same thing as zero-point energy of vacuum? It seems to be the same. The author is a

theretical astrophysicist; his paper has been published in June of 2004. It was published in a very prestigeous journal, Physical Review Letters (Phys.rev.Lett. 93 92004) 011301). The paper can be downloaded from:

http://arxiv.org/abs/astro-ph/0402316

Unfortunately, the best I can do is to read sentences between equations. That is not a critical comment; Salisury is addressing other experts.

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Public release date: 1- Jul-2004

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Dark matter and dark energy may be different aspects of a single unknown force

In the last few decades, scientists have discovered that there is a lot more to the universe than meets the eye: the cosmos appears to be filled with not just one, but two invisible constituents –dark matter and dark energy – whose existence has been proposed based solely on their gravitational effects on ordinary matter and energy.

Now, theoretical physicist Robert J. Scherrer has come up with a model that could cut the mystery in half by explaining dark matter and dark energy as two aspects of a single unknown force. His model is described in a paper titled "Purely Kinetic k Essence as Unified Dark Matter" published online by Physical Review Letters on June 30 and available online at http://arxiv.org/abs/astro-ph/0402316 .

"One way to think of this is that the universe is filled with an invisible fluid that exerts pressure on ordinary matter and changes the way that the universe expands," says the professor of physics at Vanderbilt University.

According to Scherrer, his model is extremely simple and avoids the major problems that have characterized previous efforts to unify dark matter and dark energy.

In the 1970's, astrophysicists postulated the existence of invisible particles called dark matter in order to explain the motion of galaxies. Based on these observations, they estimate that there must be about 10 times as much dark matter in the universe as ordinary matter. One possible explanation for dark matter is that it is made up of a new type of particle – dubbed Weakly Interacting Massive Particles, or WIMPs) – that don't emit light and barely interact with ordinary matter. A number of experiments are searching for evidence of these particles.

As if that weren't enough, in the 1990's along came dark energy, which produces a repulsive force that appears to be ripping the universe apart. Scientists invoked dark energy to explain the surprise discovery that the rate at which the universe is expanding is not slowing, as most cosmologists had thought, but is accelerating instead. According to the latest estimates, dark energy makes up 75 percent of the universe and dark matter accounts for another 23 percent, leaving ordinary matter and energy with a distinctly minority role of only 2 percent.

Scherrer's unifying idea is an exotic form of energy with well-defined but complicated properties called a scalar field. In this context, a field is a physical quantity possessing energy and pressure that is spread throughout space. Cosmologists first invoked scalar fields to explain cosmic inflation, a period shortly after the Big Bang when the universe appears to have undergone an episode of hyper-expansion, inflating billions upon billions of times in less than a second.

Specifically, Scherrer uses a second-generation scalar field, known as k-essence, in his model. K-essence fields have been advanced by Paul Steinhardt at Princeton University and others as an explanation for dark energy, but Scherrer is the first to point out that one simple type of k-essence field can also produce the effects attributed to dark matter.

Scientists differentiate between dark matter and dark energy because they seem to behave differently. Dark matter appears to have mass and to form giant clumps. In fact, cosmologists calculate that the gravitational attraction of these clumps played a key role in causing ordinary matter to form galaxies. Dark energy, by contrast, appears to be massless and spread uniformly throughout space where it acts as a kind of anti-gravity, a repulsive force that is pushing the universe apart.

K-essence fields can change their behavior over time. When investigating a very simple type of k-essence field – one in which potential energy is a constant – Scherrer discovered that as the field evolves it passes through a phase where it can clump and mimic the effect of invisible particles, followed by a phase when it spreads uniformly throughout space and takes on the characteristics of dark energy.

"The model naturally evolves into a state where it looks like dark matter for a while and then it looks like dark energy," Scherrer says. "When I realized this, I thought, 'This is compelling, let's see what we can do with it.'"

When he examined the model in more detail, Scherrer found that it avoids many of the problems that have plagued previous theories that attempt to unify dark matter and dark energy.

The earliest model for dark energy was made by modifying the general theory of relativity to include a term called the cosmological constant. This was a term that Einstein originally included to balance the force of gravity in order to form a static universe. But he dropped the constant cheerfully when astronomical observations of the day found it was not needed. Recent models reintroducing the cosmological constant do a good job of reproducing the effects of dark energy, but do not explain dark matter.

One attempt to unify dark matter and dark energy, called the Chaplygin gas model, is based on work by a Russian physicist in the 1930's. It produces an initial dark-matter-like stage followed by a dark-energy-like evolution, but it has trouble explaining the process of galaxy formation.

Scherrer's formulation has some similarities to a unified theory proposed earlier this year by Nima Arkani-Hamed at Harvard University and his colleagues, who attempt to explain dark matter and dark energy as arising from the behavior of an invisible and omnipresent fluid that they call a "ghost condensate."

Although Scherrer's model has a number of positive features, it also has some drawbacks. For one thing, it requires some extreme "fine-tuning" to work. The physicist also cautions that more study will be required to determine if the model's behavior is consistent with other observations. In addition, it cannot answer the coincidence problem: Why we live at the only time in the history of the universe when the densities calculated for dark matter and dark energy are comparable. Scientists are suspicious of this because it suggests that there is something special about the present era.

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What follows is an example of another difficult to accept claim. The quoted fragment was found in an article published at:

http://antigravitypower.tripod.com/FreeEnergy/

“Independent inventors have put together electrical generators based mostly on the rotating magnet concept. What is highly interesting is that their experimental data is showing efficiencies exceeding 100 percent, and some show output energy exceeding input energy by a factor of two or more times ! These machines are known as 'Free-Energy' generators.

Nobody is quite sure just where this excess energy is coming from, but theories abound from the idea that this is a safe, radiation-free conversion of nuclear forces directly into electrical energy, to the concept that this type of device taps the so-called 'zero-point-energy' of the space-fabric itself.”

What is the evidence that zero-point energy contributs to the operation of motors based on rotating magnets?

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The content of the file sent to me by Xing-liu Jiang is essentially the same as in their paper entitled “Torsion Technologies.” That paper, published in New Energy Technologies, (September - December 2003, Issue #5-6, pages 14-15) and earlier papers, can be downloaded from:

www.google.com/search?hl=en&ie=UTF-8&q=Xing-liu+Jiang&btnG=Google+Search

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EXTRACTS FROM THE FILE SENT TO ME BY X.J.

Journal of Theoretics, Volume 5-6, Dec 2003/Jan 2004

Torsion Field Effect and Zero-Point Energy in Electrical Discharge Systems

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I. INTRODUCTION

Many laboratories in world have observed nuclear reactions and excess heat in electrochemical systems. The mechanism of such anomalous phenomena is not being well understood according to normally accepted physics. Nuclear products with high concentration and tracks with highly collimated lines of low energy nuclear reactions in the electrochemical systems were recorded by CR-39 solid detectors and films at our laboratory [1,2]. These facts suggest that quasar model with spiral structure and extremely high enrage cosmic rays in the center could be used for explanation of the mechanism. It is supposed to use the concept of torsion field to interpret the observed phenomena, typically, the properties of axial acceleration, memory effect, and the polarized nuclear reactions with torsion effect [3].

II. PHENOMENA IN NATURE AND AT LABORLATORIES

The vortex and spiral structures are the archetype that appears at all levels of nature and laboratories, for example, atom structure, vortex lattice in superconductors, dense plasma focus, lightning, quasar etc. Recent report of dark matter annihilation at the galactic center describes that the cold dark matter near the galactic center is accreted by the central black hole into a dense spike [4]. Particle dark matter annihilation makes the spike to be a compact source of photons, electrons, positrons, protons, antiprotons, and neutrinos. It remind us for that there is a similarity among pitting corrosion with electrochemical noise, laboratory plasma pinching (dense plasma focus, for example), fast laser induced ion beams[5],and quasar spiral model with high-energy cosmic rays in spiral center in spite of large dimension difference.

Comparing the experimental results of electrochemical cells with excess heat and nuclear transmutation to astrophysics phenomena, it is supposed that the investigation of vortex dynamics of torsion coherence with the zero-point energy is essential for tapping the zeropoint energy. As it is predicted by quantum mechanics that the vacuum is seething with active energy, even at temperature of zero point Kelvin. This zero-point energy (ZPE) can be thought as an infinite number of virtual photons that popping out of the vacuum and going back in, but should a measurable effect en masse. To exam the origin of ZPE background, the interaction of matter with the ZPE can be treated on the basis of charged point particles interaction with a background of electromagnetic zero-point radiation with spectral-energy density.

Based on observation in the experiments, the following features are considered in order to understand the experimental results with electrical discharge systems.

A. ELECTROCHEMICAL DOUBLE LAYER

In an electrolytic cell, the electrolysis with high conductivity and the electrochemical double layer with large layer-capacitance lead to a typical structure of the cathode potential distribution similar to the cathode drop of glow discharge in low gas pressure. For a compact layer the thickness of the double layer is equal to one ionic layer, across which there is a linear fall of potential. Thus, high electric field exists in some regions on the surface of the cathode. The local

enhancement of the electric field on the cathode surface with the double layer is related to the protrusions and cracks similar to the tip discharge in air or in a vacuum. The current distribution depends strongly on the surface roughness and the work function of the electron emission. A high transient current density (> 10^8 A/cm^2) could be expected due to enhanced field.

B. ENERGY CONCENTRATION

On the cathode surface, the high persistent electrical fields (>10^7 V/cm) and large equivalent capacitance (>250μF/cm^2) lead to a high energy concentration in the double layer [6]. The concentrated field on the tips of the protrusions or cracks after a longloading period with deuterium on the palladium cathode surface creates a high transient electron flux because of the large distributed capacitance and the negligible inductance in a localized discharge mini-network. The experimental data show that the reactions take place only in some restricted areas that have specific properties. The idea of micro fusion due to the results of energy concentration and the high deuteron flux could be used to explain the nuclear transmutation.

C. TORSION FIELD AND THEIR EXPERIMENTAL MANIFESTATIONS

Elementary particles have the moment of quantity of motion, i.e. spin. If in any substance the spins of particle have a preferable direction, then is interpreted as spin polarization of the substance. Every substance creates a torsion-field (or called spin-field or axion field) in the space surrounding it when polarized by spins [7]. The superposition of torsion field, generated by the atomic and nuclear spins of each molecule, determines the intensity of torsion field in the space surrounding each molecule. Torsion field has strong penetration ability and does not interact with the crystal lattice of substances. The torsion field created by rotation of some sort of matter is concentrated in two opposite beams propagating along the rotation axis. The intensity of torsion-field with some lower constant value can be retained for several weeks after the rotation is stopped. Such the property of vortex mater has been observed recently in type II superconductors with magnetic flux line lattice [8]. The studies of vortex matter of type II superconductors have shown a number puzzling phenomena associated with vortex motion, including: lowfrequency noise and slow voltage oscillations; a history-dependent dynamic response, and memory of the direction, amplitude duration and frequency of the previously applied current.

Some behaviors of the torsion-field effect have been observed in the experiments of electrolysis regarding to so-called cold fusion, such as the gas bubble chains come out from the protrusions of the cathode surface for long time after switching off the electrolysis potential. The more surprising thing is the heat after death, which has been recorded by many labs. This phenomenon could be explained by the persistence effect of torsion-field produced by vortex dynamics of tip effect.

D. EXPERIMENTAL RESULTS IN THE ELECTROCHEMICAL CELL

CR-39 plastic films possessing with a high degree of optical clarity and isotope in track response, and been sensitive to neutron, proton,

tritium, alpha, and other charged particles, were used to detect the products of nuclear reactions. The films of CR-39 were immersed in the NaOH electrolyte of heavy water and placed adjacent to the tips of the cathode [9]. [The rest of this section is skipped. It contains figures which are not at all convincing. L.K.]

E. SONOLUMINESCENCE AND ?BUBBLE NUCLEAR FUSION?

Some scientists of Oak Ridge National Laboratory in America reported their articles of bubbles experiments in Science. Experimental results show that the radiation lights of sonoluminescence possess three characters: short duration with picoseconds; wide continual spectrum; highly oriented thin beams. Based on those characters, vortex dynamics with axial acceleration of bubble collapse could be inferred. [The rest of this section is skipped. L.K.]

III. CONCLUSION

Researchers of new energy study of ?cold fusion? type should pay great attention to the general processes of electrolysis to find the key points, which could play major role in the transit form the electrochemical processes to processes of torsion and the nuclear processes. From the authors? point of view, the evolution of double layers is of importance to understand the anomalous effect, typically on the protrusions of cathode. The change of space-time near the tips due to torsion field generated is expected to delivery the zero-point energy, and dynamic Casimir effect for the evolution of gas bubbles on the tips are expected to generate photons and excess heat [1].

The yields of transmutation products are related to the current distribution on the cathode surface. The cathodes of thin wire were benefit to the generation of nuclear reactions and excess heat for electrochemical systems. Careful examination of the evolution of electrochemical double layer will lead to a good understanding of pitting corrosion with electrochemical noise, and further to recognize the anomalous excess heat and nuclear reactions. Heat after death was observed in many laboratories. It is believed that the persistence behaviors of torsion field could be used to explain such anomalous phenomena. The contact between beads coated with a thin metallic layer or multilayered film and palladium black particles could be regarded as point-contact similar to the tip-effect [13]. We may say that, it is expected to reveal the puzzles of the mechanisms of high-Tc superconductivity with pinning vortices by torsion field theory [14,15].

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163) On reproducibility of cold fusion experiments

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1) According to a very recent article (2004), in the online encyclopedia (Word IQ):

http://www.wordiq.com/definition/Cold_fusion

"In the best experimental set-up, excess heat was observed in 50% of the experiment reproductions. Various fusion ashes and transmutations were observed by some scientists."

Most people know that random guessing, when someone is trying to predict one of two equally probable outcomes, should result in 50% of correct predictions. In other words they know that, in such situations, the ability to predict 50% of outcomes does not demonstrate that the instrument of prediction is reliable. On the contrary, it demonstrates that the claimed instrument (for example a theory) is totally unreliable.

From this somebody might conclude that the reported 50% reproducibility is the best argument against the above mentioned excess heat. Such reasoning would be incorrect. When 50% of competent scientists see a disputed effect then that effect is likely to be real. I am thinking about a situation in which the number of attempts to confirm the discovery is large. In my opinion, arguments for or against cold fusion should not depend on the number of confirmations; they should be made on the basis of examinations of procedures leading to particular claims. Scientific experiments are not predictions; they are fact gathering attempts. On the other hand, claims should not be taken for granted unless the level of reproducibility, by competent researchers, is close to 100%.

2) Reproducibility is the most essential requirement in experimental science. Let me mention some questions linked with it. Some phenomena, for example, are very difficult to observe. The required instruments (satellites, accelerators, ultra-clean labs, etc.) are expensive and only a limited number investigations can be performed. How many confirmations are needed to accept a claim? That is one question; and here is another. Suppose that results coming from two teams of researchers differ by 30%. Does it mean that the claim made by one team is confirmed by another team? It depends on the nature of the claim. If the purpose of the experiment is to confirm the existence of a phenomenon, such as generation of excess heat, then the difference of 30%, in the amount of released energy, would not be significant. But a 30% discrepancy would be very significant if the purpose was to measure a physical quantity, such as length or current, very accurately.

3) Irreproducibility usually indicates that experimentalists are not aware that something is not the same when apparently identical experiments are conducted. In the case of cold fusion, according to many, the outcome depends on hard to control impurities and defects. This became apparent when electrolytic experiments with palladium cathodes were performed to confirm reality of excess heat. Electrodes supplied by one manufacturer consistently resulted in confirmations while electrodes supplied by another manufacturer consistently resulted in failures to confirm. Furthermore, excess heat was consistently observed with some batches of electrodes supplied by the same manufacture, and not with others. Hard to identify differences, perhaps at the very low level of contamination, are likely to be responsible for the absence of 100% reproducibility. Let me show how this was described by E. Storms. In the 2001 paper, downloadable from the library at <http://www.lenr-canr.org>, and entitled "Cold Fusion: Objective Evaluation," Storms wrote:

"Presence of reproducible patterns within the data sets are as important as achieving reproducibility of the phenomenon its self. Such patterns are based on AE [anomalous energy] being produced only under certain unique conditions by all studies. ..... The work at SRI, lead by Dr. McKubre, studied the phenomena, first with $6M provided by EPRI (Electric Power Research Institute). After this program was terminated, work was continued by IMRA (Japan) at nearly the same level. A very small effort is now being funded by the US government. Over the course of this work, several designs of flow-type calorimeters were used that share the following characteristics:

A. The cells are sealed and contain a recombiner. As a result, no gas leaves the cell. Therefore, uncertainty in the amount of recombination is not an issue. Successful action by the recombiner is monitored using different methods including change in gas pressure.

B. The cells contain a heater, which maintains a constant inner temperature. Power to this heater can be adjusted to compensate for any change in temperature caused by electrolysis or by anomalous processes. This heater is also used to determine whether the power measurement, based on the flow rate and temperature change of a cooling fluid, is accurate. Sensitivity better than ±0.01 W (±0.1%) is claimed.

C. The electrolytic cell, its surrounding heater, and the cooling-fluid channels are all contained within a silvered, evacuated Dewar in order to isolate them from the environment.

D. The whole assembly is immersed in a fluid bath, which maintains a constant environment of 30±0.003°C. This bath is also the source of cooling fluid. Consequently, most studies are done at a temperature above 30°C.

E. A constant flow pump is used to circulate cooling fluid. Flow rate is checked periodically by weighing the fluid passing through the calorimeter. Better than 98% of power produced within the cell is captured in this fluid.

F. All aspects of the measurement are under computer control, which provides continuous monitoring, and redundant RTD sensors are used for temperature measurement.

G. The deuterium content of the palladium cathode is determined by measuring its change in resistance.

Nineteen samples of palladium were found to make AP with consistent results. One consistent behavior is shown in Figure 1 as the relationship between AP and average composition of the cathode. Figure 2 shows another pattern as the effect of applied current on AP. These two behaviors are found to be produced by all samples of palladium used in this work and by all samples reported in the literature when the necessary measurements were made. Variations in reported values are easily explained by the different shapes and sizes of the cathodes used, and by the amount of active material present on their surfaces. In addition, the following behaviors are also seen by everyone who has made suitable measurements.

A. The average D/Pd ratio must exceed a critical value. This value differs somewhat between studies because only the

average composition can be determined and the value depends on the method used and the shape of the cathode. Typically, the value lies between D/Pd=0.85 and 0.90. Infrequently, compositions above this range are found to be dead for unknown reasons.

B. The current must be maintained for a critical time. This time is variable and presumably depends on how rapidly the surface can acquire the active structure. The time is zero for thin layers of Pd while it can be as long as months for bulk palladium. Failure to wait the necessary time is one reason some people have not seen the effect.

C. The current density must be above a critical value. Applied current determines the surface composition, hence the nature of the active structure. A value above 150 mA/cm 2is usually found for bulk palladium. No critical value appears to be necessary for thin layers of palladium.

D. Inert palladium can sometimes be activated by addition of certain impurities to the electrolyte. These impurities are found to help the surface achieve a higher deuterium content.

E. The effect occurs in only a small fraction of samples, but more often in certain batches than in other ones. In fact, all physical properties of palladium are found to be batch specific, making this metal highly variable in its general behavior, even in conventional applications.

These patterns of behavior add evidence that the observations are a real behavior of nature and not caused by error."

4) My accpted Letter to the Editorere of Physics Today (to appear in the September 2004 issue ?) ends in the following way. "".....I also agree with chemist Allen Bard that being able to reproduce experimental results is not good enough; it is only a preliminary step. But wasn't poor reproducibility the central point of criticism when cold fusion was first investigated 15 years ago? **In my opinion, experimental claims should not be disqualified solely on reproducibility; validation should depend on credentials of researchers and, above all, on methodologies they used in particular experiments.**"

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164) A case of mutual deception?

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This morning I received an interesting private message from Teacher XX. It was probably prompted by voices reported in unit #161. But that unit is already too long. Furthermore, I think that this reflection deserves a separate unit. Is it possible that the disbelieve about cold fusion is nothing else but a self-fulfilling prophesy? Is it possible that those who believe in cold fusion are also victims of mutual deception?

Cold fusion and closed-loop self-fulfilling prophecies.

Teacher XX

Is the widespread disbelief in the reality of "Cold Fusion" based upon solid reasoning? Or could it be akin to a self-fulfilling prophecy or an "emergent phenomenon," as when each member of an animal herd rushes forward only because immediate neighbors in the herd are rushing forward? I imagine this conversation to take place between a research scientist and a journal editor:

Sci: Why is Cold Fusion (CF) an illegitimate topic?

Ed: Because there are no reports of CF observations by competent researchers.

Sci: Why are there no reports?

Ed: Because scientists don't submit them to journals.

Sci: On the contrary, I'm a scientist who does submit these to journals, which suggests that the dearth of reports is actually caused by rejections rather than lack of submissions. Why does *your* journal reject them?

Ed: Because everybody knows that CF is not real.

Sci: Why does everybody "know" this?

Ed: Because if CF was real, it would appear in many journals.

Sci: Why is CF not in one specific journal?

Ed: It's because CF is not already in many other journals, and that many journal editors couldn't possibly be wrong.

Sci: But why do the OTHER editors reject CF papers?

Ed: Well, the other journals must have a good reason, and I just follow the consensus expert opinion, and I assume that other editors have sharp minds, and this large group of experts couldn't be wrong about an important topic.

Sci: But what if all editors think exactly as you do: journal A rejects papers because they see no papers in journal B, and journal B rejects papers because they see no papers in journal A? Yet neither editor takes any CF paper seriously enough to sit down and read them with an open mind?

Ed: Well, that doesn't matter. We all know that CF is an illegitimate topic.

(go back to the top and continue.)

The above is obviously aberrant reasoning, where a population of minds can amplify the status of an initially-random opinion. The weak opinion
becomes strong because each member of the group puts great stock in the number of other members having the same opinion, so that a slight majority becomes an overwhelming majority, yet no new information justifies such certainty.

Another example is from 1905, when the Wright Brothers' claims were being ridiculed. This was back when they demonstrated their airplane repeatedly in public for more than a year. Why were there no news articles about the successful flying machine? Scientific American explained its refusal to verify the Wright's claims:

"If such sensational and tremendously important experiments are being conducted in a not very remote part of the country, on a subject in which almost everybody feels the most profound interest, is it possible to believe that the enterprising American reporter, who, it is well known, comes down the chimney when the door is locked in his face -- even if he has to scale a fifteen-storey skyscraper to do so -- would not have ascertained all about them and published them broadcast long ago?"

In other words, "we won't send a reporter, on the grounds that other magazines haven't send a reporter." Each editor also makes the unspoken assumption that those other editors must have a good reason. But what if all editors reason in the same manner? It's a self-fulfilling prophecy. That's what actually happened to the Wrights. The Wright brothers finally broke the jam by traveling to Europe where the closed-loop disbelief-reasoning had not yet emerged in the community. They demonstrated their airplane in Paris. News reporters DIDN'T refuse to attend, and the Wrights became famous overnight... this after more than a year of public demos in the USA at which no invited reporters or government observers ever showed up.

Another example involves "Ball Lightning." Today BL is increasingly considered to be a real phenomenon. But until the last two decades, the major portion of the scientific community was certain that BL was really just some misperceived retinal afterimages, if not hallucinations reported by crazies. On

what grounds did the community base its certainty?

Among other things, we reasoned that if Ball Lightning was real, then quite a few scientists would report seeing it, yet the reports came almost exclusively from the untrained public. The reason for this was not what you might expect. One investigator later questioned the staff of the Empire State lightning research project (which had set up automatic cameras to photograph the many strikes on the Empire State Building in NYC.) He discovered that a large proportion of the research crew had personally witnessed Ball Lightning. Why did they not report this? Because each was afraid of being labeled as a "lightning-ball quack!" It was a self-fulfilling prophecy: scientists were afraid to report a real but "illegitimate" event, and the event became labeled as "illegitimate" mostly on the grounds that, if it was real, scientists would already be reporting the event. The switch flipped, certainty arose, but it arose because of a runaway feedback loop.

The problem caused by community-wide closed-loop reasoning is also mentioned below in regards to the "illegitimate" status of research into hypnotism:

The Research Game (See item no. 2 about new fields of science)
<http://amasci.com/freenrg/rules.html>

The late Dr. Thomas Gold describes the problem of the unwarranted "sharpening" of community opinion. Certain researchers are trimmed out during each round of competition for funding; rejected on the grounds that they are "outliers." This trimming-of-outliers happens repeatedly until a consensus opinion rules the research community:

New Ideas in Science, Gold 1989
<http://amasci.com/freenrg/newidea1.html>

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165) Names and definitions

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In the introduction to his 2001 paper, already mentioned in unit #163, Edmund Storms commented on the name cold fusion. He wrote: "This process was first named 'Cold Fusion' by Steven Jones - an especially poor description." The alternative descriptions: LENR (low energy nuclear reactions) and CANR (chemical assisted nuclear reactions), are subsequently introduced as a replacement. To this one may a recently introduced name CMNP (condensed matter nuclear phenomena). I tried using other names but decided to return to the old name, cold fusion. Most people at once know what i am referring to, especially when the adjective "nuclear" is added.

In meeting Storms last year I noticed that he was not using the LENR and CANR; he used the term cold fusion, like everybody else at the conference. But is this name really a poor description? When I visited Steven Jones last January I asked him if that name is appropriate. He thinks it is, at least for emission of neutrons and charged particles, described by him at 10th International Cold Fusion Conference (see my unit #113). I tend to agree with Steven; the name cold fusion is likely to be appropriate for rare events he is observing but is a poor generic name for the entire field. Yet, it is commonly used as a generic name.

I think that Fleischmann and Pons had no experimental basis for saying, in 1989, that excess heat they measured had nuclear origin. In that context I would agree that the name was "an especially poor description." It generated a lot of unnecessary confusion and hostility. Today, however, the term cold fusion seems to an appropriate description of the process in which excess heat is generated. I am saying this because generation of heat has been shown to by production of ^{4}He at the rate of one atom per 24 Mev of excess heat. The issue of name should not be confused with the issue of definition. Any descriptive name can be introduced to describe a new phenomenon, preferably not a name already used to describe something else. But the definition of the meaning behind the name is much less arbitrary. Some definitions, as described in unit #136, are more appropriate than others.

P.S.
According to "Word iQ" online encyclopedia, "the term **cold fusion** was coined by Dr. Paul Palmer of Brigham Young University in 1986 in an investigation of 'geo-fusion', or the possible existence of fusion in a planetary core. It was brought into popular consciousness by the controversy surrounding the Fleischmann-Pons experiment in March of 1989." Details about prehistory of cold fusion are described by Steven Jones in unit #131. In reading that description one finds another old name, worth mentioning. It is "piezonuclear fusion." This 1988 term refers to fusion in planetary cores, or, more generally, under extremely high pressures. By the way, is it reasonable to suspect that such fusion takes place in the sun,

somewhere between its nuclear core and its surface? I am thinking about a gradual transition from hot fusion (in the core) to much less hot fusion near the surface.

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166) Eugenr Mallove was a scientific reporter, not a research scientist.

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I would like to comment on a paper by Eugene Mallove, late editor of Infinite Energy magazine, and author of the 1991 book “Fire from Ice.” The paper, available at:

<http://www.infinite-energy.com/resources/iccf10.html>

was presented at the 10th International Cold Fusion Conference in August 2003:
His presentation was very different from papers offered by research scientists. Eugene, whose tragic death was a great shock to many of us, was a journalist, with scientific background, and an enthusiastic supporter of others’ unconventional ideas . As far as I know Eugene was not involved in laboratory research. His last paper summarized three such ideas: “cold fusion”, “hydrinos” and “vacuum energy.” Each of them is identified as an “emerging energy technology.”

At the conference, I told Eugene that it was not reasonable to say that something was an emerging technology when scientific principles on which it was based were in conflict with existing scientific knowledge, and when experimental data were not yet 100% reproducible. His review was very interesting but its impact was not as strong as it would have been if the author were a professional physicist or chemist. After reading the paper I decided to ignore “hydrinos” and “vacuum energy” because I was not at all convinced that “cold fusion,” in which I am interested, has anything to do with these topics. Some comments on hydrinos can be found in units # 57 and 89 while comments on vacuum energy can be found in unit # 162. Cold fusion is already sufficiently controversial; mixing it with other controversial topics makes it much less credible.

I do not mean to say that Eugene's paper is not worth reading. On the contrary, it is an excellent example of creative writing. Let me quote the abstract and the beginning of the first section. I suggest that names found in the abstract should be used as keywords in additional Internet explorations. Keep in mind that LENR stands for “Low Energy Nuclear Reactions,” a new name for cold fusion.

LENR and "Cold Fusion" Excess Heat:
Their Relation to Other Anomalous Microphysical Energy Experiments and Emerging New Energy Technologies

(Eugene F. Mallove, Sc.D.)

Abstract
During the past 15 years, indisputable experimental evidence has built up for substantial excess heat (far beyond ordinary chemical energy) and low-energy nuclear reaction phenomena in specialized heavy hydrogen and ordinary hydrogen-containing systems. [1] The primary theorists in the field that is properly designated Cold Fusion/LENR have

generally assumed that the excess heat phenomena is commensurate with nuclear ash (such as helium), whether already identified or presumed to be present but not yet found. That was an excellent initial hypothesis. However, the commensurate nuclear ash hypothesis has not been proved, and appears to be approximately correct in only a few experiments. During this same period, compelling evidence -- although not as *broadly* verified as data from cold fusion/LENR -- has also emerged for other microphysical sources of energy that were previously unexpected by accepted physics. The exemplar of this has been the "hydrino" physics work of Dr. Randell Mills and his colleagues at BlackLight Power Corporation, which was a radical outgrowth from the cold fusion field that emerged publicly in May 1991.[2] Even more far-reaching is the work in vacuum energy extraction pioneered by Dr. Paulo and Alexandra Correa, which first became public in 1996.[3] This vacuum energy experimentation began in the early 1980s and has been reduced to prototype technological devices, such as the patented PAGD TM (pulsed abnormal glow discharge) electric power generator, as well as many published experiments that can be performed in table-top fashion to verify the Correa "Aetherometry" (*non-luminiferous* or *non-electromagnetic* aether measurement science).[4] In an era when mainstream science and its media is all agog about "dark matter" and "dark energy" composing the vast bulk of the universe, there is a great need to reconcile, if possible, the significant bodies of evidence from these three major experimental and theoretical streams: cold fusion/LENR, hydrino physics, and Aetherometry. The aim of the present paper is to compare the substantial features of each field of investigation and to suggest how to move forward for the benefit of all with openness and a minimum of preconceptions.

1. www.infinite-energy.com and www.lenr-canr.org

2. www.blacklightpower.com

3. *Infinite Energy* , No.7, March/April 1996

4. www.aetherometry.com and www.aethera.org

Introduction

On the surface, all seems calm -- at least to the so-called Scientific Establishment, in other words the mainstream scientific media. The latter includes *prominently Science, Nature, Physical Review, Scientific American, American Scientist, The New York Times* "Science Times," and a host of other publications, which alternately ignore, mock, or misrepresent those scientific findings hard-won in laboratory experiments, such as are represented in the better papers that are presented at Cold Fusion/LENR conferences such as ICCF10. And, the major peer-reviewed publications in this Establishment do not accept papers on low-energy nuclear reactions and "cold fusion" phenomena -- these are not even given entrance (since the 1989-90 period) into the vaunted peer review process. This fact is neither admitted publicly by the obstructing publications, nor noted by the community of science journalists, who should be among the first to investigate and expose this blatantly anti-scientific publication practice.

To the Scientific Establishment all is calm because there are no phenomena from table-top experiments that are allowed to challenge the basic foundational physics paradigms, which have been laid down to become what can only be described as a church-like "holy writ." It is almost as though we are back in 1894 when the sentiment expressed in the above quote of Albert Michelson prevailed -- all is well with Physical Science and "further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice." That is essentially the dogma of mainstream physics circa 2003. Moreover, most scientists in the cold fusion/LENR field, whose experimental work is rejected by the mainstream, do not wish to challenge the foundations of physics either; they believe that cold fusion/LENR does not challenge those foundations at all and that their observations can be or will be explained by prevailing quantum mechanics and relativity theory.

The last sentence above is an excellent description of what makes Eugene's paper drastically different from papers presented by research scientists studying cold fusion. Those scientists try to understand strange experimental results in terms of accepted theories; Eugene, on the other hand, urges them not to be bound by the existing paradigm. Such advice does not seem to be reasonable when experimental results, obtained by highly trained scientists, are still not yet 100% reproducible. And something much better than 100% reproducibility is needed to challenge the legitimacy of

“quantum mechanics and relativity theory.” It will be up to theoretical physicists to decide whether or not accepted theories conflict with 100% reproducible experimental data.

Eugene was certainly aware that scientists and humanists belong to two different camps, with regard to methods of validation of various claims. His writing about science, in my opinion, is excellent because it appeals to both non-scientists and scientists. I like it because it focuses on social and philosophical aspects of cold fusion; it promotes communication between two cultures--scientists and people in other areas of intellectual activity. This paper, however, made me aware that some of his formulations, intended to promote “our cause” might actually hurt it. The demarcation line, defined in the last sentence of the above quote, should help us to see Mallove’s activities in proper perspective. I bow my head to the enthusiastic defender of cold fusion. The criminal hands that killed him ended a slowly developing line of new friendship. It is so unfortunate that his own reply to this item will never be seen. Let me end these comments with another qoute from Eugene’s introduction. A section describing our existing paradigms ends in the following poetic way:

Paradigm 7: The Second Law of Thermodynamics can never be violated in macroscopic systems. One cannot make a “Perpetual Motion Machine of the Second Kind” that would convert ambient thermal energy to useful work, with no heat rejection into a lower-temperature reservoir.

The foregoing is a highly restrictive set of dogmas within which scientists are expected to conduct their work. There can be no doubt that these are the intellectual walls that the Scientific Establishment has erected. True enough, a huge amount has been learned about Nature within the confines of these paradigm restrictions, and much technological progress has occurred too -- but there is so, so much more to the universe and to what human beings surely will be able to do and become if they could be liberated from those restrictions! . . . I respectfully disagree with [this] conference chairman Professor Hagelstein that *all* heat engines will be forevermore fundamentally Carnot-limited.

Once again Eugene draws a clear line between himself and an active cold fusion scientist from MIT. Eugene does not claim to be a researcher, he sees himself as advisor and philosopher. Perhaps somebody who knew him better will elaborate on my quick observations. I would be happy to append additional comments about “two cultures,” in cold fusion. (The “Two cultures,” by the way, was the title of a book by C.P. Snow.; the book was published long before the cold fusion controversy.)

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167) A lot of noise about a speculation

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In browsing the Internet I found this document:

http://www.aetherometry.com/wilson_response.html

It is "An Open Letter to Mr. J. Wilson of Popular Mechanics" from Dr. Paulo Correa. Mr J. Wilson is a senior editor of Popular Mechanics, a journal in which a very short article on cold fusion was published recently (August 2004). Correa, accuses Wilson of turning a legitimate topic of general interest into a science fiction story about homemade hydrogen bombs.

Correa writes: ". . . What you have produced is pure sensationalistic hype - worthy of a second-rate rag. You mixed ill-digested facts with false allegations that you attributed to Dr. Mallove, in order to cook the eye-catching stew of mass-hysteria and paranoia that the cover so successfully conveys. You saw fit, in this age of rampant terrorism, to tell to your readers a scary bed-side tale - instead of doing your homework as a Science Editor. You preferred to concoct a science-fiction story of an imaginary horror, rather than educate your readers about the physics and potentialities of LENR. You knew it would sell like hot cakes, and so, overnight, 'cold fusion', which only yesterday was an affair of crackpots and was being suppressed for a falsely attributed 'lack of merit', became so meritorious and so dangerous as to deserve to become an affair of State, Army and Intelligence. ....."

Who is Dr. Correa? His name appears in Mallowe's abstract (see unit #166 at my website). Here is how his work is described by Jeanne Manning in:

http://www.earthpulse.com/science/zeropoint.html

"A Canadian couple also say they have made a breakthrough in non-polluting power generation with an invention that delivers more power than it consumes by using pulsed cold plasmas. Dr. Paulo Correa and Alexandra Correa kept the process a secret until recently, when three US patents were issued." In the same piece Manning observes: "Free energy, electricity from space, zero-point energy from the quantum fluctuations of the vacuum of space... Whatever the name of the previously-unknown source of energy that is seemingly tapped by an emerging energy technology, the proof for its existence is multiplying exponentially." In another Internet reference Paulo Correa is described as "Partner and Director of Research at Labofex -- Experimental and Applied Plasma Physics of Concord, Ontario." He is said to be an inventor of a highly desirable process. "Unlike the cold fusion process, which claims to output low grade heat, the Correa technology directly generates electricity at power voltage levels, without any utilization of cold or thermonuclear fusion principles. Another important feature of the apparatus is that it employs no radioactive compounds and generates no nuclear radiation or radioisotopes. The energy system is entirely pollution-free, self-contained and composed of readily recyclable materials."

Intrigued by Correa's "Open letter", I went to the library and read the Popular Science article. It seems that Correa makes a lot of noise about nothing. The short article is a speculation that cold fusion can possibly be used to produce large amounts of tritium for hydrogen bombs. Correa dismisses this as a "scary bed-side tale." In my opinion that speculation is not less realistic that a speculation about economically significant energy from cold fusion, as envisioned

by Mallove. Most cold fusion researchers know that tritium is not the main byproduct of cold fusion. Sending a beam of deuterons (from an easy to construct electrostatic accelerator) into heavy water would probably generate much more tritium, in one or two days, than all cold fusion reactions so far.

But the fact that this radioactive isotope of hydrogen has been detected in many laboratories is significant; it indicates that a highly unusual nuclear process was occuring in some cells. This observation was made in the same year in which the discovery of cold fusion was announced. But it significance was often dismissed on the ground of possible contaminations. I hope that claims of generation of tritium and helium will be taken seriously by scientists appointed by DOE to reinvestigate cold fusion. Nobody knows what kind of practical applications, if any, will emerge after the reality of cold fusion is recognized.

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168) An essay by Mark Hugo

Preface

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1) **In an e-mail message, received** three days ago, Mark Hugo wrote: “I've taken the liberty of attaching a 26 page pdf which details results from a lab in Kiev, Ukraine. Whether or not "Cold Fusion" is real, Dr. Adamenko, a long time stellar THEORIST was stimulated by the results of P&F and others to rethink his beliefs and theories on stellar action during the '90's. As a result of this rethinking, and because of some odd spark of genius, from 2000 onward, he has been experimenting with "coherent electron bunches" and their impact on "heavy elements". As you will see in reviewing the attached booklet.pdf (taken from www.proton21.com.ua) the result of these interactions is not only massive nuclear transmutations for energies under 10,000 eV per transmutation (miracle #1), but also the formation of easily measurable (by Mass Spec) amounts of Super Heavy Elements. (Shall we say miracle number 2.) All in all, there is a stunning amount of evidence here of phenomenon which needs careful attention to be paid to it.”

2) **In my reply, sent last night (8/12/04),** I wrote that, after reading Adamenko and Vysotskii paper, presented at the ICCF10, I do not think that the reported experiments belong to the area of cold fusion. A very short pulse of electrons, delivering 1 kJ of energy to a tiny spot (10 to 100 microns) on a target, might be responsible for a very high momentary temperature. Looks like a hot plasma setup to me. The experimental results are sensational but I am in no position to judge them. They performed 5000 experiments. That is very impressive. I also asked Mark if he could write a short summary of Adamenko’s work for a typical physics teacher, for example in a high school or college. I would be happy to post such paper on my web site.

3) **This morning Mark Hugo wrote:** “. . . Now one other thing, there is much MORE than 10^{18} atoms which receive the 1 KJoule of input energy. Probably more like 10^{21} atoms. Thus the actual energy input per atom in the target samples is closer to 10 eV. At this level we can definitely say we are in the "chemical energy" range. There are NO nuclear events which should be occurring, PERIOD. (But the experimental evidence is that they ARE occurring.) I'll take some time this weekend and give you a write up in a form that college level and high school level physics students can understand.

With 20 years of experience in nuclear power ... plus my current position as a ‘Materials Scientist’ for Hutchinson Technology, I AM in a position to judge the experimental work. Doing experimental work with materials is my "bread and butter". So I HAVE had experience with Xref, Auger, ESCA and Mass Spec many times over the last 20 years. WITH this experimental background I can say this: UNLESS ADEMENKO IS FRAUDULENTLY FABRICATING his experimental data, his data backs up all his claims. (I.e., nuclear transmutations at ‘cold’ conditions, and generation of super heavy nuclei ... using a method which bears NO relation to ‘high energy’ physics. That's where we need to start!”

4) I am still waiting for Mark’s essey (8/17/04). I strated looking for information about Adamenko over the Internet. His private Ukrainian lab (EDL, in Kiev), has a web page: <http://www.proton21.com.ua/index_en.html>; it is worth reading. It sais that the project is “based on an innovative and original conception on initiating extreme conditions for nucleosynthesis process ignition in a super-dense cold substance.” At the February 2004 workshop in Germany Adamenko said: “Briefly described is the author’s conception of shock coherent collective cascaded cumulative mechanism of particles acceleration through initiating the self-developing collapse of a converging solitary shell-wave of extreme density of energy and substance. Also provided is some part of experimental data obtained in course of

practical implementation of that concept. The paper is aimed at preliminary discussion of the composition and content of collected works describing the experimental and theoretical results obtained in the Electrodynamics Laboratory of Proton-21 company in Kiev, Ukraine. This work has been carried out within the commercial project called Luch, which is developed on our initiative and aims at the creation of new, efficient and environmentally safe nuclear technologies for neutralizing the radioactivity and synthesizing stable isotopes of chemical elements, including superheavy ones."

Luch, by the way, is a private research laboratory in Russia (near Moscow). Karabut, whose work was described in unit #13, works in that laboratory. Once again I am impressed by highly unusual isotopic ratios among the reaction products. Here is an illustration:

^{90}Zr --> 6% abundance in reaction products versus 51% in nature
^{92}Zr --> 31% abundance in reaction products versus 17% in nature
^{96}Zr --> 39% in reaction products versus 2% in nature

It appears that their compression process, which I do not understand, produces less proton-rich nuclides and less neutron-rich nuclides than found in nature. Will this experimental fact be confirmed by other qualified scientists? Showing that something highly unusual is taking place is always very important, even if the process cannot yet be comprehended. I am puzzled by the phrase "shock coherent collective cascaded cumulative mechanism of particles acceleration." I suppose that "coherent electrons" are electrons hitting the target at essentially the same time. What else can it mean in this context? The term "driver" probably refers to an electron accelerator with a photo-electric cathode. That cathode is bombarded by a pulsating laser beam. This produces bunches of electrons that are accelerated together. I happen to be know people working with such machines in the US.

What impressed me most was formation of transuranium elements. Contamination by impurities can not possibly be responsible for atomic nuclei characterized by atomic masses above 250. Such nuclei have never been found in our natural environment. At a recent workshop in Bonn (Germany, February, 2004) the authors reported the synthesis of nucei with atomic masses 253, 264, 278, 280, 294,395, 433 and 434. That is what Mark Hugo calls Miracle #2. I wander if Darmstadt heavy ions physicists were present at this workshop and how they reacted to the report. They, and scientists from Dubna (Russia), are experts in syntesizing superheavy nuclei. (These experts, by the way, use the term "cold fusion" to describe rare collisions of two atomic nuclei at sub-coulomb kinetic energies. The process in which superheavy nuclei are presumably synthesized by Adamenko et. al., seems to be very different.)

4) **P.S. (8/16/04)**
While waiting for Mark's essay I found something puzzling. By going to Ademenko's website, <http://www.proton21.com.ua/about.html> one can ask for the description of the laboratory in two languages: Russian or English. Reading it in Russian I see the phrase "perierozhdienije jadernovo veschestwa." This translates into "rebirth of nuclear matter." I think that the term "reorganization of nuclear matter" would be more appropriate, unless "zero point energy" is involved. Do they think that nuclear matter disappears for a moment and then reappears in new form? To see how the "perierozdhienije" was translated I clicked on the "English" button. But the English text is not the same as the Russian text. Why is it so? (In reading their ICCF10 report again I saw that "perierozhdienije jadernovo veschestwa" was translated as "nuclear regeneration." I was not familiar with this term.)

Here is my translation of the above Russian document.
"The laboratort of electrodynamic studies (EDL) was created in 1999, to implement new and save technology of processing radioactive waste. The method is based on the author's hypothesis concerning a possibilty of simulating a previously unknown natural process. That self-organizing process, the avalanche-like collapsing (concenteation of energy) in condense matter, can possibly result in complete rebirth of nuclear matter. First experimental confirmation of that idea, by using a specially constructed setup, occurred on Ferbruary 24, 2000. It was a demonstration of the electro-nuclear collapsing to extreme (microscopic and quasi-point-like) densities.

Several thousand successfull experiments were conducted between 1999 and 2003. Acting on soid state targets, we observed their specifically explosive destruction accompanied by emission of transformed (artificially synthesized) matter. Artificial generation of matter, generated in explosive destruction of targets, was confirmed through more than

15,000 analytical experiments, using the most modern equipment, conducted in other laboratories, not only in Ukraine, but also in Russia and USA. We can definitely conclude that:

1) Experimentally effective technology was created and confirmed. It provides conditions under which new kind of nuclear reactions (collective multi-particle processes in condensed matter) become possible for the first time in world's history.

2) An experimental setup was created to initiate energy-efficient and highly productive process of artificial synthesis of nuclear matter.

3) Products of artificial synthesis contain no radioactive isotopes.

4) In working with radioactive targets we definitively observed the decrease of radioactivity resulting from the complete nuclear rebirth in the parts of material after highly-energetic processing.

5) Products of artificial synthesis contain long-lived superheavy atoms of unknown chemical elements (at the border and outside the border of the periodic chart). The products are synthesized at rates which are many order of magnitude higher than in classical, less energy-effective methods."

Here is the text displayed after the "English" button is preassed on that page:
"Advancements over the span of the last fifty years in many fields of scientific and technological research such as Genetics, Physics, Telecommunications and other fields has outperformed progress in the field of Power Generation and Decontamination of Radioactive wastes. Progress in the fields of controlled thermonuclear synthesis and radioactive wastes decontamination technology also lag in comparison despite investments in research by the developed nations exceeding USD 100 billion. One key issue that remains unresolved to this date in this particular field of research is the development of processes and technology for controlled ignition of self-sustaining nuclear reactions. For this, an adequate "initiator" of such controlled nuclear transformations is required; one which will result in a sustainable and controlled energy output and the transmutation of radioactive atoms into stable ones.

ElectroDynamics Lab (EDL) was founded in Kiev, Ukraine in 1999 by a group of Ukrainian engineers and scientists to address the specific problem of the adequate initiator. EDL's primary mission statement was to develop a novel, safe and effective technology for radioactive wastes decontamination. Today, privately funded EDL has evolved into a leading edge Research and Development center employing in excess of 120 researchers and scientists. The proven results of its research and its proprietary process, currently being patented, are able today to address the unresolved issue of nuclear wastes transformation. EDL's results are revolutionary in their nature and are leading to important commercial and industrial breakthrough applications."

I will assume that the Russian version is more recent because it gives more details. On the other hand, it may be the other way around; perhaps they no longer believe in the rebirth theory. This does not make any difference to me; at present I am interested in strange experimental results, not in theories.

4) **P.S. (8/22/04)**
Still waiting, I went back to <http://www.proton21.com.ua/index_en.html> and clicked on the "our news" button. In the window that opened I noticed some new pdf files. The first that I downloaded was Decay_IMS3f_en.pdf. It shows that alpha particles of up to 430 MeV were observed (November 23, 2003) with a Si detector from a foil that was bombarded with 12.5 keV oxigen ions. This experimental results is not less shocking than their observation of stable transuranium nuclides.

The second downloaded file, Decay_LMS_en.pdf, shows that alpha particles were onserved with CR-39 when the foil was bombarded by red light from a laser (delivering 50 mJ/14 ns pulses to spots of target areas whose diameters were ~0.5 mm). This translates into about 3 million watts. As I said before, this is not a typical cold fusion wattage. But results are impressive. On the exposed CR-39 surface they found 149 alpha tracks (versus only 18 tracks on the other surface.

I also downloaded files containing two reports (dated April27, 2004) on methdology and results from recent experiments. I will read these papers later.

5) **Mark's essey was received** this morning (8/23/04). He wrote explained the delay and added an observation. "There is one little ray of hope for Dr. Adamenko's work. Since he started in the '80's and '90's as a 'Stellar Theorist', and was widely published and acknowledged, he has an 'in' with that 'community'. As such, he has been quietly, but deliberately showing up at a variety of 'Astro Physics' and Stellar Physics conferences and presenting his results. To date (August, 2004) however, interestingly THERE IS NO DISCUSSION OF HIS RESULTS available ANYWHERE PUBLICALLY. Only his OWN publications. [Another example of] the "benign neglect?" WHY there is no discussion?"

I do not know how to answer this question. To promote discussion I would select one easily reproducible experiment and would focus on it in all publications and talks. It is not easy to think critically about results of 5,000 experiments. And I would publish a complete description of the so-called "coherent driver" of electrons used to generate nuclear transmutations. A discussion would occur spontaneousely if essential instruments were available to perform experiments in other places.

Simulating nucleosynthesis on earth?

Mark Hugo, August 23, 2004
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Since 1999 experiments have been performed at the "Electrodynamics" lab outside of Kiev, Ukraine using "coherent electron bunches" to stimulate nuclear transformations in "heavy elements". To understand the significance of these results a brief review of some basic nuclear physics is in order. According to a reference on nuclear technology: "In 1932 Cockcroft and Walton produced the first nuclear reaction using artificially accelerated particles, bombarding and disintegrating lithium nuclei with protons accelerated to several hundred keV. By opening and closing switches in proper sequence they could build up a potential of 800 kilovolts. (Thus an 800,000 eV particle). This gave the potential to accelerate protons down an evacuated tube eight feet long. They then put a lithium target at the end of the tube and found that protons disintegrated a lithium nucleus into two alpha particles. "

Actually this is not quite correct, as the first "nuclear reaction" observed was found in 1919 by Ernst Rutherford, using collisions with naturally occurring alpha emitters. (4,000,000 eV). Nevertheless this gives us some insight into the energy levels involved in nuclear science. Starting with the Cockroft and Walton work (at 800,000 eV and moving upward) fundamentally all reactions involving re-arranging the nucleus fall into the realm of 1 million electron volts and above. This has to do with the fact that the "binding energy" between a proton and a neutron in a nucleus is on the order of 1 to 2 million eV, and has to be overcome to re-arrange a nucleus.

There are two "exceptions" to this rule, however. One is the "nuclear reactions" which a produced by adding a neutron to a heavy nucleus (200 plus atomic mass units.) This sort of reaction can involve very LOW particle energy, but demands a source of NEUTRONS to allow it to happen. Also, all known neutron stimulated nuclear reactions (referred to as "fissions") result in highly "energized" end products, which are colloquially referred to as "nuclear waste". (I.e., they are in an unstable nuclear state and continue emitting gamma, beta radiation and alpha particles after they are formed.)

The other "exception" is the nuclear reaction of "fusing" two light nuclei (<12 AMU) together. Technically this can happen with energies of just above 10,000 eV. However the "target cross section" of these reactions is so small as to make their probability very low for happening in conventional particle beam devices at energies that low. Now we come

to the work by Adamenko. Here is an example of a "heavy element" sample from Adamenko's work before it is exposed to a bunch of electrons delivering 1 Kilo-joule of energy.

Note that this is a 99.99 % pure ball of copper. After a 1 kilo-joule coherent electron blast the following is obtained:

You will notice more than a dozen elements in this analysis. This sample is on the original Cu "plate" that the original Cu "ball" was on. Careful analysis (using standard methods, EDAX, XPS, combined with Mass Spec work) by Dr. Adamenko has indicated that 10^{18} atoms have undergone "nuclear transmutations" in these samples. The highly unusual nature of these transformations becomes clear when the amount of energy per reaction is calculated. One kJ translates into 6.2 X 10^{21} eV. Therefore, only $6.2*10^{21}/10^{18}$ = 6,200 eV of energy is used, on the average, per per nuclear event. This is about 1600 times less than a typical binding energy of 10 MeV. of Dr. Adamenko. But this is not the only "anomalous" result reported by Ukrainian scientists. Another highly unusual result is detection of transuranium elements. Transuranium elements discovered by Dr. Adamenko appeared in large quantities (sufficiently large to be observed with a mass spectrometer). Furthermore, they did not decay rapidly. This can be contrasted with what is known about transuranium elements synthesized in nuclear reactions induced by heavy ions. In these reactions the amount of energy per event is higher than 100, 000, 000 eV per event.

The mass spectrometer output below has a peak due to the element whose atomic number is 115. It was produced when copper was bombarded by electrons.

The peak in the 328 to 330 mass would be the "Element 115" trace. The 338 to 433 peaks are due to elements with even higher atomic numbers. The Cs_3C_2 and Pb_2O shown on the second mass spec are there for reference. They are the only known compounds that would fit in the realms shown. The nature of the "mass spec" work is that it would tend to break the chemical bonds of the Cs/C or Pb/O combinations and resolve only the Cs, C, Pb or O individually, so it is UNLIKELY that even if the compounds existed they would show up in the mass spec results as combined.

Now without belaboring detail, the only way that mass spec signals in these realms could be detected would be with numbers such as 10^{13} or 10^{14} atoms or above in "real" terms. Thus not only is the energy levels which Dr. Adamenko making "Super Heavy Elements" divergent from the "High Energy Physics" community by a factor of 30,000 but also the "end production result" is off from the H.E.P. groups by 10^{12}. This means, of course, that there is an insanely huge gulf (right now) between the standard "high energy physics" community and the work being done by Dr. Adamenko. How do other scientists deal with this difficulty?

There are usually two approaches I have seen to this sort of situation. Number one, is …as in Pons and Fleischmann's work, to be tremendously demanding and take advantage of "perceived weakness" in the experimental work, and to pick it apart. There is one other method of response to such "revolutions" I have also found. That is what is called "benign neglect". (Or the , "ignore it and it will go away" or the "PAY NO ATTENTION THE MAN BEHIND THE CURTAIN" response.) A good example of the latter is the response that Dr. George Miley of the U. of IL has gotten to his I.E.C. fusion results. (Inertial Electrostatic Confinement Device). Miley's IEC devices are WILDLY more successful (although they cannot generate NET energy output, not yet..) than the 500,000,000 Dollar Princeton Tokomak (R.I.P.) or the equally expensive Lawrence Livermore "Shriva" laser pellet fusion device(s).

Miley spent $20,000 and 3 graduate student's time (for a year) building his I.E.C. device. But does one find the "Fusion research community" beating a path to Dr. Miley's door? NO! In point of fact, because Dr. Miley was worked on "Cold Fusion" devices, he has been pilloried as a "quaint crank", who is getting "funny" as he ages. Fortunately, George is the type of personality that doesn't bow to "external" pressure, and he continues on with his interesting and progressive work. (Although with only a FRACTION of the funding the "give us another BILLION, and in 10 years nuclear Fusion will only be 10 years away" crowd.)

Getting back to Dr. Adamenko, there is one little ray of hope for his work. Since Dr. Adamenko started in the ‘80’s and ‘90’s as a “Stellar Theorist”, and was widely published and acknowledged, he has an “in” with that “community”. As such, he has been quietly, but deliberately showing up at a variety of “Astrophysics” and Stellar Physics conferences and presenting his results. To date (August, 2004) however, interestingly THERE IS NO DISCUSSION OF HIS RESULTS available ANYWHERE PUBLICALLY. Only his OWN publications.

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169) Laser-activated excess power

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Looking for news about cold fusion (with Google) I found “The New Light on LENR” by Jones Beene: It can be seen at:

http://www.zpenergy.com/modules.php?name=News&file=article&sid=887

This recent piece(8/17/04) is essentially a reflection on the paper of Letts and Cravens: "Laser Stimulation Of Deuterated Palladium: Past And Present." That paper was presented at the Tenth International Conference on Cold Fusion last year. It can be downloaded over the Internet as:

http://www.lenr-canr.org/acrobat/LettsDlaserstimu.pdf

Beeme thinks that it is “the most important paper in LENR in 15 years.” This strong endorsment forced me read the paper again. I agree that the results are interesting but I see no evidence that they have anything to do with LENR (Low Energy Nuclear Reactions). I have no idea what is responsible for generation of excess heat; why should I assume that it is due to nuclear reactions described by Beene? In trying to answer this question I found an interesting “appeal to researchers” posted by D. Cravens and D. Letts today <http://www.cerg.org/research/publications>

The author of that document states that “demonstrable transmutations in suitably prepared metals have been reported recently under ‘low energy’ laser stimulation.” That is a different story; the term transmutations is commonly used as a reference to nuclear reactions. I agree with the author that a 30 mW laser beam, uniformly distributed over the area whose diameter is 2 mm, can not possibly generate stellar temperatures. But I would like to know more about the recent “demonstrable transmutations.” In their appeal Dennis Letts (from Texas) and Dennis Cravens (from New Mexico) offer other scientists a possibility to study their samples. Will presence of products of nuclear reactions be confirmed? I responded to the appeal offering an investigation with CR-39 detectors. This was a week ago. The reply came last night; I will have a chance to examine their cathode #163. Waiting for that cathode I will now start summarizing findings the Letts and Cravens.

The “appeal to researchers,” at the above URL, contains links to several pdf files with “related publications.” I downloaded the first of them: “Practical Techniques in CF Research – Triggering Methods.” This paper was presented by Cravens and Letts at the last International Conference on Cold Fusion (ICCF10). It can be downloaded from <www.lenr-canr.com> . the document shows that the authors have been working of the subject for 14 years and performed “thousands of experiments.” Referring to the irreproducibility they wrote: “Electrolytic cells using bulk palladium often require loading times of 10 to 20 times longer than would be expected by diffusion times of deuterium within the metal before they be expected to produce excess heat (1). This was likely the cause of failure of early researchers who rushed to replicate Fleischmann’s and Pons’ early work (2). In the first few years after the announcement, it was easier for a researcher to rush to print and claim negative results than to patiently wait until the system was fully loaded and driven into internal transitions that drive the reactions. As a result, early work more often than not failed to see excess heat.”

The introduction contains another interesting observation. Referring to Schwinger, and to more recent theoreticians, the authors wrote: “Most simple theoretical models fail to predict that nuclear reactions within a deuterated metal lattice can

take place at significant rates. Such models rely on reaction rates that are based on equilibrium placement of deuterium within a metal lattice or on wave functions based on such placements. In particle models, the global average of the deuterium density within the metal is on the order of an Angstrom or more even for extreme loading ratios of D/Pd. It is clear that deuterium at such remote nuclear separations would not be expected to lead to nuclear events.

The imposition of dynamic conditions can cause the local separations of deuterium to be significantly different from the value predicted by the global density alone. It also seems that dynamic conditions provid[ing] ways for coupling of energy to drive the reactions and impurities within the lattice can allow for spin exchanges required for spin selection rules. It is a surety that the energy required to drive any nuclear events and energy released from such events are much larger than any external energy available to the deuterium based on a per atom division of energy (3). This means that any external energy driving the possible nuclear events must act in a coherent way to channel energy from a large region of many atoms to the active sites (4,5,6).

This coherent channeling must involve over 10^8 atoms and likely many more. The experimental conditions then must make use of non-equilibrium events acting on a system that has some group coherent nature. The methods described here are simple and practical methods that can be used to produce such dynamic conditions, which may lead to the desired nuclear events. The assumption here is that the reactive nuclear species must be driven to a dynamic active state before the desire events can produce excess energy within the system."

This would be a good reply to those who formed their opinion about cold fusion in 1989 and 1990. But general observations of that kind do not substitute for identifying conditions under which unexpected nuclear reactions are reproducible. I have no doubt that quantitative theoretical models will be found, sooner or later, after cold fusion phenomena become 100% reproducible. Why should situations in cold fusion be different from situations in other areas of science? Let me now focus on another paper that Letts and Cravens presented at the same conference. That paper, entitled "Laser Stimulation Of Deuterated Palladium: Past And Present," can be also be downloaded from< www.lent-canr.org> . The authors discovered that laser light can trigger generation of excess power in suitably prepared materials. Here is the abstract of their paper:

"A method is disclosed to fabricate a Palladium cathode that can be electrolyzed in heavy water and stimulated with a laser at a predetermined wavelength to produce apparent excess power; the fabrication method involves cold working, polishing, etching and annealing the Palladium prior to electrolytic loading with Deuterium. Loading is accomplished with the cathode sitting in a magnetic field of 350 Gauss. After loading the cathode with Deuterium, Gold is co-deposited electrolytically on the cathode. When a coating of Gold is visible on the cathode, co-deposition is halted and the cathode is stimulated with a low-power laser with a maximum power of 30 milliwatts. The thermal response of the cathode is typically 500 mW with maximum output observed of approximately 1 watt. The effect is repeatable when protocols are followed and has been demonstrated in several laboratories."

The constant current and voltage, used by the authors, are typically 1A and 5V. They wait till the Pd cathode is loaded with deuterium and measure the temperature of the electrolyte after thermal equilibrium is established. The relationship between the input power (in this case 5000 mW) and the temperature is known from the "isoperibolic calibration" of the electolytic cell. The loaded cathode is then exposed to a laser beam, delivering only 30 mW of power, and the new, slightly higher equilibrium temperature is established. The observed temperature change, however, does not correspond to the input power of 30 mW, it corresponds to the power of, for example, 5500 mW. Excess power, for this numeric illustration, would be 470 mW. In other words, 5030 mW input (electric+light) and 5530 mW output (thermal). The ouput is nearly 10% hogher than the input. The excess energy, Q = 2.7*t=X joules (where t is the laser-exposure time is seconds) is very small but it is probably larger than in Galvani's experiments with frog's legs.

What is the origin of excess power? Without having any evidence that it comes from nuclear reactions I tend to suspect that it comes chemical reactions of some kind. Perhaps chemical energy was somehow stored in the cell during the period of loading palladium with deuterium and subsequently released under laser irradiation. I know that this kind of guessing in not original; speculations of that kind were advanced as soon as the discovery of cold fusion was announced. The authors specify that to load the cathode with deuterium the current of 0.1 A was flowing through the cell for 100 hours. After that the current was increased to 1 A for 24 hours. What evidence do the authors have that a

fraction of inputed energy could not possibly be accumulated, like in a car battery, before the laser beam was turned on?

I am not a chemist and I will not speculate about reactions that can conceivably be involved in the storage of energy. I would like to see the energy balance for the entire experiment. Was the average input power during the entire experiment, for example, in 100 +24 +3 = 127 hours, significantly smaller than the average output power for the same time interval? A positive answer to this difficult question, and absence of large experimental errors, would convince me that the energy storage during the loading of deuterium did not occur. On the other hand, as I indicated above, presence of nuclear reaction products, or nuclear particles, would by itself be sufficient to demonstrate that at least a fraction of excess power was nuclear. The situation seems to be the same as it was in 1989, except that the effect is said to be highly reproducible, at very low wattage levels. Is it a nuclear effect or is it a chemical effect? That is the main question.

In the section entitled "Results" the authors wrote: "It has been observed that a common red laser (30mW) when tuned to specific wavelengths appears to trigger a cathodic exothermic reaction 5-30 times greater than the magnitude of its radiant power output. The effect has not disappeared or diminished as calorimetric quality improved. It has also been observed that the magnitude of the thermal response of the cell may be altered by the polarization of the laser beam with respect to an external magnetic field." They do not speculate on the "nature of the cathodic exothermic reaction." They do not say that it might be nuclear. But the phrase "nuclear events" does appear in their papers. I will assume they think that the reactions are nuclear. The papers were presented at the cold fusion conference and "cold fusion" is often described as a new kind of nuclear reaction.

I suppose that better "calorimetric quality" refer to more accurate calorimetry. The numbers 5-30, on the other hand, refer to ratios of excess power over the laser beam power. These numbers show that small powers trigger releases of much larger powers. Let me turn to two other observations made in the paper: the effect of the magnetic field on the loading of deuterium into palladium, and the effect of polarization of laser light (with respect to the magnetic field) on excess power. In the section entitled "Experimental" that authors wrote: " We think the magnetic field improves the loading of deuterium." I suppose that the phrase "improves the loading" is meaningful to a chemist. The magnetic field strength, at the cathode submerged in the electrolyte, was 350 Gs. This is only several times larger than the terrestrial field. How can loading of deuterium be influenced by such field? How did the D/Pd ratio differ between loading with and without the magnetic field? These questions alone can become subject of a doctoral dissertation.

The same is probably true for the effect of light polarization. The section devoted to this subject begins with the following statement: "During the course of experimentation it was discovered that polarization of the laser beam can dramatically affect the thermal response of the cathode to the laser beam. Cravens observed during one of our runs that when the laser beam polarization is perpendicular to an external magnetic field, the thermal response of the cathode is maximized." I suppose that both authors observed the effect in subsequent experiments; a single observation of something unusual would not be trustworthy. The effect of polarization is described in the caption of Figures 10. It is difficult to understand this effect without first understanding the effect of the magnetic field on triggering excess energy. (It seems that the magnetic field plays a role not only in loading but also in what happens after loading.) Accepting experimental data, as reported on Figure 10, one has to face a difficult task of understanding what was going on.

Figure 11 introduces another complicating factor. "The cathode didn't respond well to the laser stimulation until the rare earth solution was added to the electrolyte. With the additive, the cathode became responsive to a laser operating at 657nm with a radiant output of 30 mW." The nature of that additive is not specified in the paper. In the caption of that figure the authors state that the "power to the cell was constant at 7 +/- .01 watts." That shows that the excess power was, at the highest point, equal to 10% of the input power. In the remaining part of this section the authors state that the effect was witnessed by four invited scientists. I do not think that this refers to the effect of polarization. It probably refers to the effect red light on the excess power (at constant polarization). For how long would the excess power be generated, after 2100 hours, if the laser beam was not turned of? My guess is that chemical reactions taking place in the cathode eventually destroy it. Is it possible that the observed excess heat is due to these reactions? Were the effects of polarized light on the yields of chemical reactions studied in other contexts? I do not know how to answer such questions.

The most recent paper of Letts and Cravens, as far as I know, was presented at the ASTI Conference in Italy (19-21 March 2004). The link to that document is available in

http://www.cerg.org/research/publications/index.html

The abstract is essentially the same as in the ICFF10 paper. Most of what I see in the last paper has already been communicated in the two papers to which I referred above. Graphical illustrations of seventeen cathode-preparation steps (previously described in words only) will certainly be appreciated by those who are interested in studying the unexplained Letts-Cravens effects. Some statements, however, are new. The authors wrote, "We did a series of experiments that replaced the LiOD with LiOH. It was clear that the LiOH did not yield any excess [energy]." That is a very significant experimental fact. They also wrote: "There seems to be a relationship between the temperature of the metal host lattice and the wavelength needed to trigger the effect..... There are runs where we see no excess [energy] until the correct wavelength [from a tunable laser] is reached. It is very reassuring to see excess heat response when the wavelength of the laser is altered by only a few nm. It is also hard to conceive of a calorimetric error that is so wavelength sensitive." I agree with this observation.

Referring to the effect of magnetic field on excess heat, and to the effect of polarization, the authors wrote: "Our better experiments have been with cathodes that were initially loaded in the presence of an external field. Often the excess heat is dependent on the relative angles between the linear polarized lasers and the direction of the magnetic field applied when the metal was first loaded. Again, it is hard to conceive of calorimetric errors that would change with the rotation of a laser outside the controlled environment or the insertion of a quarter wave optical plate in the beam. The maximum effect coincides with the E field of the polarized laser being perpendicular to the magnet field." This implies some kind of magnetically induced alignment on the cathode during its loading with deuterium. Was the magnet, located outside the electrolytic cell, removed after loading? I assume it was. The word "often" is reassuring; it shows that the effect of laser light polarization on the excess power was not a single observation.

The issue of nuclear versus non-nuclear origin of excess power was not addressed in this paper, except indirectly by telling us what happens when the LiOD electrolyte was replaced by LiOH electrolyte. Isotopic effect on chemical reactions, if any, are extremely small. Another issue that was not addressed has to do with duration of excess power runs. I would like to know, for example, why the laser had to be shut off after about 150 minutes in the experiment #560 (or after 3900 minutes in the experiment #587). I would also would like to know which "hidden variable" was responsible for the fluctuations of excess power (between 100 mW and 800 mW) in the experiment #587. The laser beam was presumably stable during the entire 5900 minutes interval.

I have no doubt that the authors are also interested in "hidden variables." Hopefully their more recent experiments will lead to better understanding of the cause-and-effect relations in this fascinating area of science. Asking questions is easy, answering them is much more difficult. Identification of hidden variables has been the major preoccupation of scientists involved in cold fusion experiments. The progress would be more rapid if the area was not declared to be pseudoscientific. Methods of validation used by individual researcher must always be questioned; condemning that entire field as pseudoscientific is not justifiable, in my opinion. Papers like those of Cravens and Letts should not be rejected by editors of mainstream journals; they should be allowed to undergo the usual pear review process. Financial support of research should be allocated to cold fusion researchers in the same way as in any other area of science. Will the situation change after the ongoing DOE investigation is over? I hope so.

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170) Kanarev and Naudin

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Here is the e-mail message received from Stefan Sundström in Sweden today: “Mr Kowalski: In your seeking for information on cold fusion, I recommend you to study the work of Mr. Kanarev. You can find and download his article and books on this site <http://kanarev.innoplaza.net/> I also recommend you to take a lock at the European patent data base . . .” The link brought P.M.Kanarev’s document entitled “Prof. Kanarev's View Point on the Energy Problem: WATER IS THE MAIN POWER CARRIER OF FUTURE POWER ENGINEERING.” The author seems to be a theoretical physicist from the Kuban State Agrarian University in Krasnodar, Russia. In the reply to Stefan I wrote: “I do not consider myself competent enough to deal scientifically with ‘the axiom of space - matter - time unity.’ But if you send me a short simplified essay on that subject, at the level accessible to high school physics teachers, I might post it on my web site.” I also asked about Stefan’s background and and about one particular claim made by Kanarev.

I suspect that the phrase “power carrier,” in the title of the article, is probably a poor translation of a Russian term for the “energy resource.” I am sure the author knows the difference between power and energy; he certainly does not think that power can be carried by a substance. By the way, it is not the first time that I see how a foreign author puts himself into unfavorable situation by posting a poorly translated article. Translational errors would most likely be corrected in a peer-reviewed article, published in a scientific journal. Quick publishing over the Internet is far from being perfect; it has many negative “side effects.”

The above was written in anticipation of an essay from Stefan. But the reply received today (9/3/04) did not contain an essay. He wrote: “I am not a scientist; I am a open minded family man. If we learn what energy is then we could tap on to something new, and do something better for our children.” In other words, he is layman aware of negative aspects of utilization of available energy resources. I understand frustration of people like him. He is desperate to hear about better alternatives.

His message contained the URL for the laboratory of Jean-Louis Naudin (in Paris) and a suggestion that students should study the description of French experiments on excess heat. I did correspond with Naudin last year but for some reason he stopped responding to my questions. I expected him to publish the fascinating results at the last International Conference on Cold Fusion (ICCF10) but he did not come. Will he come to the next conference, ICCF11, in Marseilles? I hope so. His results, described at

http://jlnlabs.imars.com/cfr/index.htm

are interesting. Naudin constructed a simple device, named cold fusion reactor (CFR) to generate excess power electrochemically. This URL shows results of numerous experiments designed to test reproducibility. He performed 32 such experiments and in all of them exit power was larger than input power, sometimes by the factor larger than two. Two other teams of researchers, one in the US and another in France, also confirmed generation of excess heat. In one case a toy engine was run on the excess power of the CFR.

I wish I were able to convince myself that the recorded excess power could not possibly be due to chemical reactions taking place inside the cells. Unfortunately, this possibility is not even mentioned. The name of the device, CFR, implies

nuclear origin of excess power and I expected to find a justification of such claim. That is why I was disappointed. But I was impressed by graphical illustrations and by simplicity of the apparatus. I plan to replicate his results as soon as a chemistry collaborator is found.

Naudin's document begins with a declaration: "I have used the experimental protocol fully described by Eugene F. Mallove at . Mallove, who died tragically this summer, was the editor of Infinite Energy magazine. I did fetch his description of the experimental protocol to which Naudin refers but found no discussion of chemical reactions. Why should I rule out a possibility that the excess heat is due to chemical reactions taking place inside the reactor? But, as said before, I am not a chemist. Perhaps what is not clear to me would be obvious to a chemist. Mallove wrote that the experiment "involves high-temperatures, high-voltages, explosive mixtures of oxygen and hydrogen, caustic solutions, and steam generation. .....It is visually and audibly spectacular -- brilliant glowing, pink, purple, lavender with white flashes on an underwater tungsten (W) electrode (e.g. 2 mm x 5 mm W foil or 1 cm x 1.6 mm diameter tungsten welding rod). A plasma-like underwater discharge on the electrode that often manages to disintegrate or melt tungsten underwater." Why should I exclude a possibility that some of the chemical reactions in CFR are exothermic? The current and voltage, in experiments described by Mallove, were 5 A and 180 V. No wonder that intensive underwater sparking and boiling takes place. I had a chance to experiment with similar underwater sparking two years ago but no calorimetric measurements were made.

Another thing that impressed me was that one of the teams that confirmed Naudin's findings consisted of highly trained electrochemist. On the other hand, I was slightly disappointed that the identity of the US laboratory, in which the confirming calorimetric experiments were conducted, was not specified. The document shows the photo of two researchers from Louisiana, and their names (two nice looking young girls, probably students), but not the name of the laboratory, or the name of the project director. Was it a high school project or a projects at a well known university? Such information is important when controversial results are presented. Why was it not provided by Naudin?

Before finishing this unexpected essay let me reflect on what came to my mind as I was reading Kanarev's paper again. I was thinking about similarities between his claims of Adamenko and those of Adamenko. Both scientists introduce themselves as theoretical physicists. Both refer to totally unheard off, and hard to believe, experimental results. Both are seeking foreign and domestic investors to support their projects. I suspect that these two people know each other very well. Perhaps they were working in the same Soviet laboratory about fifteen years ago; perhaps they learned physics from the same teachers. What a coincidence; I was informed about work of these scientists by strangers, and approximately at the same time (see unit #168).

Reading Kanarev's paper I was thinking about the Chinese scientist, Xing-liu Jiang, who wrote to me about zero point energy. This was described in my unit #162. After posting that, I asked the scientist to comment on what he wrote. He never did and I do not know why. Perhaps he expected me to be more knowledgeable in theoretical physics. Yes, my limitations prevent me from penetrating Kanarev's reasoning leading to new ways of obtaining energy from water. Perhaps a knowledgeable person will send me a simplified description of that reasoning. The best I can do is to show some quotes from Kanarev's paper:

"Problems of power engineering are well known. The power carriers, which are used nowadays, are not only exhaustible, but they cause a considerable damage to environment. Nearly 40 years ago it was announced that controlled thermonuclear fusion could be the future inexhaustible energy source. More than 25 billion dollars have been spent for the investigation of this source, but there is no positive result. This state can be explained by a considerable lag of theoretical investigations from the possibilities of industry to implement any installation in order to check an intuitive idea being formulated. As a result, the main attention has been paid to an *experiment*, not to a *theory*." What is wrong with using experimental evidence to validate theoretical anticipation?

"As there was no acceptable theoretical description of the planned process of thermonuclear fusion control, a positive result could be obtained only by chance. Theoretical prognostication of this result was impossible due to the absence of any notion concerning the models: the electron, the proton, the atom of hydrogen and the ions of chemical elements, which form plasma. Orthodox physics did not even set itself such task." My impression was that quantum mechanics, and quantum electrodynamics, made big progress in dealing with such topics.

“Modern industrial installations require 4 kWh for production of 1 cubic meter of hydrogen from water. When this hydrogen is burnt, 3.6 kWh of energy is released. If the energy expenses for production of hydrogen from water are reduced by twofold or threefold, it becomes a competitive energy carrier. If it is possible to reduce these expenses of hydrogen tenfold, it will become the cheapest energy carrier. In this case, coal, oil and natural gas fail to compete with it. Our investigations have shown that there are some plasma electrolytic devices and modes of their operation, which reduce energy expenses for obtaining one cubic meter of hydrogen up to 0.40 kWh. In this case, more than 1000% of additional energy is obtained. A laboratory device with such indices was made one year ago.”

The transition from a laboratory installation to the industrial one requires additional investigations with the use of rather expensive spectrometers, gas analyzers, electronic sets for simultaneous registration of more than 10 induces of the plasma electrolytic process. As hydrogen is an explosive gas, it is impossible to ignore the investigation stage for making the laboratory installation the industrial one. During the scale operation, dangerous radiations can take place, which accompany the transmutation process of the chemical element nuclei.

Five patents have been received for the investigation results; three positive decisions concerning the issue of the patents and three claims are in the process of consideration. Three editions of the book ‘*Water as a New Source of Energy*’ are available with the detailed theoretical description of plasma electrolysis of water and the quantitative calculations of the experimental results..... As the author has kept walking along the corridors of the Russian power without success for five years in order to get financing, the last hope remains – to find a foreign investor. The author is busy with this problem as it is clear that a delay in financing is equal to a loss of priority in this topical field of investigations, which solves two global problems of the mankind: the *energy problem* and the *environmental problem*.”

P.S. (9/26/04)
In Kanarev’s Page at I see some starnge phrases. On page 3, for example, I see “binding energy of the electron in the atomic nucleus” and “electron begins to absorb thermal photons.” On the next page I see “the proton should capture 2.531 electrons in order to become a neutron” and “particle ‘being dissolved’ in the ether.” Sentences in which these phrases appear might have some profound meaning to a theorist. But I am not a theorist. The ideas of electrons being captured by atomic nuclei, or being dissolved in the ether, are in conflict with what I know about nuclear phenomena. I hope someone more knowledgeable will comment on Kanarev’s project and ideas.

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171) When will we hear from the DOE?

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Do you remember the April, 2004, issue of Physics Today? It published a short article of Tont Feder entitled “DOE Warms to Cold Fusion. According to James Decker, deputy director of DOE's Office of Science, she wrote, a review of cold fusion will begin in the next month or so and it “won't take a long time--it's a matter of weeks or months.“ That was half a year ago; not much was written on the subject since that time. But, according to Justin Mullins article, in the September 2004 issue of IEEE Spectrum on Line,

http://www.spectrum.ieee.org/WEBONLY/resource/sep04/0904nfus.html

“Later this month, the U.S. Department of Energy will receive a report from a panel of experts on the prospects for cold fusion -- the supposed generation of thermonuclear energy using tabletop apparatus.” I would like to thank Steven Krivit for informing me about this paper yesterday. The Internet search engine tells me that Justin Mullins is a science journalist and one of the editors of New Science. But is he the author of the IEEE piece? I do not know; perhaps it is somebody else with the same name. And how does he know that the panel report will be ready this month? I asked Steven this question. The answer was: “He doesn't KNOW, but is guessing. According to information I have heard from insiders, his guess is accurate.” I know, from one of the participants, that the panel review took place on August 23 and 24. But I have no idea when will the report be released.

I hope that the panel will look upon cold fusion as a set of interesting scientific claims; looking at it as a technology for “generation of thermonuclear energy using tabletop apparatus,” as described by Mullin, would be totally unjustified at this time. My Letter to the Editor of Physics Today (just published in the September, 2004, issue), focuses on that topic. Here is the content of my letter:

“As a physics teacher who is uncertain how to answer students' questions about cold fusion, I welcome the upcoming US Department of Energy investigation of recent claims in this controversial area. I agree with Toni Feder (Physics Today, April 2004, page 27) that "skepticism about the credibility and reproducibility of cold fusion remains widespread."

I have some questions I'd like to see the DOE investigators answer. Is it true that unexpected emission of neutrons, protons, tritons, and alpha particles (at rates significantly above the background) has been observed in several cold fusion experiments? Has accumulation of helium-4, at the rate of about one atom per 24 MeV of excess heat, been confirmed by many scientists, as reported by electrochemist Michael McKubre in Feder's story? Have highly abnormal isotopic ratios been found in some cold fusion setups? Is there any indication that leading cold fusion scientists are incompetent or that their data are fraudulent? Is the research methodology that cold fusion scientists use different from that used in other areas of physical science? Answers to these questions will help me decide what to think about cold fusion and what to tell students about it.

Speculations about practical applications of new findings should be de-emphasized at this time. They will emerge naturally when basic scientific claims are recognized as valid and when researchers in cold fusion are no longer treated as if they were con artists and charlatans. The "chilling effect" mentioned by Randall Hekman in the Physics Today story prevents young scientists from entering the area of cold fusion research. I also agree with chemist Allen Bard that being able to reproduce experimental results is not good enough; it is only a preliminary step. But wasn't poor

reproducibility the central point of criticism when cold fusion was first investigated 15 years ago? In my opinion, experimental claims should not be disqualified solely on reproducibility; validation should depend on credentials of researchers and, above all, on methodologies they used in particular experiments. “

I was very surprised that my letter to the editor was not “one of many.” Does it mean that nobody else reacted to Feder’s article? That is not likely to be the case. In the e-mail message the editor wrote to me that they expect to have a “lively discussion” on the subject and that several letters had already been accepted. The implication was that my letter will not be in the first bunch to be published. And then, at the end of July, I was informed that my letter was accepted. I was also given an opportunity to revise the letter. The last sentence was added at this time. I was tempted to add more but was afraid that this might delay the publication. What happened? Why other letters were not published at the same time?

My guess is that they received many letters that will appear in the next issue. My letter was probably chosen because it shows no bias toward or against cold fusion. It could be published alone without giving preference to one point of view or another. Other contributors will probably be made by active research scientists; I am eager to read them and to learn from them. I am happy that my letter was published at this time; perhaps it will be taken seriously by the members of the panel investigating cold fusion.

Let me end this short essay with some extracts from Feder’s article. Referring to her interview with Decker she quotes him: “Whether or not it has applications to the energy business is clearly unknown at this point, but you need to sort out the science before you think about applications." Is this an indication that the panel will focus on science and not on science fiction? I hope so. Referring to a excess power she wrote: "We know that this has economic implications and, potentially, security implications. The main application that cold fusion enthusiasts foresee following from their work is a clean source of energy; transmutation of nuclear waste and tritium production to augment weapons are also on their list.” It is difficult to be an enthusiast and not to allow the enthusiasm to interfere with objectivity.

As I wrote before, appearance of excess power per se is not an indication that nuclear energy is involved. The only way to make excess power relevant, in the contest of cold fusion, is to focus on the associated accumulation of reaction products. Many cold fusion researchers are fully aware of this. Contrary to initial expectations radioactive byproducts, if any, are not always dominant. That makes their identification difficult when the amounts of released energy are small. Another difficulty has to do with the possible “electric battery” effects. How do we know that a positive excess power, observed in a later part of an experiment, is not accompanied by an earlier negative excess? By negative excess power I mean some kind of storage of supplied energy, for example, in a chemical compound. Both difficulties were recognized when the discovery of cold fusion was announced in 1989.

Some theoretical physicists (see units #162, #168 and #170) try to bypass the difficulties by postulating that excess power can come from nowhere. They speculate about the so-called “zero point energy of vacuum” without performing experiments to validate such ideas. I am neither qualified nor ready to defend this approach. In my opinion, the panel of scientists investigation cold fusion should focus on claims supported by experimental evidence. Theoretical models are important components of our understanding of nature but focusing on them at this time, in the highly controversial area of excess power, seems to be premature. Discussion involving theoretical models will develop spontaneously after the reality of at least one experimental claim is recognized. A panel-investigation approach to a scientific controversy is highly unusual; in this case it is needed to correct the effect of an error made fifteen years ago. The main purpose should be to establish normality not to make a definite pronouncement about a set of claims.

A recent overview of cold fusion, by a scientist: Edmund Storms, was submitted to the panel of experts appointed by the DOE to reevaluate cold fusion. The reevaluation, as indicated by Storms, started on August 23, 2004. To see his overview go to:

<http://lenr-canr.org>

and scroll down to the link “Why I believe 'Cold Fusion' is Real.” Click on that link and the pdf file containing the paper will be downloaded to your computer. It is interesting that neither zero point energy nor hydrinos are mentioned. This confirms what I wrote about these fields; they are not part of cold fusion. Let me pause now. Additional paragraphs are likly to be appended to this essay in coming weeks. They will focus on the expected “lively discussion” triggered by the

DOE initiative, and by Feder's article.

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172) Items for student projects

Ludwik Kowalski (9/9/04)
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Browsing the Internet I discovered a student-oriented item on cold fusion. The URL for this item is:

http://www.all-science-fair-projects.com/science_fair_projects_encyclopedia/Cold_fusion

I am posting its content here because I do not know how permanent this Internet item will be. The Science Fair Projects Encyclopedia, whose URL is:

http://www.all-science-fair-projects.com/science_fair_projects_encyclopedia/Main_Page_asfp

seems to be a good resource to start learning about a subject. Here is a list of main topics in the area of physics and chemistry.

* **Physics**
* Acoustics
* Astronomy
* Astrophysics
* Atomic, Molecular, and Optical physics
* Biophysics
* Computational physics
* Condensed matter physics
* Cryogenics
* Electronics
* Engineering
* Fluid dynamics
* Polymer physics
* Optics
* Materials physics
* Mathematical physics
* Nuclear physics
* Plasma physics
* Particle physics (or High Energy Physics)
* Vehicle dynamics

* **Chemistry**
* Censored page
* Biochemistry
* Computational chemistry
* Electrochemistry
* Inorganic chemistry
* Materials science

* Organic chemistry
* Physical chemistry
* Quantum chemistry
* Spectroscopy
* Stereochemistry
* Thermochemistry

Other areas, such as Biology or Computer Science, offer similar links to start searching. To find the link to cold fusion I clicked on “Nuclear physics”, then on “Fusion.” The link was found at the bottom of that item. A student who does not know how to search by categories can find an item of interest by typing it in the search box. I typed “cold fusion” (must be without quotation marks) and the item appeared.

That particular item was “last updated” on 9/2/2004. In trying to locate the author I found the following: “The content of each page in our Encyclopedia section originates from a corresponding web page at Wikipedia.org and is licensed for use under the terms of the GNU Free Documentation License. The source web page can be accessed by going to http://en.wikipedia.org/wiki/[heading of the relevant encyclopedia page]. For example, if you are at viewing a page in our Encyclopedia with the heading "Biology", you can go to the origin of this content at Wikipedia.org by pointing your browser to http://en.wikipedia.org/wiki/biology. There, you will be able to view the original content and may be able to participate in editing such content . . . “

Aha, Wikopedia! That is an online encyclopedia in which anybody can change anything. Several weeks ago I added a sentence to one items because I felt that it was important. A week later that item was removed by somebody who did not like it. My sentence was replaced by something else. The authors of posted items have not be selected on the basis of their competence, and the content has not been screened for accuracy by recognized experts. Therefore students should be advised to verify the content by going to more reliable sources. The underlined words and phrases were links in the original; they are no longer links in my pasted text below.

Cold fusion

Cold fusion is a nuclear fusion reaction that occurs well below the temperature required for thermonuclear reactions (millions of degrees Celsius). Such reactions may occur near room temperature and atmospheric pressure, and even in a relatively small (table top) experiment. In a narrower sense, "cold fusion" also refers to a particular type of fusion presumably occurring in electrolytic cells.

The term "cold fusion" was coined by Dr Paul Palmer of Brigham Young University in 1986 in an investigation of "geo-fusion", or the possible existence of fusion in a planetary core. It was brought into popular consciousness by the controversy surrounding the Fleischmann-Pons experiment in March of 1989. A number of other scientists have reported replication of their experimental observation of anomalous heat generation in electrolytic cells, but in a non-predictable way, and most scientists believe that there is no proof of cold fusion in these experiments. A majority of scientists consider this research to be pseudoscience, while proponents argue that they are conducting valid experiments in a protoscience that challenges mainstream thinking.

The subject has been of scientific interest since nuclear fusion was first understood. Hot nuclear fusion using deuterium yields large amounts of energy, uses an abundant fuel source, and produces only small amounts of manageable waste; thus a cheap and simple process of nuclear fusion would have great economic impact. Unfortunately, no experiments have yet been able to show both a "cold" fusion reaction and a large net release of energy over the whole experiment.

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History of cold fusion by electrolysis

Early work

The idea that palladium or titanium might catalyze fusion stems from the special ability of these metals to absorb large quantities of hydrogen (including its deuterium isotope), the hope being that deuterium atoms would be close enough together to induce fusion at ordinary temperatures. The special ability of palladium to absorb hydrogen was recognized in the nineteenth century. In the late nineteen-twenties, two German scientists, F. Paneth and K. Peters, reported the transformation of hydrogen into helium by spontaneous nuclear catalysis when hydrogen is absorbed by finely divided palladium at room temperature. These authors later acknowledged that the helium they measured was due to background from the air.

In 1927, Swedish scientist J. Tandberg said that he had fused hydrogen into helium in an electrolytic cell with palladium electrodes. On the basis of his work he applied for a Swedish patent for "a method to produce helium and useful reaction energy". After deuterium was discovered in 1932, Tandberg continued his experiments with heavy water. Due to Paneth and Peters' retraction, Tandberg's patent application was denied eventually.

Pons and Fleischmann's experiment

On March 23, 1989, chemists Stanley Pons and Martin Fleischmann ("P and F") at the University of Utah held a press conference and reported the production of excess heat that could only be explained by a nuclear process. The report was particularly astounding given the simplicity of the equipment, just a pair of electrodes connected to a battery and immersed in a jar of heavy water (dideuterium oxide). The press reported on the experiments widely, and it was one of the front-page items on most newspapers around the world. The immense beneficial implications of the Utah experiments, if they were correct, and the ready availability of the required equipment, led scientists around the world to attempt to repeat the experiments within hours of the announcement.

The press conference followed about a year of work of increasing tempo by Pons and Fleischmann, who had been working on their basic experiments since 1984. In 1988 they applied to the US Department of Energy for funding for a larger series of experiments: up to this point they had been running their experiments "out of pocket".

The grant proposal was turned over to several people for peer review, including Steven Jones of Brigham Young University. Jones had worked on muon-catalyzed fusion for some time, and had written an article on the topic entitled *Cold Nuclear Fusion* that had been published in Scientific American in July 1987. He had since turned his attention to the problem of fusion in high-pressure environments, believing it could explain the fact that the interior temperature of

the Earth was hotter than could be explained without nuclear reactions, and by unusually high concentrations of Helium-3 around volcanoes that implied some sort of nuclear reaction within. At first he worked with diamond anvils, but had since moved to electrolytic cells similar to those being worked on by Pons and Fleischmann, which he referred to as *piezonuclear fusion*. In order to characterize the reactions, Jones had spent considerable time designing and building a neutron counter, one able to accurately measure the tiny numbers of neutrons being produced in his experiments.

Both teams were in Utah, and met on several occasions to discuss sharing work and techniques. During this time Pons and Fleischmann described their experiments as generating considerable "excess energy", excess in that it could not be explained by chemical reactions alone. If this were true, their device would have considerable commercial value, and should be protected by patents. Jones was measuring neutron flux instead, and seems to have considered it primarily of scientific interest, not commercial. In order to avoid problems in the future, the teams *apparently* agreed to simultaneously publish their results, although their accounts of their March 6th meeting differ.

In mid-March both teams were ready to publish, and Fleischmann and Jones were to meet at the airport on the 24th to both hand in their papers at the exact same time. However Pons and Fleischmann then "jumped the gun", and held their press conference the day before. Jones, apparently furious at being "scooped", faxed in his paper to *Nature* as soon as he saw the press announcements. Thus the teams both rushed to publish, which has perhaps muddied the field more than any scientific aspects.

Within days scientists around the world had started work on duplications of the experiments. On April 10th a team at Texas A&M University published results of excess heat, and later that day a team at the Georgia Institute of Technology announced neutron production. Both results were widely reported on in the press. Not so well reported was the fact that both teams soon withdrew their results for lack of evidence. For the next six weeks competing claims, counterclaims, and suggested explanations kept the topic on the front pages, and led to what writers have referred to as "fusion confusion."

In mid-May Pons received a huge standing ovation during a presentation at the American Chemical Society. The same month the president of the University of Utah, who had already secured a $5 million commitment from his state legislature, asked for $25 million from the federal government to set up a "National Cold Fusion Institute". On May 1st a meeting of the American Physical Society held a session on cold fusion that ran past midnight; a string of failed experiments were reported. A second session started the next evening and continued in much the same manner. The field appeared split between the "chemists" and the "physicists".

At the end of May the Energy Research Advisory Board (under a charge of the US Department of Energy) formed a special panel to investigate cold fusion. The scientists in the panel found the evidence for cold fusion to be unconvincing. Nevertheless, the panel was "*sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system*". [1] http://www.ncas.org/erab/sec5.htm

Both critics and those attempting replications were frustrated by what they said was incomplete information released by the University of Utah. With the initial reports suggesting successful duplication of their experiments there was not much public criticism, but a growing body of failed experiments started a "buzz" of their own. Pons and Fleischmann later apparently claimed that there was a "secret" to the experiment, a statement that infuriated the majority of scientists to the point of dismissing the experiment out of hand.

By the end of May much of the media attention had faded. This was due not only to the competing results and counterclaims, but also to the limited attention span of modern media. However, while the research effort also cooled to some degree, projects continued around the world.

Experimental set-up and observations

In their original set-up, Fleischmann and Pons used a Dewar flask (a double-walled vacuum flask) for the electrolysis, so that heat conduction would be minimal on the side and the bottom of the cell (only 5 % of the heat loss in this experiment). The cell flask was then submerged in a bath maintained at constant temperature to eliminate the effect of

external heat sources. They used an open cell, thus allowing the gaseous deuterium and oxygen resulting from the electrolysis reaction to leave the cell (with some heat too). It was necessary to replenish the cell with heavy water at regular intervals. The cell was tall and narrow, so that the bubbling action of the gas kept the electrolyte well mixed and of a uniform temperature. Special attention was paid to the purity of the palladium cathode and electrolyte to prevent the build-up of material on its surface, especially after long periods of operation.

The cell was also instrumented with a thermistor to measure the temperature of the electrolyte, and an electrical heater to generate pulses of heat and calibrate the heat loss due to the gas outlet. After calibration, it was possible to compute the heat generated by the reaction.

A constant current was applied to the cell continuously for many weeks, and heavy water was added as necessary. For most of the time, the power input to the cell was equal to the power that went out of the cell within measuring accuracy, and the cell temperature was stable at around 30 °C. But then, at some point (and in some of the experiments), the temperature rose suddenly to about 50 °C without changes in the input power, for durations of 2 days or more. The generated power was calculated to be about 20 times the input power during the power bursts. Eventually the power bursts in any one cell would no longer occur and the cell was turned off.

Continuing efforts

There are still a number of people researching the possibilities of generating power with cold fusion. Scientists in several countries continue the research, and meet at the International Conference on Cold Fusion (see Proceedings at www.lenr-can.org http://www.lenr-canr.org/index.html)

The generation of excess heat has been reported by

Michael McKubre, Center at Stanford Research International,
Richard A. Oriani (University of Minnesota, in Dec 90),
Robert A. Huggins (at Stanford University in March 90), and
Y. Arata (Osaka University, Japan)

among others. In the best experimental set-up, excess heat was observed in 50% of the experiment reproductions. Various fusion ashes and transmutations were observed by some scientists.

Dr. Michael McKubre thinks a working cold fusion reactor is possible. Dr. Edmund Storms, a former scientist with The Los Alamos National Laboratory in New Mexico, maintains an international database of research into cold fusion.

In March 2004 the US Department of Energy decided to review all previous research of cold fusion in order to see whether further research was warranted by any new results.

Arguments in the controversy

A majority of scientists consider current cold fusion research to be pseudoscience, while proponents argue that they are conducting valid experiments that challenge mainstream science. (see history of science and technology). Here are the main arguments in the controversy.

Reproducibility of the result

While some scientists have reported to have reproduced the excess heat with similar or different set-ups, they could not do it with predictable results, and many others failed. Some see this as a proof that the experiment is pseudoscience.

While the experiment would indeed be more convincing if it were reproducible by simply following a recipe, or if the power generation was continuous instead of sporadic, it is not uncommon for a new phenomenon to be difficult to control, and to bring erratic results. For example attempts to repeat electrostatic experiments (similar to those performed

by Benjamin Franklin) often fail due to excessive air humidity. That does not mean that electrostatic phenomena are fictitious, or that experimental data are fraudulent. Occasional observations of new events, by qualified experimentalists, can be preliminary steps leading to recognized discoveries.

Most scientists doubt this is one of those cases, however, due to the fact that many experiments have been done in the 15 years since the 1989 announcements, and experimental results continue to be erratic, unconvincing, and poorly understood. Recent experimental results, on the other hand, are much less erratic than 15 years ago. Steven Jones, for example, reported (in 2003) that unexpected neutrons and charged particles, from TiDx, are observed in approximately 70% of experiments. Criticism of experimental data should not be based on reproducibility, it should be based on credentials of researchers and, above all, on examination of methodologies they use in particular investigations. An experimental claim, however, can not be taken for granted without one hundred percent reproducibility.

Current understanding of nuclear process

The DOE panel says: "*Nuclear fusion at room temperature, of the type discussed in this report, would be contrary to all understanding gained of nuclear reactions in the last half century; it would require the invention of an entirely new nuclear process*".

However, this argument only says that the experiment has unexplained results, not that the experiment is wrong. As an analogy, superconductivity was observed in 1911, and explained theoretically only in 1957.

Current understanding of hot nuclear fusion shows that the following explanations are not adequate:

* Nuclear reaction in general: The average density of deuterium in the palladium rod seems vastly insufficient to force pairs of nuclei close enough for fusion to occur according to mechanisms known to mainstream theories. The average distance is approximately 0.17 nanometers, a distance at which the attractive strong nuclear force cannot overcome the Coulomb repulsion. Actually, deuterium atoms are closer together in D2 gas molecules, which do not exhibit fusion.

* Absence of standard nuclear fusion products: if the excess heat were generated by the fusion of 2 deuterium atoms, the most probable outcome would be the generation of either a tritium atom and a proton, or a ^{3}He and a neutron. The level of neutrons, tritium and ^{3}He actually observed in Fleischmann-Pons experiment have been well below the level expected in view of the heat generated, implying that these fusion reactions cannot explain it.

* Fusion of deuterium into Helium 4: if the excess heat were generated by the hot fusion of 2 deuterium atoms into ^{4}He, a reaction which is normally extremely rare, gamma rays and Helium would be generated. Again, insufficient levels of Helium and gamma rays have been observed to explain the excess heat, and there is no known mechanism to explain how gamma rays could be converted into heat.

Energy source vs power store

While the output power is higher than the input power during the power burst, the power balance over the whole experiment does not show significant imbalances. Since the mechanism under the power burst is not known, one cannot say whether energy is really produced, or simply stored during the early stages of the experiment (loading of deuterium in the Palladium cathode) for later release during the power burst.

A "power store" discovery would have much less value than an "energy source" one, especially if the stored power can only be released in the form of heat.

Other kinds of fusion

This article focuses on fusion in electrolytic cells. Other forms of fusion have been studied by scientists. Some are "cold" in the sense that no part of the reaction is actually hot (except for the reaction products), some are "cold" in the sense that the energies required are low and the bulk of the material is at a relatively low temperature, and some are

"hot", involving reactions which create macroscopic regions of very high temperature and pressure.

Locally cold fusion :

* Muon-catalyzed fusion is a well-established and reproducible fusion process which occurs at low temperatures. It has been studied in detail by Steven Jones in the early 1980's. Because of the energy required to create muons, it is not able to produce net energy.

Generally cold, locally hot fusion :

* In Cluster impact fusion , microscopic droplets of heavy water (on the order of 100-1000 molecules) are accelerated to collide with a target, so that their temperature at impact reaches at most 10^5 Kelvin, 10,000 times smaller than the temperature required for hot fusion. In 1989, Friedlander and his coworkers observed 10^{10} more fusion events than expected with standard fusion theory. Recent research ([2] http://arxiv.org/abs/nucl-th/0304066) suggests that the calculation of effective temperature may have failed to account for certain molecular effects which raise the effective collision temperature, so that this is a microscopic form of hot fusion.

* In sonoluminescence, acoustic shock waves create temporary bubbles that collapse shortly after creation, producing very high temperatures and pressures. In 2002, Rusi P. Taleyarkhan explored the possibility that bubble fusion occurs in those collapsing bubbles. If this is the case, it is because the temperature and pressure are sufficiently high to produce hot fusion.

* The Farnsworth-Hirsch Fusor is a tabletop device in which fusion occurs. This fusion comes from high effective temperatures produced by electrostatic acceleration of ions. The device can be built inexpensively, but it too is unable to produce a net power output.

* Antimatter-catalyzed fusion uses small amounts of antimatter to trigger a tiny fusion explosion. This has been studied primarily in the context of making nuclear pulse propulsion feasible.

Hot fusion :

* "Standard" fusion, in which the fuel reaches tremendous temperature and pressure inside a fusion reactor, nuclear weapon, or star.

Several of these systems are "nonequilibrium systems", in which very high temperatures and pressures are produced in a relatively small region adjacent to material of much lower temperature. In his doctoral thesis for MIT, Todd Rider did a theoretical study of all non-equilibrium fusion systems. He demonstrated that all such systems will leak energy at a rapid rate due to Bremsstrahlung, radiation produced when electrons in the plasma hit other electrons or ions at a cooler temperature and suddenly decelerate. The problem is not as pronounced in a hot plasma because the range of temperatures, and thus the magnitude of the deceleration, is much lower.

References

Popular accounts of the controversy

Robert L. Park (2000) gives a thorough account of cold fusion and its history which represents the perspective of the mainstream scientific community. Two other sceptical books from the scientific mainstream are those by Frank Close (1992) and John Huizenga (1992). Huizenga was co-chair of the DOE panel set up to investigate the Pons/Fleischmann experiment, and his book is perhaps the definitive account of the cold fusion affair.

Eugene Mallove's *Fire from Ice* (1991) is an early account from the pro-cold-fusion perspective. Charles Beaudette 's *Excess heat* (2000) is a more recent scientific account of why cold fusion research prevailed.

* **Voodoo Science: The Road from Foolishness to Fraud**, by Robert L. Park; Oxford University Press, New York; ISBN 0195135156; May 2000.
* **Too Hot To Handle**, by Frank Close; Penguin Books; ISBN 0140159266; 1992.
* **Cold Fusion: the scientific fiasco of the century**, by John R Huizenga; Oxford Paperbacks; ISBN 0198558171; 1992.
* **Fire from Ice**, by Eugene Mallove; Infinite Energy Press; ISBN 1892925028; 1991.
* **Excess Heat: why cold fusion research prevailed**, by Charles Beaudette; Infinite Energy Press http://www.infinite-energy.com ; ISBN 0967854814

See also

* alchemy
* transmutation
* Pathological science
* Protoscience

External links

Information:

* Energy Research Advisory Board, "*Conclusions and recommendations* *http://www.ncas.org/erab/sec5.htm* "
* "*Low Energy Nuclear Reactions - Chemically Assisted Nuclear Reactions* *http://www.lenr-canr.org/* ". -- Information and links from pro-cold fusion research.
* L. Kowalski's web site http://blake.montclair.edu/~kowalskil/cf/ : an overview of the current state of cold fusion research from a physics teacher
* Britz's cold nuclear fusion bibliography http://www.chem.au.dk/~db/fusion/ : An extentisve overview and review of almost all available publications about cold nuclear fusion.

News:

* "*Sound waves size up sonoluminescence* *http://physicsweb.org/article/news/6/2/3* ". PhysicsWeb. February 2002.
* "*Whatever happened to cold fusion* *http://physicsweb.org/article/world/12/3/8 ?*". Physics World. March 1999.
* "*Fusion experiment disappoints* *http://news.bbc.co.uk/2/hi/sci/tech/2151215.stm* ". BBC News. July 25, 2002
* "*Cold Fusion Heats Up* *http://www.radio.cbc.ca/programs/quirks/archives/03-04/dec13.html* . *CBC Science.*
* DoE to review cold fusion http://physicstoday.org/vol-57/iss-4/p27.html *Physics Today* April 2004.
* Phys. Rev. E 69, 036109 (2004) http://scitation.aip.org/dbt/dbt.jsp?KEY=PLEEE8&Volume=69&Issue=3 "*Additional evidence of nuclear emissions during acoustic cavitation*", R. P. Taleyarkhan, J. S. Cho, C. D. West, R. T. Lahey, Jr., R. I. Nigmatulin, and R. C. Block.

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173) Pseudoscience in Wikopedia

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As I indicated in item #172, there is an interesting creature on the Internet, an online encyclopedia that is editable. If you do not like something you can simple delete it, or replace it with something else, or add a comment, etc. etc. It is called “Wikipedia, the free encyclopedia.” To learn more about this creature go to:

http://en.wikipedia.org/wiki/Main_Page
http://en.wikipedia.org/wiki/Wikipedia:Welcome%2C_newcomers

Here is an item on pseudoscience that I found in Wikopedia. The issues of protoscience and pseudoscience belong to philosophy, like the issue of the so-called “scientific method.” But they are certainly worth discussing with students. That is my justification for posting the piece below.

Pseudoscience

From Wikipedia, the free encyclopedia.

A **pseudoscience** is any body of knowledge purporting to be either both factual and scientific, or of an even higher standard of knowledge , but which fails to comply with scientific method.

Motivations for the advocacy or promotion of pseudoscience range from simple naivety about the nature of science or of the scientific method, to deliberate deception for financial or other benefit. Some people consider some or all forms of pseudoscience to be harmless entertainment. Others, such as Richard Dawkins , consider all forms of pseudoscience to be harmful, whether or not they result in immediate harm to their followers.

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[edit]

Classifying Pseudoscience

Typically, pseudoscience fails to meet the criteria met by science generally (including the scientific method), and can be identified by a combination of these characteristics:
by asserting claims without supporting experimental evidence;
by asserting claims which contradict experimentally established results;
by failing to provide an experimental possiblity of reproducible results;
by asserting claims that violate falsifiability ; or
by violating Occam's Razor (the principle of choosing the simplest explanation when multiple viable explanations are possible); the more egregious the violation, the more likely.

Pseudoscience is distinguishable from revelation, theology or spirituality in that it claims to offer insight into the physical world by "scientific" means (i.e., most usually in accordance with the scientific method). Systems of thought that rely upon "divine" or "inspired" knowledge are not considered pseudoscience if they do not claim to be scientific or to overturn well established science.
[edit]

Pseudoscience Contrasted with Protoscience

Pseudoscience also differs from protoscience . The latter may be defined as speculation or hypothesis which has not yet been tested adequately by the scientific method, but which is otherwise consisent with existing science or which, where inconsistent, offers reasonable account of the inconsistency. Pseudoscience, in contrast, is characteristically wanting adequate tests or the possibility of them, occasionally untestable in principle, and its supporters are frequently strident in insisting that existing scientific results are wrong. Pseudoscience is often unresponsive to ordinary scientific procedures (e.g., peer review, publication in standard journals). In some cases, no one applying scientific methods could disprove a pseudoscientific hypothesis (i.e. untestable claims) and failure to do so is often cited as evidence of the truth of the pseudoscience.

The boundaries between pseudoscience, protoscience, and "real" science are often unclear to non-specialist observers. They can even be obscure to experts. Many people have tried to offer objective criteria for the term, with mixed success. Often the term is used simply as a pejorative to express the speaker's low opinion of a given field, regardless of any objective measures.

If the claims of a given pseudoscience can be experimentally tested it may be real science, however odd, astonishing, or intuitively acceptable. If they cannot be tested, it is likely pseudoscience. If the claims made are inconsistent with existing experimental results or established theory, it is often presumed to be pseudoscience. Conversely, if the claims of any given "science" cannot be experimentally tested it may not be a real science, however odd, astonishing, or intuitively acceptable.

In such circumstances it may be difficult to distinguish which of two opposing "sciences" are valid; for example, both the proponents and opponents of the Kyoto Protocol on global warming have recruited the help of scientists to endorse contradictory "scientific" positions, because of differing political goals. This

enlistment of science in the service of politics is sometimes called " junk science ".

Such fields as acupuncture and lucid dreaming may be categorized as protosciences; there is a reasonable expectation that as they are experimentally examined, they may produce some scientifically valid results. They are, at least, accessible to experimental examination.
[edit]

The Problem of Demarcation

After more than a century of active dialogue, the question of what marks the boundary of science remains fundamentally unsettled. As a consequence the issue of what constitutes pseudoscience continues to be controversial. Nonetheless, reasonable consensus exists on certain sub-issues. Criteria for demarcation have traditionally been coupled to one philosophy of science or another. Logical positivism , for example, espoused a theory of meaning which held that only statements about empirical observations are meaningful, effectively asserting that statements which are not derived in this manner (including all metaphysical statements) are meaningless. Later, Karl Popper attacked logical positivism and introduced his own criterion for demarcation, falsifiability . This in turn was criticised by Thomas Kuhn , and also by Popper supporter Imre Lakatos who proposed his own criteria that distinguished between progressive and degenerative research programs.
[edit]

Kuhn and paradigm shifts

Thomas Kuhn , an American historian of science , has proven very influential in the philosophy of science , and is often connected with what has been called postpositivism or postempiricism . In his 1962 book The Structure of Scientific Revolutions , Kuhn divided the process of doing science into two different endeavors, which he called **normal science** and **extraordinary science** . The process of "normal" science is what most scientists do while working within the current accepted paradigm of the scientific community, and within this context Karl Popper 's ideas on falsification as well as the idea of a scientific method still have some currency. This sort of work is what Kuhn calls "problem solving": working within the bounds of the current theory and its implications for what sorts of experiments should or should not be fruitful. However, during the process of doing "normal" science, Kuhn claimed, anomalies are generated, some of which lead to an extension of the dominant paradigm in order to explain them (like the idea of punctuated equilibrium within the paradigm of evolution), and others for which no satisfactory explanation can be found within the current paradigm.

When enough of these anomalies have accumulated, and scientists within the field find them significant (often a very subjective judgment), a "crisis period" is began, and some scientists begin to participate in the activity of "extraordinary" science. In this phase, it is recognized that the old paradigm is fundamentally flawed and cannot be adapted to further use, and totally new (or often old and abandoned) ideas are looked at, most of which will be failures. But during this time, a new paradigm is created, and after a protracted period of " paradigm shift ," the new paradigm is accepted as the norm by the scientific community and integrated into their previous work, and the old paradigm is banished to the history books. The classic example of this is the shift from Maxwellian /Newtonian physics to Einsteinian /Quantum physics in the early 20th century . If the acceptance or failure of scientific theories relied simply on simple falsification,

according to Kuhn, then no theory would ever survive long enough to be fruitful, as all theories contain anomalies.

The process by which Kuhn says a new paradigm is accepted by the scientific community at large does indicate one possible demarcation between science and pseudoscience. Richard J. Bernstein reads Kuhn as saying that a new paradigm is accepted mainly because it has a superior ability to solve problems that arise in the process of doing normal science. That is, the value of a scientific paradigm is its predictive power and its ability to suggest naturalistic solutions to new problems while continuing to satisfy all of the problems solved by the paradigm that it replaces. Pseudoscience can then be said to be demarcated by a failure to provide such naturalistic explanations, which leads to the labeling of any theory represented as science and appealing to metaphysical explanations for natural phenomena as a pseudoscientific idea.
[edit]

Feyerabend and the problem of autonomy in science

There has been a post-Kuhn trend to downplay the difference between science and pseudoscience, as Kuhn's work largely called question to the Popperian ideal of simple demarcation, and emphasized the human, subjective quality of scientific change. The radical philosopher of science Paul Feyerabend went so far as to claim that there can be found no method within the history of scientific practice which has not been violated at some point in the advancing of scientific knowledge. Both Lakatos and Feyerabend suggest that science is not an autonomous form of reasoning, but is inseparable from the larger body of human thought and inquiry. If so, then the questions of truth and falsity, and correct or incorrect understanding are not uniquely empirical. Many meaningful questions can not be settled empirically -- not only in practice, but in principle.

The problem of demarcation is considered solved by some, for others there is no such thing as an autonomous scientific method , no definitive philosophy of science and no clear and agreed-upon distinction between science and pseudoscience.
[edit]

Examples of Pseudoscience

Main article :List of alternative, speculative and disputed theories

Examples of fields of endeavor that many consider -- to varying extents -- pseudoscientific include Cold fusion, pseudoarchaeology, Gene Ray 's Time Cube, astrology and homeopathy . Pseudoscientific science and medical practices are often quite popular. Medical pseudosciences even sometimes show notable theraputic benefits, possibly due to the placebo effect or observer bias. Many pseudosciences are associated with the New Age movement and there is a tendency to improperly associate all practices of the "New Age" with pseudoscience.

There are also young fields of science that are sometimes frowned upon by scientists from established fields, primarily because they are speculative in nature:
exobiology /astrobiology

Search for Extraterrestrial Intelligence (SETI)
Communication with Extraterrestrial Intelligence (CETI)

These fields are not considered pseudoscientific or protoscientific by most scientists, though, and they are studied at many universities and specialized institutes. SETI and CETI advocates do generally not claim that extraterrestrials exist, although most consider the possibility likely (see Drake equation). There is controversy in biology about whether evidence of extraterrestrial microbial life has been found (fossilized in meteorites and as part of the Viking program 's exobiology experiments).

Certain "watchdog" groups, such as CSICOP , have released statements expressing concern about the apparent growing popularity of pseudoscience, especially when it applies to scientific fields that are intended to save people's lives. A number of self-proclaimed alternative medicine treatments have been designated pseudoscience by critics, largely because some of these methods inspire false hope in terminally ill patients, and end up costing large amounts of money without actually providing any real benefit, treatment, or cure for various ailments.
[edit]

Non-Pseudoscience Nonsense

There is a subset of what is often called pseudoscience which differs from what has been here termed pseudoscience. Most of these are mathematically based, and the problems are often phrased with tempting simplicity. They often live in a closed system of assumptions and premises, and depend on a faulty interpretation of the rules of that system. While pseudosciences have merely failed to prove themselves true, these undertakings can be proven impossible.

The ancient geometric problems of trisecting an angle using only a straightedge and compass , and of drawing a square with the same area as a given circle (or " squaring the circle ") are examples of this kind of problem. Some say that the inventions purporting to illustrate perpetual motion also fall into this group. The latter appears with such frequency that the U. S. Patent Office has a policy not to consider patent applications of this sort (see perpetual motion for more information).

Because its success does not depend on empirical evidence from the "real" world, some scientists do not consider mathematics to be a science. In that context a violation of the rules of mathematics cannot be pseudoscience. Those scientists who are mathematicians however, would say instead that the correct technical term for something violating the rules of mathematics would be 'wrong'. Mathematics differs from the other sciences in that it is based on proof , which (mathematicians say) provides a much higher degree of certainty than can be afforded by experiment (though some experimentalists disagree).

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174) Naudin's Excess power

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Highly impressive findings of Jean-Louis Naudin have been reported more that one year ago, as described in unit #73. In reading this unit again I see that something very important was not emphasized. Naudin's experiments use ordinary water and they generate excess powers of hundreds of watts. This can be contrasted with excess powers smaller than one watt reported to be reproducible by other electrochemists. In one case the input was 1200 W input while the output was 3093 W output. The excess power of 1893 W simplifies calorimetric measurements; the amount of generated heat is calculated from the amount of evaporated water. (If the value of 3093 W were calculated from the weight of lost water then the true excess power would be even larger because some excess heat is likely to be lost through convection and conduction.

The powers are high because the currents of up to 10 A are forced to flow through the electrolyte by differences of potentials exceeding 100 V. The law of conservation of energy does not allow the output power to be larger than the input power, unless some exothermic reactions are taking place. Our students often verify that the amount of heat released and the amount of electric energy supplied, to a wire in a calorimeter, are essentially equal. If a student reports that the heat released is larger than the electric energy supplied then I know that some kind of experimental or computational error was made. Such conclusion, however, would not be necessarily valid if chemical reactions were allowed to take place in the material through which electric charges are flowing. For some reactions the excess heat would positive and others it would be negative.

In electrochemical cells used by Naudin excess heat is positive and this indicates that exothermic reactions are dominant. If the excess heat were negative one would know that the dominant reactions are endothermic. But are these reactions chemical or are they nuclear? Unfortunately, the origin of excess power is not discussed by Naudin. By naming his cells "cold fusion reactors" he implies that reactions are nuclear. But this is far from being obvious. After posting Unit #73 (last year) I advertised it on Phys-L, a discussion list of physics teachers. I wrote: "Reasonably reproducible data on abnormal excess heat are described, as summarized in item #73. The French author, Jean-Louis Naudin, calls it a 'replication' of Mizuno's experiment in Japan. I was impressed." Responding to this one teacher wrote: " The cynic sees that the electrolyte is heavy in oxygen and that the plasma reaction produces scum. It would be well to address conventional avenues for the excess heat noted -- say burning a cell component in oxygen....." This teacher thinks that Naudin has no right to say that reactions are nuclear without showing that they are not chemical. As far as I know (by browsing the Internet) the issue of the origin of excess heat in Naudin's experiments has not been addressed. Why is it so? I have no answer.

Let me insert here a brief description for those who might be interested to access information on Naudin's web pages. These pages are highly impressive not only in terms of content but also in terms of its graphical form. But the way of browsing through them might not be obvious. Start with his "summary page" at:

http://jlnlabs.online.fr/cfr/html/cfrdatas.htm

Note that each picture on that page is a link to a more detailed description. Subsequent pages contain similar impressive pictures but these pictures are not links. Some people might not be aware of this. They can also be confused by some underlined phrases. The phrases look like conventional links but clicking on them brings no now information. In other words, the summary page is also a menu leading to fifteen different descriptions. The first of them, containing 11 pages,

is entitled “cold fusion reactor tiny,” the second, containing 8 pages, in entitled “cold fusion reactor v1.4,” etc.

The amount of published data is very extensive. If I were a chemist I would try to replicate Naudin experiments and analyze reaction products. Measuring input power and output power should not be difficult, provided a power supply, and appropriate electrodes, are available. But the issue of calculating chemically released heat, on the basis of the amounts of identified byproducts, would be too difficult to a me. Will I find a chemist willing to participate? This remains to be seen. Showing that Naudin’s excess heat is due to well know chemical reactions would imply that it is not due to nuclear reactions. This would not ruled out a possibility that nuclear processes identified by Mizuno, Ohmori and Oriani, etc., also take place but at rates much smaller than necessary to generate measurable amounts of excess power.

Unlike Naudin, these authors were able to identify nuclear reaction reactions signatures, such as highly unusual isotopic ratios and alpha particles. Let me finish this essay by summarizing the Ohmori and Mizuno paper; it can be found in a special collection of papers from the Infinite Energy magazine at:

http://www.infinite-energy.com/iemagazine/issue20/index.html

Note that only some of these papers are devoted to cold fusion. Eugene Mallove, the late editor of Infinite Energy, supported all sort of unusual claims. Ohmori and Mizuno analyzed a tungsten cathode “electrolyzed at high power.” In that cathode they discovered elements, such as Fe, Cr, Ti, Ca, Ni, C, Re and Pb, that were not present before the high power electrolysis. More significantly, the isotopic ratios for these elements were often very different from the ratios in our natural environment. ^{50}Cr, for example, had the abundance of 6.1%, versus 4.31% found in nature. Likewise, ^{207}Pb, was found to be 55% abundant versus 22% in nature. Production of heavy isotopes indicates that nuclear reactions “are not necessarily conventional deutron-deutron fusion.” Naudin attempt to show that nuclear reactions are present (by detecting gamma rays from the products) was not successful. Nuclear reactions with which we are familiar often produce radioactive gamma-radioactive isotopes while cold fusion products seem to be mostly stable. Obtaining nuclear energy without producing radioactive waste would be highly desirable.

P.S. (10/2/04).
A chemical reaction that should not be ignored is electrolysis of water. It takes 118 kcal (494 kJ) to decompose one mole of water (18 grams). How much water is decomposed in a typical experiment? The energy equation should be written as:

$$Eel = W + Qv + Qc$$

where Eel is electric energy supplied, W is work of decomposition (1.25 eV per molecule), Qv is thermal energy lost through evaporation, and Qc is thermal energy lost through conduction, convection and radiation. Hydrogen is a fuel; why is W not counted as a component excess energy? Also why was Qc ignored? I suppose that Qc is much smaller that Qv when experiments are performed in a thermos. But in an open beaker Qc is certainly much larger. I think that Qc should also be counted as excess heat. In other words, excess energy reported by Naudin is only a lower limit; how large would it be if both W and Qc were added? The main question, after establishing reality of excess heat, is to account for it in terms of products (either chemical or nuclear).

What follows is a set of messages fetched from the discussion group of NFR users:

http://groups.yahoo.com/group/cfr_project

I joined the group expecting to ask questions about the origin of excess energy. But the group appears to be inactive; the last message was posted more than half a year ago.

Date [of message 1 below]: Sat Feb 7, 2004 2:27 pm
Subject: Source of excess energy [in Naudin’s CFR].

Message 1:

Tom, It's already been checked. It's not coming from the tungsten reacting with oxygen or any of the other chemicals present. As I said before, *read the archives*, this work has already been done.

Message 2:
Greetings all,
After many hours of testing and retesting I have come to the conclusion that the excess energy must come from the destruction of the electrode. The byproducts left at the bottom of the solution are artifacts of tungsten that are at a lower energy level. If we figure in the power used in the production/refining of the tungsten electrode there would be a net loss rather than a gain. In effect we have not produced overunity, rather a conversion of matter into energy and a byproduct. As we all know, in any conversion there is some amount of loss in the process. If we come up with an electrode that produces the same effect and remains at the same mass we can claim overunity. Until then it will remain an interesting experiment and no more. I welcome any feedback...

Message 3:
I think you missed the focus or I was not clear enough in my post. I was considering the energy used in creating the tungsten electrode as the part that tips the balance. As you say the chemical portion of the reaction produces a small fraction of the energy released. I don't know how the energy release could be that small. It is my understanding that matter changed to energy takes a bit of power input and releases quite a bit of energy. Has anyone done any work on the artifacts (chemical makeup)? In all fairness any material used up and replaced has to be figured in to get the final, or better, the net power output. On a cost VS production we are creating pennies of extra power for dollars of destroyed materials. Even if the gain was 300% you would not break even on the cost of production. Only if we have a material for the electrode that remains intact, or the cost of energy increases 10 fold or more, will we have gain over the cost of production. What are your COP results for your experiments?

Message 4:
I think your stuck on the chemical reaction. Forget the reaction and concentrate on the cost of materials used verses the extra energy produced. And as for the archives, anyone can post something to the group. Because it gets posted doesn't mean it's true... By the way I didn't see your experimental results in your post. Have you done the experiment yourself? Maybe you just want to debate? I don't have the time to debate pie in the sky. I need real input from real experimenters with real data...Remember, concentrate on the cost of production verses the amount of extra energy not the reaction! Pennies of extra energy for dollars of materials. I can't make it clearer than that without sounding like I'm teaching elementary school children.

Message 5:
The price you have quoted is for tungsten ore. Then you have to process it. A very costly process that uses a lot of energy (consider the melting point of tungsten). Then ingots of raw tungsten are heated again and another purification is done which uses more energy. Then forming machines (using energy) pound the hot tungsten into the shape desired using energy to keep the material hot during the process. Then it has to be shipped and you have to drive your car to the supplier to buy it. Another factor is the chemical for the electrolyte that has to be processed and shipped and so on. And let's not forget your own energy expended to make it all happen. Don't get me wrong, the experiment is quite amazing and we do get more energy out than we put in (from the reaction). But keep in mind that this is only true if electricity alone is consumed and nothing else. As for searching on google, I was not looking for anything so why would I need to do a search? As for attitude, when you have all your ducks in a row, you too can have attitude... (as the they say in a song "Don't try to describe a KISS concert if you have never seen one") My point again: Pennies of extra energy at Dollars of cost is not overunity.

Message 6:
Tungsten costs ~$50 per metric ton.
http://www.google.com/search?hl=en&lr=&ie=UTF-8&oe=UTF-8&as_qdr=all&q=tungsten+price+%24+ton+metric

Considering the relative inexpensiveness of the bulk metal (which is probably in powdered or > granulated form), I expect that bulk purchases of rod, wire, bar or other forms will be considerably cheaper than buying welding rods, which are simply the most convenient form of tungsten available to make a cathode with. The price of said rod is

obviously dictated by market forces, not by the cost of the base metal. However, these matters are immaterial to the science behind plasma electrolysis and are instead of a manufacturing nature. As far as my own experiments are concerned, I am waiting on a step up transformer. When I am done I will have all my reviewed by some off list colleagues before I publish. It appears you want this material spoon-fed. Frankly, I don't have time to hand-hold someone who is so lazy they won't do a basic Google search *and* who on top of it all gives me attitude too boot. Roland,

Message 7:
If you want small quantities, tungsten powder is available for $8 a pound.
http://www.tungsten-heavy-powder.com/Tungsten_Heavy_Powder/Tungsten_Heavy_Powder_Products/tungsten_heavy_powder_products.html

This is the only listed price I've found on the net and is probably quite a bit higher than the market price, given the nature of the source. So I wouldn't be surprised if my initial assessment of bulk tungsten metal being $50 a metric ton as accurate. $50/ton is probably a price for a minimum 100 ton order off a bulk container ship, however...At any rate, if you want large quantities of tungsten stock then good luck on trying to find a price listed on the internet, say for a 10kg billet or 3m of 14mm diameter rod. You can't, because these items are usually sold in industrial quantities and the price probably fluctuates with the market spot price. You probably couldn't even buy those quantities if you wanted to and I expect most companies wouldn't bother with that small an order. This is because small quantities of formed tungsten (billet, bar, rod, wire) are simply not purchased by the general public except in the case of welding rod, there is no end user market for tungsten products except maybe for shotgun pellets for reloading. Tungsten is simply not used for tool stock, etc. as other materials are better suited.

Granted, tungsten is hard to work given the high melting temperature, but that's why it's usually sintered vs. melted & poured into various shapes; so industry has already found a solution to what would be the expensive energy requirements of working the metal. At any rate, the main impediment to the CFR (or any energy device) being put into practical production isn't the cost of materials, it's a dogmatic and entrenched scientific bureaucracy followed by entrenched energy production interests - and their lobbying groups

Message 8:
Thomas, the real cost you need to assess is the recycling cost of the electrodes. Sure the tungsten is expensive first time round, Subsequently, through, it should be cheaper to recycle it rather than dig it out of the ground again.Then again, maybe not, but worth debating.Note that the theoretical chemical energy released is exactly the energy required for recycling the tungsten. Pragmatic inefficiencies will, of course, increase the required energy. However, if recycling energy, including inefficiencies, is less than the energy released in the reactor, then we have net energy gain.

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175) Excess heat is not chemical

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Jean-Louis Naudin, whose experiments were described in unit #174, is probably not a chemist. How else can I understand the fact that the origin of excess heat was totally ignored? He does not claim to discover anything new, he simply confirms that excess heat, reported by Mizuno et al., can easily be demonstrated with widely available instruments. This is a very important contribution. If I were in Naudin's position, I would also say that, "according to Japanese scientists, less then 1% of excess heat could be attributed to chemical reactions." Only this context makes "cold fusion reactors" scientifically important.

The paper of Japanese scientists, in which experimental work is discussed in the context of the energy conservation principle, is shown below. Only the text is shown; the entire paper (with two figures and the table) can be downloaded from the library at <www.lent-canr.org>. Will the claim made in this paper withstand the scrutiny of good chemists? I do not know. If it does then the puzzle of excess heat will be taken much more seriously.

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Conference Proceedings Vol. 70, ICCF8 Società Italiana Di Fisica, Bologna, 2000, p. 75

Confirmation of heat generation and anomalous element caused by plasma electrolysis in the liquid

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Abstract

Plasma was formed on the electrode surface in a liquid electrolyte when a metal cathode was polarized in high voltage electrolysis in the solution. During the plasma electrolysis large amounts of heat are sometimes generated. The heat can exceed input substantially, in some cases by up to 200 percent of input power. At the same time, anomalous elements are detected in the electrolyte and on the electrode surface. Based on the heat and the product, we hypothesize a nuclear reaction can be induced by photon activation on the cathode element.

1. Introduction

We previously reported that anomalous isotopes are created on metal surfaces and surface regions, which cannot be explained by ordinary electrolysis.1 In other words, this extruded material consists of elements, which cannot conceivably be contamination, because the isotopic distribution of most of the elements is unnatural. It is thought that this indicates what type of reaction affecting the nucleus has occurred. We expect to clarify the reaction mechanism by analyzing these reaction products. The plasma phenomenon has been in the literature [2-9] for quite a long time. The

reaction mechanism of plasma discharge is not understood in as much detail as ordinary electrochemical reactions.

2. Experimental

Method 2.1 Electrolysis system
Heat was measured by a method that combined open cell isoperibolic calorimetry and flow calorimetry. The cell is a cylindrical glass vessel, 100 mm in diameter and 150 mm tall. A magnetic stirrer to ensure uniform temperature distribution mixes the electrolyte. It has two layers, or chambers. The anode, cathode and electrolyte are placed in the inner chamber. The outer chamber is filled with primary cooling water and heat exchanger coils (Teflon tubing). During electrolysis, secondary-cooling water flows through the coils and removes heat from the primary cooling water. Thermocouples are installed in the heat exchanger coils inlet and outlet, in Tee fittings, to perform flow calorimetry. Cooling water is circulated through the Teflon tubing from a constant temperature bath with an FMI precision metering pump with 1% precision; for example, 4.26Å}0.05 g/s. This pump circulates water at any rate from 1 to 20g/s.
The flow rate is verified by collecting the water in a flask for a fixed period of time and weighing it on a precision scale.

2.2 Heat calibration The heat balance is obtained with the following formulas:

Input (Joules) = I (ampers)*V (volts)*t (seconds)

Output (Joules) = Hw (heat of solution and cell) + Hc (heat of coolant) +
Hr (heat release) + Hv (heat of vapor) + Hg (heat of water decomposition)

All data, including voltage, current, temperature, and mass of secondary cooling water, are collected with a data logger for each 10s. Current is determined by measuring voltage through a shunt. Cell voltage is measured by attaching sensor wires directly to the electrode leads in the top of the cell. Data from the logger is captured by the computer, and finely recorded on diskette. All electric lead junctions from the thermocouples to the logger are compensated.

The heat balance was calibrated by changing the input power of the Joule heater immersing in the electrolyte. The out/input ratio stayed at 0.89 during the calibration run except when the cell was boiling, and during glow discharge boiling seldom occurred. The results show that a large change in input has no effect on the heat recovery calibration. In other words, changes in input power can be precisely monitored by the flow calorimetry method and by the open cell isoperibolic method. The only difficulties arose when the input power is very low, because the temperature precision was only 0.1C, and below 70W measurement accuracy suffered.

2.3 Electrolyte and Cathode Material Preparation
The electrolyte solution was prepared with Milli-Q pure distilled water that is filtered. It was used after being redistill in quartz glass. Ultra high pure reagents were used for the K_2CO_3 electrolyte. High purity of tungsten plate (99.98%) was used for the cathode. Platinum wires or mesh were used as the counter electrode and electrode lead wires.

2.4 Analysis of generated element
Materials in the solution and deposition bottom of the cell were calibrated by ICP analysis. The element on the electrode surface was analyzed by the EDX. The elements were then melted by an acid and calibrated by ICP method.

3. Results
When we increased the input voltage, excess heat was clearly generated. A typical result is shown in Figure 1. With a tungsten electrode of 0.5mm thickness; the graph on the left shows changes of input current, voltage and the electrolyte temperature during electrolysis. The figure on the right shows the input and output in watts and the ratio between them. The output heat showed slight excess comparing with the input power after 2,000s of plasma electrolysis start. After the 3,000s, the excess was considerably larger than the heat measurement error of 20W and reached 30-40W of average during the plasma electrolysis.

Figure 1. Result of small excess heat of 0.5-mm thickness Tungsten plate in 0.2 M K_2CO_3.

Tungsten samples of 0.3-mm thickness also produced significant excess heat generation. A typical result is shown in Figure 2. Excess heat was observed after only 100s of plasma electrolysis. At the time, the excess was considerably larger than the heat measurement error of 20W, reaching 40W average for the first 1,500s of plasma electrolysis. The output and input ratio changed over time. For the first 1,000s, excess was 20%, but after 1,600s it increased to around 60%, with fluctuations. The ratio again increased up to 300% after 3,000s then fell back to one after the voltage decreased to 100V. The total output heat power during plasma electrolysis was estimated as 388.7kJ while the total input electric power of 334.3kJ. Excess heat is 54kJ or 16% of input power.

Figure 2. Result of large excess heat generation during plasma electrolysis of Tungsten electrode.

After the voltage fell to 100V, the output and input ratio seems to fall gradually below unity. The ratio slowly increased after the voltage decreased into 60V. A possible reason is that the electrolysis temperature rose close to boiling point during glow discharge, and heat was lost to the water vapor released from the cell. Although we did not measure heat lost to vapor, the overall balance during glow discharge remained well in excess of unity because so much excess heat was generated. After the voltage was lowered, excess heat stopped although large losses to vapor continued, so ratio fell under unity. At 60 volts, glow discharge stopped, the boiling ceased, and the heat losses to vapor declined, so the balance returned close to unity.

It has been established that at the extraordinary excess heat generation is sensitive to many parameters, such as electrolysis and electrode material. Especially, it depends on the electrolysis temperature, input voltage and the duration of plasma discharge. We observed no excess heat at the beginning of the plasma electrolysis even the temperature and the input voltage were quite high, at 100V and 85ºC. However, after several hundred seconds of plasma electrolysis, we always observed large excess heat generation. We conclude that if we attain sufficiently high temperature and voltage, and hold them, we observe excess heat with 100% reproducibility.

It is apparent that excess energy production depends on electrolysis voltage and the temperature of the electrolyte solution. The discharge voltage at which excess heat production begins is quite different as these conditions vary. Even when these conditions are satisfied, other parameters, such as the shape of the cathode, cause dissimilar discharge conditions. The spectrum of light from the reaction shows that two kinds of reactions can occur. [10] A reaction in which hydrogen dominates in the spectrum, or a reaction in which the alkaline in the electrolyte and the cathode metal, which have no direct role in the reaction, are more pronounced in the spectrum. In the latter case the electrochemical potential near the electrode is large. We assume this affects the excess heat generation.

After excess heat was produced, many elements were found in the electrolyte, the eroded and precipitated cathode material, and on electrode surface. On the other hand, there were fewer elements when no excess heat was measured. Several elements were detected on the cathode surface after excess heat evolution, including Cl, Ca, Ti, Fe and Zn. The distribution of the elements was different in different parts of the electrode surface. Most were found around the center of sample. Meanwhile, other elements were detected in the solution, which were not the same as those found on electrode surface and the deposition at the bottom of the cell. Typical element distributions are shown in the Table 1. A clear difference can be seen between the case when excess heat was generated, and when it was not.

4. Discussion

In evaluating heat production, is necessary to consider whether the heat might have been produced by chemical changes. After discharge electrolysis it is clear that the cathode material was worn down and precipitated to the bottom of the cell in the form of fine particles. Because this material was entirely made up of pure metal, the destruction must have been caused by hydrogen corrosion as well as heat damage. Therefore chemical changes cannot explain the heat. Ignoring the fact that this is impossible, let us consider some hypothetical chemical reactions:

$2W+3O_2=2WO_3 + 200.84kJ$
$WO_3+H_2O=H_2WO_4 + 179kJ$

In other words, if 183.85g of tungsten reacted, the maximum heat generation from this reaction would be 380kJ. The

actual volume of excess heat measured using flow calorimetry was 54.4kJ for the case of plasma electrolysis with 200V of input for 3.1ks. The mass of tungsten removed in the reactions described above was 0.1g, and if we assume hypothetically that all of this material reacted, the heat would have been only 0.207kJ or 0.38% of the actual excess heat observed. The heat production in this experiment cannot begin to be explained as a chemical reaction.

This discussion has been predicated on the supposition that the tungsten underwent a chemical reaction, but in reality the tungsten found at the bottom of the cell was recovered as pure metallic fine powder, so no chemical reaction occurred. Furthermore, the decomposition of carbonates in a water solution is an endothermic reaction, so in this case 274kJ/M of heat would be absorbed, therefore even assuming that a reaction occurred no excess heat would be produced. Ordinarily, when metal is transformed into a hydride, it produces on the order of 200kJ/M, the exact amount depending on the metal. If a hydride was formed, based on the amount of tungsten lost from the cathode, the excess heat from the reaction should have been 0.1kJ approximately, which is simply too small to make a difference. In short, the actual heat that might have been produced from potential chemical reactions is essentially nonexistent.

5. Conclusions

According to many reported experiments, in almost all cases no radioisotopes have been detected, and no radiation or only very weak radiation such as neutron and other gamma or x-rays have been detected during and after the cold fusion experiment. We have to explain the fact by a plausible mechanism that produces no radioactive materials and might be trigged by the the electrochemical reactions. Takahashi [11] has made progress the mechanism of photo-fission for the case of palladium, the material usually used in cold fusion experiments. We understand that the Takahashi hypothesis also applies to a tungsten electrode.

Table 1

There are two peaks for the element distribution of the product of Tungsten electrode as shown in Table 1. One peak is the major element of iron and zinc and other is the In. The total generated amount of element for the case of excess heat evolved was calibrated as the order of a milligram. Meanwhile, the total excess heat was calculated as on the order of 10^6J from the products. The mechanism is well explained the excess heat evolution. We can say the photo-fission mechanism explain the amount of excess heat and the distribution of the element generation during the electrochemical treatment.

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176) Very convincing results

Ludwik Kowalski (September 19, 2004)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Two days ago I received an e-mail message from a cold fusion researcher, Dr. Richard Oriani. It was prompted by my letter to the editor of Physics Today.

"I read your recent letter to Physics Today with interest. I hope that the DOE panel will answer questions to your satisfaction. I presume that you know that I for one am certain that the phenomenon that is poorly labeled as cold fusion is real. I would like to send you two papers for your consideration, having to do with alpha particle generation. I need your full address for that mailing. I remain highly frustrated by irreproducibility despite having tried hard. I regard the solution of this problem as very important for acceptance of cold fusion by the nuclear community. I suspect that progress in developing reproducibility depends on developing understanding of the basic physics, and this in turn depends on developing reproducibility so that control of the relevant parameters will be possible. A chicken and egg dilemma !"

As described in unit #108, I met Richard Oriani at the last cold fusion conference; he is a retired teacher (like I am now) in Minnesota. Oriani probably forgot my name and is not aware that I am the same person who corresponded with him about a year ago. Are the papers he is sending me now new or are they the same as those presented at the last cold fusion conference? I will know in a day or two. Meanwhile I reread his last year papers and decided to summarize one of them. It describes an experiment that seems to be suitable for student projects. The papers shows that unexpected charged particles are emitted in a very simple electrochemical cell. The idea of using ordinary water and a nickel cathode (instead of less accessible heavy water and palladium) is very attractive.

The experimental setup is essentially a test tube containing a solution of Li_2SO_4 in water (concentration 25 grams/liter) and two electrodes: Pt (anode) and Ni (cathode). An electric current, such as 0.2 A, flows through the cell for several days. An identical setup is used in a control experiment; the current in that experiment is zero. Energetic nuclear projectiles, most likely protons or alpha particles, were recorded with CR-39 detectors immersed in the solution. The number of observed particles in the control tube were found to be significantly lower than in the tube through which the electric current was flowing.

Unfortunately results are not reproducible; track densities fluctuate widely from one observation to another. But the average track densities from control cell were much lower that form the cell in which the electrolysis was taking place, as illustrated below. The cause of fluctuation remains unknown and the author is "highly frustrated by irreproducibility despite having tried hard." Recognizing "hidden variables" in cold fusion experiments is the number one task for those who conduct research in that area of science. Let me mention that electrostatic experiments were also highly irreproducible before air humidity was recognized as a hidden variable. The same was true for semiconducting devices, before the importance of extreme cleanness was recognized.

The first column below shows the outcomes from 32 measurements made in the cell with the current while the second column shows the outcomes from 16 measurements made in the cell without the current. The sorted numbers represent tracks per square centimeter (and per equal time of one run, I suppose). How can there be any doubt that the first set of data is very different from the second? The average from the first column is 469.7 while the average from the second is 165.2. The corresponding median values are 328.5 and 136.0, respectively. This indicates that the distributions are not

Guassian.

With current	Without current
1405	64
1355	338
1202	272
993	260
982	260
922	160
898	143
741	142
532	130
532	130
510	107
402	95
390	95
390	83
335	78
332	35
325	mean=165.2
307	median=136.0
283	st.dev.=98.8
272	
237	
221	
221	
213	
202	
185	
141	
120	
107	
98	
95	
83	
mean=469.7	
median =328.5	
st.dev.=383.7	

A skeptic might suspect that excess tracks observed in CR-39 detectors are not due to swift nuclear particles but to something else, such as bubbling, associated with the current. This possibility was investigated by Oriani; detectors subjected to intensive non-electrolytic bubbling did not reveal more tracks than detectors used in the control cell. Experimental data in which heavy water and palladium were used produced equally significant results, as described in papers of Oriani at al. These papers can be downloaded from the library at <http://www.lenr-canr.org>.

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177) List of Proceedings of International Conferences on Cold Fusion (ICCF)

Extracted from a document prepared by P. Hagelstein et. al

Ludwik Kowalski (September 20, 2004)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Proc. ICCF1 (1990):
Proceedings of the First International Conference on Cold Fusion,
Mar. 1990 Salt Lake City, UT, edited by F.Will.

Proc. ICCF2 (1991):
The Science of Cold Fusion, Proc. of the Second Annual Conference on Cold Fusion, Como, Italy, June 29-July 4, 1991, edited by T. Bressani, E. Del Giudice and G. Preparata, Published by Societa Italiana di Fisica, **33**.

Proc. ICCF3 (1993):
Frontiers of Cold Fusion, Proceedings of the Third International Conference on Cold Fusion, Oct. 1992 Nagoya, Japan, edited by H. Ikegami, Universal Academy Press, Tokyo.

Proc. ICCF4 (1993):
Proceedings of the Fourth International Conference on Cold Fusion,
Dec. 1993 Maui, Hawaii, edited by T. O. Passell and M. C. H. McKubre.

Proc. ICCF5 (1995):
Proceedings of the Fifth International Conference on Cold Fusion, Monte Carlo, Monaco April 9-13, 1995, IMRA Europe, Sophia Antipolis Cedex, France (1995).30

Proc. ICCF6 (1996):
Proceedings of the Sixth International Conference on Cold Fusion, October 1996 Hokkaido, Japan, edited by M. Okamoto.

Proc. ICCF7 (1998):
Proceedings of The Seventh International Conference on Cold Fusion,
Vancouver, Canada, April 19-24, 1998, ENECO, Inc., Salt Lake City, UT (1998).

Proc. ICCF8 (2000):
Proceedings of the Eighth International Conference on Cold Fusion, May 2000 Lerici (La Spezia), Italy, edited by F. Scaramuzzi.

Proc. ICCF9 (2002):
Proceedings of the Ninth International Conference on Cold Fusion, May 2002 Beijing, China, edited by X. Z. Li.

Proc. ICCF10 (2003):
Proceedings of the Tenth International Conference on Cold Fusion,
August 2003 Cambridge, MA, edited by P. L. Hagelstein and S. R. Chubb,
World Scientific, to appear.

Proc. ICCF11 (2004):
To take place in November, 2004, Marseilles, France (see http://www.iccf11.org).

I was not able to locate a place at which all proceedings are available to a researcher. Please contact me if you know about such place. I will append that information here. Suspecting that such places do not exist, I would like to suggest that a person having the proceedings, and willing to donate them to a library, contact me. I will try to find a permanent home for such donation. These materials should be preserved for future generations. I will contact Niels Bohr Library about this.

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178) Recent messages and comments

Ludwik Kowalski (September 21, 2004)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

I often receive cold-fusion-related messages. Let me share some of them here.

1) On September 18, 2004 a stranger wrote: “I refer to your Physics Today letter concerning ‘Seeking Answers From Cold Fusion Review.’ The Golden Rule of science that I was taught was: Reproducibility. If a phenomenon can't be reproduced, it cannot be measured, it cannot be counted and is therefore inaccessible to analysis by mathematics. If a phenomenon can't be corroborated it will not be accepted by a consensus of scientists working in the field.

2) My immediate reply was: “I agree with you. Cold fusion is not an established scientific discipline. Neither was electrostatics at the time of B. Franklin. The main hidden variable at that time was humidity. That is why experiments were highly irreproducible. Do you think that cold fusion findings reported by qualified scientists (in the last five or ten years) are based on fraud? Do you think that methods of validation used by them are different from those used in other areas of physics and chemistry? Do you think that they are ignorant charlatans or highly skilled con artists?

Who are you and why are interested in cold fusion? I am a recently retired physics teacher trying to find a way to check some claims made in the area of cold fusion. I will not hesitate to say that my data contradict the reported claims, if that is going to happen. The null hypothesis is going to be that claims are not valid (because they are so extraordinary). I hope to hear from you again.” Today is September 21; I do not know why that person decided not to answer. But that is OK with me.

3) I asked a friend in France about the attitude of French scientists toward cold fusion. Replying to my message (on September 20) he wrote: “People here are quite reluctant - as I already told you - concerning cold fusion. At the beginning there had been much excitement, but at that time all serious experiments which were performed showed that the effects observed were due to background. I just have seen, and read, your letter to Physics Today (it arrives much later on this side of the Ocean); it is quite all right... “

During my trip to France, to participate at the 11th International Conference on Cold Fusion (see http://www.iccf11.org>), I will ask what my friends know about recent cold fusion claims. I suspect that many of them think that cold fusion is pseudoscience, not worth their time. In other words, the situation there seems not to be different from that in the US. The word “reluctant” is interesting. Does it mean they are ready to change their minds when (and if ?) convincing experimental evidence is presented in peer reviewed papers? I hope so. Most scientist are open-minded.

4) Several days ago I discussed a cold fusion topic with someone over the Internet. After answering my questions (on September 17) that person added: “I suspect that many would be reluctant to become involved in CF because of its stigma as fringe science. This stigma may or may not be deserved. The point of any experiment is to answer a question, and in my personal but far from universal opinion there is something to be learned by further research into electrochemical CF. So, do you have any suggestions? What has been your experience with colleagues?” I said that I understand the fears of being negatively labeled. Then I resumed our discussion about instruments to be used.

Responding yesterday (September 20) he wrote: “I received your message. Sorry about the delayed response; I had to give this some consideration over the weekend. I believe that there are a few minor misunderstandings in what you say that you learned from me, and I'll address these in another email if you wish. If you want to include our URL in your

item, that is OK, but I would prefer that you not use my name. This is just to put a little public distance between me and CF experiments..... " Hmm, a person willing to discuss nuances of an anticipated CF experiment in private is reluctant to associate his name with that topic publicly. I know that he is an open-minded individual. But he is also realistic; he knows what others think about cold fusion and he does not want to be responsible if his institution is labeled as pseudoscientific.

5) On September 20 a stranger wrote: "Hi, I've been following your site with interest for a few months. Bob Park is bothered by the anonymity of the new DOE panel reviewing cold fusion. (see below). The first DOE panel on cold fusion was not anonymous. However, anonymous peer review is the norm for many prestigious journals. Maybe if the first panel had remained anonymous cold fusion studies would not have been so stigmatized." He then pasted what Bob Park wrote (on September 17 at <http://www.aps.org/WN/>) about the DOE investigation. Here is that piece:

"COLD FUSION: DOE REVIEW IS HIDDEN BEHIND A CLOAK OF SECRECY. Believers see DOE's review as vindication after 15 rough years (What's New, 2 Apr 04). But watchers are puzzled by how little is known about the process. Who are the reviewers? Who are they talking to? What's New hears that DOE is claiming anonymous peer review. That shouldn't please anyone. The controversy will simply continue. Bob Park can be reached via email at whatsnew@bobpark.org THE UNIVERSITY OF MARYLAND. Opinions are the author's and are not necessarily shared by the University, but they should be."

6) This interesting message shows that information about the panel composition (which was shared with me by X) is not widely known. I do know when the deliberation took place and who was present. This is likely to become public knowledge very soon. I already commented on what Robert Park wrote about cold fusion in his book "Voodoo Science." It's a good book but I disagree with Park's idea of placing cold fusion researchers in the same category as astrologists, UFO-logists and con artists. One should not be surprised to discover con artists pretending to be scientists; there so much to gain and so little to lose. Bob, and his Russian counterpart, Krugliakov, did expose many pseudoscientific swindlers. And they deserve credit for this.

But Bob's position on cold fusion is wrong. Why do I think so? Because I have not seen anything scientifically-convincing in his accusations. He seems to be sticking to old arguments and ignores new claims (in his book and in What's New internet messages). My impression is that he is totally unfamiliar with recent cold fusion publications. I did not see him at the cold fusion conference last year. He was personally invited by the co-chair, I heard, but decided not to come. That is strange; the conference would offer him an opportunity to learn about new findings, and to engage scientists whom he ridiculed in numerous public addresses. He always presents himself as a scientist. So why was he not willing to discuss cold fusion topics with other scientists? I can understand my fiend in France, he is active in other areas and has no time to read cold fussion litarature. But a material scientist who publicly treats cold fusion researchers as if they were charlatanes has a moral obligation to be seriousely involved. Will Dr. Park come to the conference in Marseilles? I would welcome an opportunity to speak with him, and to write about our conversations. I wish I had his writing skills.

7) A person I met at the cold fusion conference last year wrote to me (on September 15) about a book he is writing. In a reply I said that I would be interested in seeing one or two chapters. The entire draft was sent to me as an attached file. I read parts of it and made some comments. The message ended with the "I wish you luck in publishing the book." The reply surprised me; the author wrote: "I am not going to try to publish it. When it is finished and it has been reviewed by several researchers and friends, I will upload it to .....[web site was given] along with our other papers. I will not copyright it. There is no point. No publisher will touch a book about cold fusion, and people will not pay money to read about it. This goes back to Howard Aiken's dictum: 'Don't worry about people stealing your ideas. If your ideas are any good, you'll have to ram them down people's throats.' " I have no doubt that good books about cold fusion will be in demand, no matter what the final verdict of DOE will be. Cold fusion is not only science; it is also an interesting social phenomenon. The phenomenon might be indicative of something very profoud on the subject of "science and society."

8) I am appending an important paragraph at the bottom of item #177; it has to do with an attempt to find someone willing to donate a set of proceedings from cold fusion conferences to an accessible library. Please contact me about this:
<kowalskil@mail.montclair.edu>

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180) Preserving Cold Fusion Documents

Ludwik Kowalski (October 1, 2004)
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About two weeks ago I wrote a letter to Dr. R. J. Anderson, the Director of the Niels Bohr Library. That library, located in Maryland (near the capital of USA), is the Center of History of Physics of the American Institute of Physics. This item shows two original snail-mail messages and subsequent e-mail messages. My appeal to those of you who might be willing to contribute is shown at the end. Preserving cold fusion documents in a central location, particularly in the prestigious Niels Bohr Library, is worth supporting.

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Dear Dr. Anderson:

I am a recently retired physics teacher interested in cold fusion. You are probably aware that the CF field is very controversial (see my letter to the editor in the last issue of Physics Today, September, 2004). I would like to know what CF materials are available in the Niels Bohr Library. More specifically, I am interested in proceedings from International Cold Fusion Conferences listed below. They are not available in most research libraries, probably because this field is not recognized as scientific. I hope that the Niels Bohr library has the proceedings because they reflect evolution of a unique scientific controversy. If not then I would appreciate information about a library, or institution, that collects and preserves such items.

Proc. ICCF1 (1990):
Proceedings of the First International Conference on Cold Fusion, (ICCF)
Mar. 1990 Salt Lake City, UT, edited by F.Will.

Proc. ICCF2 (1991):
The Science of Cold Fusion, Proc. of the Second Annual Conference on Cold Fusion, Como, Italy, June 29-July 4, 1991, edited by T. Bressani, E. Del Giudice and G. Preparata, Published by Societa Italiana di Fisica, **33**.

Proc. ICCF3 (1993):
Frontiers of Cold Fusion, Proceedings of the Third International Conference on Cold Fusion, Oct. 1992 Nagoya, Japan, edited by H. Ikegami, Universal Academy Press, Tokyo.

Proc. ICCF4 (1993):
Proceedings of the Fourth International Conference on Cold Fusion, Dec. 1993 Maui, Hawaii, edited by T. O. Passell and M. C. H. McKubre.

Proc. ICCF5 (1995):
Proceedings of the Fifth International Conference on Cold Fusion, Monte Carlo, Monaco April 9-13, 1995, IMRA Europe, Sophia Antipolis Cedex, France (1995).

Proc. ICCF6 (1996):
Proceedings of the Sixth International Conference on Cold Fusion, October 1996

Hokkaido, Japan, edited by M. Okamoto.

Proc. ICCF7 (1998):
Proceedings of The Seventh International Conference on Cold Fusion,
Vancouver, Canada, April 19-24, 1998, ENECO, Inc., Salt Lake City, UT (1998).
Proc. ICCF8 (2000):
Proceedings of the Eighth International Conference on Cold Fusion, May 2000
Lerici (La Spezia), Italy, edited by F. Scaramuzzi.

Proc. ICCF9 (2002):
Proceedings of the Ninth International Conference on Cold Fusion, May 2002
Beijing, China, edited by X. Z. Li.

Proc. ICCF10 (2003):
Proceedings of the Tenth International Conference on Cold Fusion,
August 2003 Cambridge, MA, edited by P. L. Hagelstein and S. R. Chubb,
World Scientific, to appear.

CF will be part of the history of physics, no matter what conclusions are reached about the validity of its claims. Leading researchers in that field, such as Steven Jones, Martin Fleischmann, Mike McKubre, and John Bockris are recognized authorities in their disciplines.

Unable to locate the above proceedings in local university libraries, and suspecting that nobody gathers them, I would like to suggest that your library take the initiative to collect and preserve CF documents for the benefit of future generations. Scientists who attended earlier conferences might be willing to give their own copies to the library. They might even be willing to be interviewed by a historian of science.

My own involvement is recent; the only conference I attended was ICCF10, last year. But I want to become a cold fusion researcher. That is why I am going to attend the next conference in France (see http://www.iccf11.org). With your approval, I would be willing to make an appeal urging old-timers to donate their copies of procedures to the library. Please note that such a conference is an ideal place for a historian of science to interview old-timers. Most of them are eager to talk about personal experiences in the CF field. Opportunities for interviewing some of them might not be available in a year or two.

If the library already has cold fusion materials then I would very much like to examine them after the conference. Please let me know if this is possible. Other researchers might become interested in the CF proceedings after the report from the ongoing DOE investigation is published. Attached is my modest donation to help support the library. Respectfully yours, . . .

= =

Dear Dr. Kowalski:

Thank you for your recent letter regarding cold fusion materials and for kind donation to the Friends of the Center for History of Physics. We appreciate both the contribution and the offer to help us obtain copies of the proceedings of the international conferences on cold fusion.

We do not have the proceedings and would be pleased to have you ask participants at the upcoming conference in France for copies on our behalf. We've checked online to determine if any one library has the proceedings for all ten conferences, and we weren't able to locate any that do. However, many university libraries have proceedings for one or a few of the conferences, and the University of Michigan catalog shows holdings for all but the first. For your research, you should be able to ask your local university or public library to obtain them for you through interlibrary loan.

I appreciate your suggestion that we conduct oral history interviews with cold fusion researchers. Unfortunately, however, the Center has limited resources to devote to oral history and a very long list of scientists to interview,

including even some Nobel laureates who have not been interviewed by historians. However, we do offer grants in aid (http://www.aip.orc/history!web-arnt.htm) to historians of science who are interested in conducting interviews with scientists in their own areas of interest, and we can sometimes also offer free transcription for interviews in physics and all of its fields. In addition, the Cornell University Cold Fusion Archives has conducted some interviews.

We do have here videotape recordings of the Special Session on Cold Fusion that was sponsored by the American Physical Society in 1989. Another collection that may be of interest is the Steven Jones Papers at the Marriott Library, University of Utah. I've enclosed a catalog description of the Special Session on Cold Fusion videocassettes, along with descriptions of the Cornell University Cold Fusion Archives and the Steven Jones Papers. I've also enclosed a copy of our Application for Access form. If you would like to use the Special Session videocassettes, please complete the form and send it to us. . . .

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Prompted by the above I started thinking about another initiative; it is summarized in this e-mail message:

Dear Dr. Anderson:

Thanks for the reply. I will make an appeal and hopefully somebody will donate the entire collection to the library. I will let you know. In the letter you say that only financial limitations prevent you from sending a historian to interview one of the cold fusion scientists that I mentioned. I have a question about this. Suppose that during the conference (which I am going to attend) I find a person committed to the cause (preserving cold fusion history) and willing to donate a sum of money. Would you accept a donation which is dedicated to a particular field (but you choose the scientist)? If this is acceptable then I will start asking. Can you please indicate, based on experience, how much it costs, typically? And one more question. Is it OK if the letter I received today is shown to others. They are likely to react positively to my appeal by reading what you wrote. Sincerely yours, . . .

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Here is the reply form Spencer R. Weart, the Center's director:

Dear Dr. Kowalski, We hope your appeal succeeds; the conference proceedings would be a fine addition to the collections of our Niels Bohr Library. Certainly you can show Joe Anderson's letter, and this or any of our other e-mails, to anyone.

We are glad to help support interviews with physicists whenever an appropriate interviewer can be found. In our grants-in-aid program, we will support any qualified historian (someone with publications in history of science, or enrolled for a degree in the field) and let them choose who is to be interviewed. The first problem is thus to find the historian. I don't know any who have been studying cold fusion -- certainly it is a fascinating topic but there are so many other topics to study, and historians usually look at things farther in the past. Thus it would be necessary to advertise in the history of science community and see if we could find, for example, an underemployed postdoc who would like to do it as a job.

Oral history interviewing is surprisingly expensive. For an ongoing program in oral history, there is an incremental cost of about $750 per hour of interview to do the research, make travel arrangements, conduct the interview, and transcribe, edit, and catalog the tape-recording and transcript. Thus a typical three-hour interview would have an incremental cost of $2250. But for a new project there would also be start-up costs (hiring the interviewer and other administrative arrangements, initial research to select the best interview subjects, etc.). A minimum of $15,000 would be needed to initiate a program that would conduct in-depth interviews with several pioneers, perhaps totaling ten interview hours or a bit more. The selection of people to be interviewed would be up to the historian, but of course your advice, and the

advice of others in the community, would be seriously considered. All that said, I can't guarantee we could find a suitable interviewer, but the chances are pretty good.

An alternative would be for you to find someone, not necessarily a trained historian, interested in researching and conducting the interviews as an unpaid volunteer. We could then offer guidance in how to go about it, and could probably provide some travel funding through a grant-in-aid, plus a limited amount of transcription and cataloging. If more than a few interview hours are done we'd need a bit of support for the transcription and cataloging. The problem of course is to find someone who is interested, which would probably be someone in your community, and who would at the same time interview in a thoroughly objective fashion. Sincerely, Spencer Weart

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This item (#180) will be “advertised” on two discussion lists: Phys-L, and Newsletter. The first list is read by many science teachers; they may help to find a historian. such a person should contact me. The second list is read by members of iscmns (International Society of Condense Matter Nuclear Science); I will suggest to them how the collection of conference proceedings might be organized.

Needless to say, I am composing this item for a broader group of potential readers. Contact me if you, or somebody you know, are interested in helping the project. Keep in mind that cold fusion will certainly be considered an important episode in the history of science, no matter what the final verdict about its claims will be. My e-mail address is:

<kowalskil@mail.montclair.edu>.

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181) A new book about cold fusion

Ludwik Kowalski (October 8, 2004)
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Last night I received an advance copy of a just-published book about cold fusion by Steven Krivit. The title of the book is "*The Rebirth of Cold Fusion;"* the second author is Nadine Winocur. My first intention was to browse through the book and then share my first impressions. But I changed my mind, after seeing how the book is introduced by several people. It is better if I show what they wrote in the opening pages:

Advance acclaim for
The Rebirth Of Cold Fusion: Real Science, Real Hope, Real Energy
Steven B. Krivit and Nadine Winocur, Psy.D.
Foreword by Sir Arthur C. Clarke
http://www.newenergytimes.com

1) "*The Rebirth of Cold Fusion* is a very important and timely book. It may help educate a generation of physicists told to believe cold fusion doesn't exist. I look forward to future editions describing a definitive theory of the phenomenon."
- **Robert H. Parmenter**, Emeritus Professor of Physics, University of Arizona, and co-author of "Cold Fusion in Metals" (Proceedings, National Academy of Science, Vol. 86, 1989) with Willis E. Lamb, Nobel prize winner in Physics, 1955

2) "Basic research and its progress depend upon how open the society is to accept new ideas and pursue them to the end - right or wrong. *The Rebirth of Cold Fusion* brings out the important question of whether the present form of peer review and financial control by government agencies help in new innovations or not. It is obvious that some changes have to be made, with the future in view."
- Dr. P. K. Iyengar, Chairman (retired), Atomic Energy Commission, India

3) "The Rebirth of Cold Fusion is an important report in the good tradition of American investigative journalism. It exposes a scandalous case of scientific misconduct, engages those who are guilty of foul play, and sets the record straight. The book comes at a critical moment in which a neglected field of inquiry is fighting for recognition. Let's hope it helps cold fusion gain the support a potential energy source deserves. Everybody should be aware of the facts in this book.
- **Haiko Lietz,** Science Reporter, Germany

4) About Cold Fusion
"The only thing pathological about cold fusion is the way the scientific establishment has treated it." -
Sharon Begley, "Cold Fusion Isn't Dead, It's Just Withering From Scientific Neglect" (Wall Street Journal), Sep. 5, 2003

5) "No cover-up like this has happened before. It is a profound scandal in American science."
- **Charles Beaudette**, author, *Excess Heat & Why Cold Fusion Research Prevailed*, 2002

6) "In regard to cold fusion, it would be advisable for the scientific community to brace itself for the fallout that will be coming soon when the public starts to become aware that the scientific community was engaging in an act of gross self-deception back in 1989."
- Brian Josephson, Nobel prize for physics, 1973

7) "Cold fusion may provide a clean nuclear energy to mankind. Since I have worked long in nuclear engineering, it is a dream of nuclear energy."
- Akito Takahashi, professor, department of nuclear engineering, graduate school of engineering, Osaka University

8) "If Professor X.Z. Li [Tsinghua University, China] is correct, then I'll have to throw away about 14 of the 16 chapters in my book *Introduction to Fusion Energy*, because it will no longer be relevant to the kinds of fusion that could result from this 'cold fusion' process."
- Dr. J. Reece Roth, head of the industrial plasma engineering group, University of Tennessee

I am still open-minded about cold fusion claims. At present I am more interested in papers of research scientists than in books focusing on social and historical aspects of cold fusion phenomenon. That is why I will wait a while before reading this new book. Let me mention another item, a paper, not a book, that is worth reading. It is available over the Internet at:

http://www.newenergytimes.com/library/2004JosephsonB-LindauLecture.pdf

The author, Brian Josephson, is a Nobel Prize Laureate. He is a physicist but this paper is a set of philosophical reflections. Josephson writes about validation of scientific claims, and about social aspect of cold fusion. Note that he is one of those who was quoted above.

Josephson's paper can also be downloaded from the library at www.lenr.canr.org

http://lenr-canr.org/acrobat/JosephsonBpathologic.pdf

The most important cold fusion papers (organized alphabetically by the first author) can be found in that library. Its downloadable pdf files are usually not available anywhere else. The library is a widely used resouce for cold fusion researchers.

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182) With my own eyes...

Ludwik Kowalski (October 13, 2004)
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Montclair State University, Upper Montclair, NJ, 07043

Yesterday was a highly exciting day for me. I discovered a lot of tracks of charged particles on CR-39 detectors applied to a cathode provided by Dennis Letts. How can I describe that "day of glory"? One way is to show relevant e-mail messages, some old and some recent. Let me begin by mentioning that item #169 was based on work by Dennis Cravens and Dennis Letts. My detailed results and pictures will be presented in unit #183.

8/21/2004 (from L.K. -->Dennis Letts)
Dear Dennis and Dennis: I read your "Appeal to Researchers" and I would like to be involved. I am Ludwik Kowalski, a physics teacher at Montclair State University in New Jersey. I am interested in cold fusion. ..... What I would like to do, as soon as possible (preferably before traveling to ICCF11), would be to examine your Pd with CR-39 detectors. I have everything needed. Your ICCF10 paper gives no evidence that the excess heat has nuclear origin; observing tracks due to alpha particles and protons (above natural background) would provide the needed evidence. P.S. I have a Ph.D. in nuclear physics (1964) and I have worked with many different detectors of nuclear particles. But this was long ago. Experience with CR-39 is very recent.

8/27/2004 (from Dennis Letts --> L.K.)
Ludwik, We have just started a new series of tests; our first cathode in this series, #613, produced a large amount of apparent excess power during the loading phase at low current: .05 amps, 3.3 volts. Dennis and I will send you this cathode and any subsequent cathodes that appear to produce excess power for analysis.

We do require that the cathodes be returned after analysis; further, we do not plan to release any experimental details until after all of the experiments are completed, projected to be by December 31st, 2004. Of course, you are free to release/publish your findings whenever you wish. We assume that you will give us a copy of your findings when they are complete. Please send your mailing address if this arrangement is acceptable. Dennis Letts, Dennis Cravens

8/27/2004 (from L.K. -->Dennis Letts)
Hi Dennis and Dennis: I will be very happy to examine your cathode. I will place the CR-39 on top of it for about two weeks. Then I will send the sample back to you. I will release nothing without your OK. . . .

8/27/2004 (from D.L. --> L.K.)
[You wrote.....] This series of experiments is closed at the request of those with whom we are collaborating. [You wrote . . .] We are not developing anything new at the moment--only demonstrating what we have already published and the community is welcome to all of that information. [You wrote.....] Unfortunately we are in "demo" mode now and will be in this mode until December 31st, 2004. My phone number in Austin, Texas is ..... [You wrote.....] I will send cathode #613 Monday; cathode #615 will begin testing Saturday evening. If it makes excess power I will send it as soon as it comes out of the cell.

8/28/2004 (from L.K. --> D.L.)
I will be ready for your samples #613. And good luck with the sample #615. As I said, you will be informed about all that I do on your cathodes. Thanks for sending them. At what average rate was the excess heat generated in the sample #613, and for how long?

8/28/2004 (from D.L. --> L.K.)

Ludwik, I had hoped that I made our policy clear on my first e-mail: No experimental details will be released until the series of experiments is complete, projected to be December 31st, 2004. This is NOT our normal policy but our current situation requires that we work in this way. The only fact you will know before December 31st, 2004 is that the cathode made apparent excess power. If this arrangement is satisfactory for your purposes, please confirm in your next e-mail.

8/28/2004 (from L.K. --> D.L.)
Dennis: Yes, this arrangement is satisfactory. I know that you have the information and that it will become available later. I will assume that your policy has something to do with the patent aspects of the project. If I see a lot of tracks due to charged particles (well above the background) then we will address the issue of when and how to share this component of YOUR work with others. I am doing a trivial thing while you discovered something innovating and challenging. Thanks again for the offer. . . .

8/28/2004 (from D.L. --> L.K.)
The policy of "no information until we're done" has nothing to do with patent issues--all of our information has already been made public. It has ONLY to do with not wanting to release incomplete information to the community. This series of tests will be a confirmation/demonstration of the laser effect or a repudiation of the laser effect. We want to make sure ALL of the results are known before we go public with the data.

8/28/2004 (from L.K. --> D.L.)
1) That is a very good policy; Fleischmann and Pons paid a heavy price for not following it. 2) As you know, I have a web site on which I summarize what I learn and think about cold fusion. I am reading your ICCF10 paper and I want to have an item based on. The first draft is about 50% finished. The attached file (in the MS Word format) shows what I wrote so far. I will send you the finished draft later, and will ask for corrections, if necessary. I will stop writing (and will not post my essay) if you have an objection. Let me know.

8/28/2004 (from D.L. --> L.K.)
. . . Neither Cravens nor I want to have any editorial control over your work--write and publish as you wish. We only ask you to use the usual care when attributing a quote to us..... Cravens and I say very little publicly outside of our contributed papers or slide presentations. Everything we currently know about laser stimulation is on the LENR-CANR web site in the form of our papers and slide presentations..... Please continue your work as you see fit.

9/1/2004 (from L.K. --> D.L.)
Hi Dennis: When should I expect to receive the sample (or samples)? Ludwik

9/1/2004 (from D.L. --> L.K.)
. . . I will send cathode #613 Monday; cathode #615 will begin testing Saturday evening. If it makes excess power I will send it as soon as it comes out of the cell. [You asked:] **"**Did you find a lot of excess heat from the last cathode?" **Yes, it appeared to do so.**

9/2/2004 (from L.K. --> D.L.)
1) Thanks sharing good news about your work in progress. I am ready for the cathode. 2) The CR-39 detector is thick enough to stop the charged particles (such as 3 MeV protons observed by Jones or alpha particles). Therefore the side of CR-39 in contact with the cathode will be use to get the "signal + background" while the other side will be for the "background" only (from radon, etc.) I should have no trouble with the identification of the signal if one side has more than ten times more tracks than the other. We will see.

9/7/2004 (from L.K. --> D.L.)
The cathode just arrived. I at once sandwiched it between two pieces of CR-39. The third piece will later be exposed to an alpha source for 10 seconds. All three pieces will be etched at the same time. I would like to keep the exposure (of your cathode to CR-39) for several weeks; the longer the better. Is anybody waiting for this cathode? Let me know. Thanks for allowing me to participate.

9/8/2004 (from D.L. --> L.K.)
There are no other tests planned for the cathode, keep it as long as necessary.

9/16/2004 (from L.K. --> D.L.)
The ICCF11 abstracts must be submitted before 9/30/04. I would like to make a short presentation based on my analysis of tracks from two foils: your Pd and Jones' TiDx foil that I processed in June. . . . My abstract is shown below. . . .

9/20/2004 (from D.L. --> L.K.)
That's fine with me--I'll be very surprised if there is any evidence of nuclear activity but all efforts are worthy and should be pursued. Good luck!

9/20/2004 (from L.K. --> D.L.)
Your second sample arrived and I just sandwiched it between two pieces of CR-39.

10/1/2004 (from L.K. --> D.L.)
Your third cathode arrived today (10/1/04) and has just been sandwiched between two CR-39 detectors. I will etch all detectors in about two weeks.

= = = = = = = And here are messages from yesterday and today = = = = = = =

10/12/2004 (from L.K. --> D.L.)
Last evening I opened all three CR-39 sandwiches (exposed to your three cathodes) and etched them.

Cathode A (sent to me on September 7) after 820 hours of exposure (your #613 ?)
Cathode B (sent to me on September 21) after 485 hours of exposure (your #616 ?)
Cathode C (sent to me on October 1) after 266 hours of exposure (your #615 ?)

The first quick inspection showed that:

1) Practically nothing from cathode C
2) Some tracks from cathode B (perhaps 10 or 20 times the background)
3) A huge number of tracks from cathode A (perhaps 1000 times the background)

I just placed fresh CR-39 detectors on cathodes A and B to see if they are still emitting charged particles. We will know in several weeks (after my return from the ICCF11 on November 10).

Here are some preliminary observations concerning cathode A:

Both sides were emitting particles but one side has about ten times more particles per unit area than the other. By looking at the cathode I see that one side still shows the trace of the wire that was supporting it. That side shows fewer tracks that the other side. The side that has more tracks shows clustering; I can identify at least two fields of very high track density. All tracks are quite different from those due to alphas from my Am-241 source; it may be that they are not due to alpha particles.

It is your experiment, Dennis, and you should decide what to do with this information. You wrote that you plan to release all findings at the end of the year. It would be difficult for me to keep quiet at the ICCF11; my talk is based on tracks I observed from a TiDx foil that Steven Jones sent me last spring. But I will keep quiet, if you prefer. I would very much prefer to rewrite my ICCF11 paper (with you as the first author) to include new results. Even your cathode B has many more tracks than Steven's TiDx foil. My intention was to talk about difficulties (and possible bias) in counting rare tracks. But how can I talk about this now? . . .

Naturally, your input is essential. You do not have to reveal everything in the ICCF11 paper; just provide some minimum and refer to pending publications. While awaiting your reply I will select some pictures of what I saw today under the microscope. I will send them to you as an attached file. Let me know what you think. You can call me, if you prefer.

10/12/2004 (from L.K. --> D.L.)
I SUSPECT THAT THIS FILE WAS NOT SENT PROPERLY ABOUT AN HOUR AGO. IT DESCRIBES THE FILES THAT WERE ATTACHED TO MY LAST MESSAGE.

1) file 00... Magnification 40. The distance between small divisions is 0.01 mm
2) file 14... alpha particles from Am-241 (magnification 40)
3) file 15... alpha particles from Am-241 (magnification 200)
4) file 27... cluster of tracks from the cathode A (magnification 40)
5) file 29... outside the cluster (same side of the same cathode (magnification 40)
6) file 32... same tracks under magnification 200

It looks like your particles are much more penetrating than alphas of several MeV. They are able to traverse my detector. But I must take another look to be sure; the microscope is at school and I am at home now.

10/12/2004 (from D.L. --> L.K.)
Ludwik, Dennis Cravens and I are pleased to hear about your positive result and encourage you to report your result at ICCF11 in any manner you choose. We decline your kind invitation to be co-authors because this is your work and it should remain independent of us. A minor acknowledgment for Letts, Cravens and Earthtech International would be more than sufficient.

We will provide experimental details concerning the 3 cathodes you have analyzed in order for you to write a meaningful paper. Let me know what you want to know by e-mail and I will respond promptly. I received the photos and they are really interesting, even with my limited knowledge of the CR-39 technique. The cluster was especially interesting--can you tell me on what part of the cathode the cluster was located? The cluster might correspond to where the laser beam spent most of its time. I know where that was but you don't--so this would be a good test. Your paper should be very well received and we wish you good luck.

10/13/2004 (from L.K. --> D.L.)
Dear Dennis: 1) Thanks; the audience at ICCF11 will have no doubt that I am only a self-appointed technician wishing to contribute to your ongoing project. I asked Jean Paul (the conference chairman) to modify my very short abstract. Details will be needed when I start writing the paper, after the conference. During the conference I will focus on pictures and will say that information about cathode #113 will be published by you in the near future.

2) I will be very busy learning as much as I can from the CR-39 tracks. If I knew I had something like this to do I would not schedule my departure to Paris on October 20. I will let you know what I need. Would you be accessible, either by e-mail or by phone during the weekend? That could help me a lot.

3) The most obvious question has to do with differences between the three cathodes. What made your sample 613 different from samples 615 and 616? If possible, please send me a piece of Pd (from the same stock) that has not been used as a cathode. It is essential to show that Pd is not contaminated with something radioactive. The same goes for chemicals you used to make the electrolyte. I would like to cover these materials with CR-39 for several weeks and then look for the tracks. Showing that numbers of tracks due to contamination are negligible, in comparison with those from your cathode 113, would satisfy honest skeptics.

4) In the future I would like to replace the CR-39 with a Si detector of charged particles (similar to that used by Steven Jones but not as sophisticated). Unlike CR-39, it would let us distinguish protons from alpha particles, and to learn about the emission time-dependence. Are we observing something that decreases exponentially or something that has a more complicated curve (first growth then decay)?) We can address this issue later. The biggest task for you would be to produce several highly radioactive cathodes in a row. I hope that generation of excess heat in correlation with emission of nuclear particles will one day be as reproducible as as exposing CR-39 to a radioactive source and observing the tracks.

10/13/2004 (from D.L. --> L.K.)
Ludwik, On August 1st 2004, cell #613 had a boil-off during loading at .1 amps. The cell was in its 5th day of loading

and was well loaded with Deuterium. I checked the cell at 8 pm on July 31st; Scott Little checked the cell the following day, Sunday, August 1st and made the attached entry into the Earthtech online logbook. The cell was NOT instrumented during this event so we can't rule out that the power supply malfunctioned BUT the power supply is of good quality and has been under test since then and has not failed.

Your findings, of course, support the idea that perhaps the boil-off was real and not due to the power supply. It should be noted that the cell top and surroundings were NOT coated with Lithium precipitate after the boil-off, which suggests that the water vaporized very rapidly. The body of the cell got hot enough to melt an epoxy that was holding an external magnet to an aluminum support. This is shown in the first photo, magnet support lying to the left of the cell. Dennis Letts

10/13/2004 (from D.L. --> L.K.)
1) OK--I'll provide what you need. 2**)** Yes, my home and personal lab phone is.....Call anytime, day or night. **3)** Cathode #613, #615 and #616 were all made from the same stock following our 17 step protocol exactly. The electrolyte was prepared in the same way and from the same materials. The cells were loaded and run in the same way. I will send an unused cathode and Lithium from the same stock today. I don't have any unused D_2O from the runs. It should be noted that a few drops of Cravens #1 sauce was added to #613 but NOT to #615 and #616. I will send some #1 sauce for CR-39 analysis.

Results from runs: **#613** -- During loading at .10 amps, 3.3 volts cell #613 had a boil-off event. The cell was NOT instrumented at the time but was loading quietly in a controlled temperature enclosure at about 15C. Cathode was reloaded with Deuterium and tested. No excess power was observed after the boil-off during loading. The details of this boil-off have been sent in a separate e-mail. **#615** - Loaded normally without a boil-off and showed no signs of excess power. **#616** - 10 hours into loading at 3.3 volts, .05 amps, #616 had a boil-off and was partially instrumented. The boil-off caused the cell parameters to go out of range for the existing instrumentation BUT it appears that the power supply did fail the second time. However, this time there was evidence of Lithium precipitate on and around the cell. This IS consistent with a slower boil-off condition possibly caused by the power supply. #616 did produce up to about 250 mW of excess power with an isoperibolic measurement but no excess power by flow measurement.

**an explanation: We are using two calorimeters. One is called the Avanti, which is just a temperature controlled enclosure and is ONLY an isoperibolic system. The second system is called MOAC and is a high-precision dual method calorimter (flow and isoperibolic). We usually load in the Avanti and test briefly with the laser. We then put the cell in MOAC for detailed testing. Both boil-offs occurred in the Avanti system.

4) OK, we will do our best [to perform reproducible experiments] ..... Dennis

10/13/2004 (from L.K.. --> D.L.)
You wrote: "Cathode 613, 615 and 616 were all made from the same stock following our 17 step protocol exactly. The electrolyte was prepared in the same way and from the same materials. The cells were loaded and run in the same way. . . . " That is a very strong argument. Therefore, do not send anything but the #1 sauce. How can an honest scientist refuse to rule out a possibility of contamination of the cathode or the electrolyte after reading the above? One cathode produced practically no tracks. The possibility that my CR-39 pieces was contaminated can also be ruled out; all tiny pieces I used were cut from the same 1" by 1" radon detection chip. More later.

10/13/2004 (from D.L. --> L.K.)
Ludwik, I shipped the remainder of the #1 sauce in the original bottle; there are a few milliters left, hopefully enough for your analysis. It will arrive Friday by UPS. . . .

10/13/2004 (from L.K.. --> D.L.)
Was this sauce dissolved in the electrolyte or was it used as an electrolyte? If it was dissolved in water (together with other chemicals) then what was the concentration of the sauce? Does it mix well with water?

10/13/2004 (from D.L. --> L.K.)

I added about 1/4 of a small pipette to the cell on the last day of the experiment; the cell contained 100 mL of .7M LIOD. I estimate that I added at most about 1/2 mL to the cell. The elements in the sauce are written on the bottle containing the last of the sauce. The sauce was provided by Cravens; if you have detailed questions about the sauce e-mail him directly. The sauce mixed very well and very quickly--one would think that both sides of the cathode were plated about equally. A pair of external permanent magnets were around the cell during the entire experiment--I don't know if or how that would affect the plating process.

= =

There will be more messages but I must stop somewhere. I know that Cravens and Lotts were at the 10th International Conference on Cold Fusion. But I do not remember talking with them privately. Trying to find out who they are I found what they wrote about themselves. At his home page: Cravens wrote: “I am a ‘semi-retired’ college Prof (Physics, Chemistry, Biology and Math -- Yes all 4) who is now teaching a few online college classes for ENMU [Eastern New Mexico University] and SCU [Santa Clara University, California]. But my real ‘love’ is to work on things that might make the world a better place.”

I suppose that Letts, who is probably in his fifties, is also a teacher of physics and chemistry. In one of his e-mail messages he wrote: “I have performed the CF experiment thousands of times and have observed the excess power/heat effect many times; HOWEVER I am not convinced that the effects were real. I have spent 7 years improving my calorimetric skills and perhaps soon I will be able to discern fact from artifact--but until then I won't be ready to ‘move on.’ "

I also found this: “Letts and Cravens have been trying to present a credible cold fusion experiment at EarthTech for 8 years. ICCF10 demo experiment #602 finally convinced EarthTech to build a special dual-method calorimeter to test the laser effect to a higher standard than has been used previously. Letts and Cravens will work closely with EarthTech over the next several months to perform a series of experiments in a high performance calorimeter called MOAC (mother of all calorimeters). If the laser effect appears in MOAC, then cold fusion credibility will be enhanced.”

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183) Rough numbers

Ludwik Kowalski (October 14, 2004)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Here are some facts concerning observations described in the unit #182. I plan to add additional comments at the end; please revisit.

1) Using CR-39 to detect alpha particles from radon is very common. The detector is a transparent plastic plate, typically 1 mm thick. Alpha particles create latent tracks and these tracks become visible after etching the plastic for several hours in the 6.5 N solution of NaOH at 65 degrees. After that tracks can be observed through an ordinary microscope. I am using a microscope that has a digital camera able to capture pictures. The field seen with the naked eye is circular (for example, the diameter of 5 mm at the lowest magnification 40). But the field seen through the camera, on the monitor, is smaller. In my case it was 1.3 mm by 0.88 mm at magnification 40 and 0.26mm by 0.18 mm at the magnification 200.

Figure 1 Typical tracks from an 241 Am souce; magnification 200

Figure 2 Typical tracks from a Pd cathode ; magnification 200

The two figures above show tracks of alpha particles from ^{241}Am (mostly circles) and tracks from the cathode #613 (often comet-like). In both cases the magnification was 200. Only small sections of photographs are shown to keep the graphic files short. Note that to scan the entire 10 by 5 mm cathode, at that magnification, would require 1100 fields. After realizing how much work is required I selected several fields randomly and counted numbers of particles. These results were then extrapolated to obtain total numbers of particles on the six CR-39 chips exposed to both sides of three cathodes. The numbers given below are likely to be reliable within the factor of two or three.

CR-39 facing the more active side of the cathode 613 (exposure time 820 hrs):
Total area of about 50 mm^2. The highest track density, probably the cluster where the laser beam was intercepted, was close to 6000 tracks/mm^2 (or about 500,000 tracks in the entire cluster). The number of tracks outside the cluster is about 20000. The density distribution outside the cluster was not uniform on the entire surface.

Figure 3 Tracks outside (left) and inside (right) of the cluster; magnification 40

CR-39 facing the less active side of the cathode 613 (exposure time 820 hrs):
The number of tracks from that side was about 4000 (1000 in the cluster and 3000 outside). That is less than one percent of the number of tracks on the opposite side. The apparent cluster is situated at essentially the same location, with respect to the Pd surface, as the much stronger cluster on the opposite side. Is this only or coincidence? I do not know. How can a 30 mW laser beam intercepted at one surface be responsible for the number of tracks on the opposite surface? The thickness of the cathode was close to one millimeter. It clear that the number of tracks on this surface is about 100 times fewer than on the CR-39 exposed to the more active side of the cathode.

CR-39 facing the more active side of the cathode 616 (exposure time 485 hrs):
The area of this piece of Pd was also about 50 mm^2 but the shape of the cathode was nearly rectangular. The total number of tracks on the surface was about 8000. Their distribution was not uniform; not even approximately.

CR-39 facing the less active side of the cathode 616 (exposure time 485 hrs):
The total number of tracks on that surface was about 3000. Their distribution was also not not uniform, not even approximately.

CR-39 pieces facing surfaces of the cathode 615 (exposure time 266 hrs):
The Pd area was close to 50 mm^2; there were less than 100 tracks on each CR-39 surface. The real background (cosmic rays, radon, thoron, etc.), on a piece of CR-39 that was not exposed to the cathode, would be less than 50, for the entire area. It is reasonably to think that all tracks (on surfaces exposed to palladium) were due to the background.

I was wrong in hinting (in unit 182) that some of the particles were able to pass the CR-39 detectors (about 1 mm thick). My CR-39 are dirty and, in a quick look, I probably took some dirty spots for tracks. But Pd particle tracks are definitely different from those due to alphas of ^{241}Am. My guess is that nearly all of them are "born" on the surface, they do not come from the depth of Pd. How else can one explain a relatively large number of tracks looking like comets (not like circles). My etching time of 6 hrs (perfect for alpha particles from my ^{241}Am source) was too long for shallow tracks. Many tracks might have been dissolved by the hot NaOH. If I had to do this over again I would etch for only 2 hrs, examine the tracks, etch for another 2 hrs, examine the tracks, etc. Very tedious, indeed. It would be much easier to use Si detectors, instead of CR-39, as Steven Jones does.

Comments and additions, made after October 14, will be appended below.

10/15/04:
Dividing 500,000 tracks by 820 hours one gets about 600 tracks per hour. This is much higher than several tracks per hour observed from the TiDx foils by Jones. But even this would be too little to generate measurable amount of excess heat. On the other hand, the unusual excess heat event occured on July 31. This was several weeks before I received the cathode #613. Perhaps the ~500,000 tracks from that cathodes is nothing in comparison with what would be recorded if the CR-39 detectors were applied much earlier. My assumption that the nuclear activity was constant during the entire 820 hours can also be questioned. And I have no idea how many shallow tracks might have been desoved in the hot NaOH.

10/15/04 By Steven Jones:
Dear Ludwik and Dennis and Dennis:

The CR-39 results achieved and posted by Ludwik, using the Dennis**2 Pd foils, are very intriguing indeed. Congratulation to all!! Ludwik, I suggest you post the thickness of the Pd foils on the latest posting, as this would affect the ability to see tracks on both CR-39 plates. Also, I certainly agree with Ludwik that follow-up tests using silicon (ion-implanted) detectors would be extremely useful. With the Si detectors available now at BYU, we can determine both the energy and identity of the particles. Seems this should be done right away. And I agree with Ludwik also that the Pd plates should be looked at as soon after excess heat observation as possible. With a Pd foil on the "outside" of a cell, that is, as a wall, we can even put an energy-dispersive Si detector right next to the Pd (in air or helium to allow alphas to get to the detector more readily) -- and thus hope to catch emanated particles DURING excess heat production! This would really clinch it!

Anyway, I share Ludwik's enthusiasm and hearty congratulations to Dennis and Dennis and Scott. (PS -- Is the MOAC giving results yet? I'm very much looking forward to these... I may be convinced yet of correlated heat and particle production...)
--Steven Jones

10/15/04:
The thickness of Pd was 0.42mm. Let me remind you that MOAC, in Steven's message, stands for Mother Of All

Calorimeters. I am sure it will be fully described for us by two Dennises at the end of this year. A very short description was already made in unit 182.

In my first comment above I referred to 600 tracks per hour. Note, however, that for the 20,000 tracks outside the big cluster (cathode #613) one gets 24 counts per hour. This is much closer to what was occasionally observed by Steven, if I remember correctly. Perhaps there are two kinds of cold fusion tracks, those due to the 30 mW laser beam and those that would be present even without the 30 laser beam.

10/15/04 By D2 (Dennis Cravens, not Dennis Letts, D1, with whom I corresponded.):

I would be very much in favor of having Steve get an energy distribution on the electrodes. And then at the end, perhaps an XRF from Scott and perhaps an elemental analysis from Ed.

I am uncertain right now if the sauce sent to Letts several years ago had U in it. I think it is the same one that Scott originally did the XRF on (see below) last year. I would be very cautious on making too much of the counts until we are sure about any U contribution. (my guess is U alphas will be easy to ID). However, as said before, the interesting thing to me is the variation from side to side and area to area on the electrode since any plating should be nearly uniform across the plate and from front to back of the electrode.

If it is the sauce I think it is, it has Ce, Er, Rh, U, Li, Pd and La and a little Hg - The idea at the time was to incorporate high nuclear spins and high quatrapoles for spin exchange to the lattice and then seal with Hg or Au and to have point defects in the lattice for phonon exchange. However, I think it is mostly Pd and Li with some sulfamic acid to keep ions in solution.

[The XRF stands for the X-rays fluorescence. One bombards a sample with photons of 60 keV and observes characteristic photons of lower energies (soft x-rays) due to deexitations in different elements. As one can see below, the spectrum has two tiny picks identified as uranium. In the next comment D1 reminds us that the "sauce" was added to the electrolyte for the cathode #613 only. Is it possible that uranium was subsequently deposited on Pd surfaces? Yes it is. But why would most of the uranium be deposited on a small part of one surface? I do not know.]

Figure 4; the XRF spectrum sent by D2

10/15/04 By D1 (Dennis Letts):

Steve, Thanks for your interest and enthusiasm. As I told Ludwik, #613 was spiked with a small amount of Cravens sauce #1 during electrolysis and that may be the source of the large number of counts. I sent the remainder of the sauce to Ludwik and hopefully he will be able to make an estimate of how much the sauce influenced the high counts on #613. The other two cathodes did not receive any Cravens sauce, so the elevated counts on cathode 616 may be valid.

So far I have failed miserably in MOAC: Scott and his colleague, George Luce, have designed and built a splendid machine and their cooperation has been "over the top." Our plan was to see the laser effect isoperibolically in MOAC and then observe the mass flow measurement a few minutes later, since MOAC was designed to be a dual method calorimeter. If the flow measurement showed a similar response to the isoperibolic measurement, then we would conclude that the signal was real. Failing to see a flow confirmation, we would conclude that the signal was false.

I never contemplated that we would see NO signal. A possible reason is this: I ran out of my old stock of Palladium before beginning the MOAC tests. Cravens and I thought that our methods would work for any metal source but that may not be true. We are presently testing other Pd, Lithium, Gold, D2O sources in an attempt to re-establish the effect so that MOAC can judge our results. As McKubre has said "there's a demon in these experiments". Any analytical help from you, for my part, would be welcome...hope you and your work are well,
Letts, Cravens and the Earthtech group.

10/15/04
I just received the "sauce #1" in a little bottle; about 1 mL. In unit 182 Letts wrote: "I added about 1/4 of a small pipette to the cell on the last day of the experiment; the cell contained 100 mL of .7M LIOD. I estimate that I added at most about 1/2 mL to the cell. ....." Thus the worse possible scenario is that all the uranium from the 0.5 mL of the sauce was deposited on one Pd surface. To simulate this (at the reduced scale) I deposited 0.1 mL of the sauce on a sheet of plastic and dried it on my desk below the 60 W light bulb. The sauce in the bottle did not look like a solution, it looked as a colloidal suspension. The dry layer is probably as thick as the range of alpha particles of uranium. I will measure the thickness in mg/cm^2 eventually. A piece of CR-39 was placed over the dried spot (same shape and same area as the Pd 613 cathode). I will see how many tracks will be created in CR-39 after several weeks of exposure.

10/15/04
David Dow (from..... ?), who read this unit, used my Figure 3 (see above) to produce a density distribution chart shown below. His specialty is GIS (Geographic Information Systems) and he has software for creating density maps. I think that Richard Oriani also used this kin of mapping to analyze cascades of tracks on his CR-39 detectors. His paper on massive cascades of tracks (in the air above the electrolyte) was presented at the ICCF10 last year. It can be downloaded from library at

http;//www.lenr.canr.org

Oriani, as far as I know is the only researcher who also saw cascades of incredibly large numbers of tracks. I am going to work with him after the ICCF11. The goal will be to develop an experiment that can be performed by high school students.

Figure 5 Density distribution map produced by David Dow.

10/16/04
I just realized that I am actually conducting two tests on the uranium hypothesis. I will call them "thick source test'" and "thin source test." The first one was described in the piece above; it involves the "sauce #1." The second test started when the Pd-613 was sandwiched again. Suppose a thin layer of uranium, deposited on the cathode is responsible for the observed 500,000 tracks. In than case I should observe the same number of tracks again. The half-life of uranium isotopes is very very very long. The alpha radioactivity may actually increase due to an accumulation of daughters.

If the number of tracks observed from the thin source test is negligible, in comparison with ~600 per hour, then the idea of uranium contamination could be put aside. But suppose it is not negligible. This should not be taken as a conclusive validation of the hypothesis. But would be a very strong indication that the hypothesis is valid). If this turns out to be the outcome of the test then I would ask for your permission to send the cathode to Steven Jones. The energy spectrum of alpha particles from uranium, and its daughters, are well known. If the observed spectrum matches known peaks of uranium than we would say that uranium was indeed responsible for our excitement. I am assuming that Steven would be happy to conduct the decisive test with a Si detector.

Modified on 10/17/04
Let me add one more observation. Some say that cold fusion people are con artists. Are we con artists? A con artist would hide everything that might cause doubt about his claim, he would focus on convincing arguments only. But the idea of a possible uranium contamination came from Dennis Cravens. He tells us not to accept the results from Pd-613 before further testing. I agree with this. His observation about Pd-616 (11,000 tracks) is also valid. Dennis's experiment was designed to confirm the reality of cold fusion; how can he remain objective in the face of an apparent confirmation? But he is saying to us "hold your horses". That this fact is very significant to me, in the context of the cold fusion controversy. Dear Dr. Park, are you reading this unit? I would be happy to append your comment below. Do you think that what dennisses do is voodoo sciece? Will I see you at the ICCF11 event or are you going to boycot it?
End of modification.

I was tempted to ask D1 and D2 about their motivation for adding the "sauce 1" into the electrolyte. I did not ask

because they decided to wait till December 31, the date of the announcement. The answer came, quite unexpectedly, from Steven Krivin who interviewed D2 before writing a new book on cold fusion (see item 181). During the interview Steven asked: "Can you describe your work in layman's terms?" The answer of Dennis Cravens was: "Right now I'm doing work with Dennis Letts, who's doing laser induced reactions, trying to stimulate the surface and get surface reactions going. Also, chemical excitation of a palladium cathode. And putting in special chemicals within the mix and then electrolyzing and then kicking off the reaction through a new surface applied to the palladium." That is good enough for me, at least for the time being. To see the entire interview go to:

http://www.newenergytimes.com/Conversations/cravens.htm

10/18/04 from Steven Jones to D1 and D2.
Thanks for the additional – and very important—information. As D2 noted, “I am uncertain right now if the sauce sent to Letts several years ago had U in it. I think it is the same one that Scott originally did the XRF on (see below) last year. I would be very cautious on making too much of the counts until we are sure about any U contribution. (my guess is U alphas will be easy to ID). “ This is just the sort of thing we need to do – check and re-check ourselves!

Note that an advantage of the dual-coincidence spectrometer here (which includes a cosmic-ray veto) is that we are not sensitive to alphas from uranium – or radon, etc., since a coincidence is required, such as proton + triton from d-d fusion. (Another example is alpha + alpha from d-Li6 fusion.) We would miss events in which single nuclear particles are emitted. And we did see a signal from the foil D2 prepared last year when placed in our dual-coincidence spectrometer. I wish you the best of progress and success with the MOAC. To me, an instrument such as this has great promise in unraveling the mystery of “cold fusion.” 10/19/04 My message emailed to D1 and D2: A nuclear physicist, and a friend, who is very skeptical about cold fusion, asked: "How pure is your palladium? Are you sure that palladium does not contain naturally a minute intrinsic admixture of thorium or uranium?" Well, one can find atoms of any anything in a manufactured object. The issue is how many. This question of impurities is likely to be asked by others. The XRF spectrum sent to me by D2 does not have any numbers (ppm?) along the vertical axis. Do you have manufacturer's specification of impurities? If so then I would like to have it in Marseilles. Let me summarize this issue of contamination.

1) According to D2, who was the first to say "hold your horses," the sauce #1 might have contained some uranium. To back this hypothesis he showed the XRF spectrum with two tiny peaks attributed to uranium. He is not certain that the sauce (~0.5 mL) actually added to the electrolyte (~250 mL) was the same as that used in the XRF analysis. Therefore, a possibility that the sauce actually used was even more contaminated should not be excluded.

2) Two tests for contamination with an alpha-radioactive source are in progress. Waiting for the result one must rely on the following reasoning (speculations).

a) One line of reasoning goes like this: "The Pd-615 cathode was made from the same stock as the Pd-613. Both cathodes were processed in exactly the same way. The Pd-615 cathode did not produce tracks. Therefore, palladium itself, or the electrolyte, were not contaminated."

b) Another line of speculation goes like this: "Contamination from the electrolyte would lead to essentially the same number of tracks on both surfaces of Pd-613 due to the geometry of electrodes (Pd cathode surrounded by the spiral Pt anode). But the numbers of tracks produced were found to be very different on two surfaces."

c) Contamination of the original Pd, or contamination of the electrolyte, could not produce a highly localized cluster discovered on the Pd-613 cathode. Why would the size of the cluster, and its location on the cathode, coincide with the location and the size of the laser beam?

d) An apparent correlation between the number of tracks observed with three cathodes and the excess heat they generated is based on three cases only. It can be a matter of coincidence. I am referring to is this: Pd-613 --> a lot of tracks and a lot of excess heat, Pd-616 --> much less tracks and much less excess heat, Pd-615 --> no tracks (above the background) and no excess heat.

The weakness of the first three arguments is that the contamination might have been introduced when the Pd-613

cathode was handled. It is not totally impossible that such contamination occurred on one of the Pd-613 surfaces, and exactly at the same place where the laser beam was later intercepted. That is why the outcome of the ongoing "thin source" test will be so important. I just calculated the amount of U-238 needed to emit 500,000 alpha particles in 820 hours. That amount would have to be only 0.14 micrograms. Assuming that the CR-39 detects one half of alphas (the other half is emitted into the opposite direction) the amount would be due to 0.28 micrograms of uranium. On the other hand, uranium is likely to be mixed with its alpha-radioactive daughters. In that case even 0.05 micrograms could be sufficient to produce the observed tracks. The mass of Pu or Am, needed to produce 500,000 alphas in 820 hours, would be many orders of magnitudes smaller than what was calculated for the U-238.

10/19/04 A reply to the above from Dennis Cravens:
The Pd used, that you have is four 9's .....99.99%. Many other researches in CF have very well characterized Pd. The most sensitive way to check for U or Th is via neutron activation of a sample. We don't do that but others have. U and Th is not at all likely to be in quality Pd. I prepared four sauces for Letts. Only one had U and it is the one that you have the XRF of. I just do not know which one is the one he used in that experiment. In the case of treatment of Jones' Ti (though I am not sure you have that one) no U was used only La. The bottom line is that the sample you have is either the same as the one that the XRF was done on or it has no U at all. It cannot be the case that it has more U than the XRF sample.

10/19/04 Additional details from Dennis Letts:
This metal was provided by Scott at Earthtech International. The metal may be nearly a decade old. The metal is: "Premion" Palladium foil, 1mm thick, 99.9975% purity (metals basis), stock # 12056, lot# w25022. Seller: Alfa Aesar, 30 Bond St, Ward Hill Ma 01835 800-343-0660. I called Alfa Aesar a few minutes ago and they will look for a spec sheet on the metal but they said the lot number was old and they probably would NOT have the data. I do have plenty of the metal left for analysis, so if purity becomes an issue then we could have it analyzed by a recognized lab.

10/19/04
I think that the most sensitive method of testing for an alpha emitter of a surface is detection of alpha particles. A negative outcome from our "thin source" test, already described above, would be a definitive proof of "no contamination." Revisit this page on November 14 to see how many tracks were found on the CR-39 exposed to the suspected Pd-613 surface for one month. It should be close to 500,000, if uranium is responsible. I would be nearly certain of contamination in such case; to be 100% certain one must show that the energy spectrum of particles matches the known spectrum of uranium. We will see.

10/20/04
One possibility of contamination has been ignored so far. How can you be sure, a person with a healthy dose of skepticism might ask, that the Cr-39 used was not contaminated? My answer would be "all seven pieces of CR-39 were cut from one larger piece. Why would only one of them be contaminated?" But once again, this argument belongs to logic; my answer should be based on a test. Perhaps the contamination took place after the original piece was cut. Here is what I am going to do, right now. I will expose the CR-39 surface, that produced tracks shown in Figure 2, to a fresh CR-39 detector. If the surface is contaminated then the fresh detector will show tracks resulting from contamination. The result will also be shown here on November 14; please revisit. I know that I will embarrassed if contamination is found. It would be a lesson; one should not claim anything important without conducting all the necessary tests. But not announcing a tentative conclusion would deprive me of readers' constructive comments.

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184) Three weeks later

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As reported in the unit #183, three cathodes (Pd613, Pd616, and Pd615), obtained from the Texas group of D. Letts, were examined with CR-39 detectors. The purpose was to look for possible presence of charged particles (such as those observed by S. Jones et al.). The results were exciting:

1) A very large number of tracks were observed in the CR-39 detectors exposed the Pd613 (much more on one side than on another).

2) A large number of tracks were observed in the CR-39 detectors exposed the Pd616; again more on one side than on another).

3) Only background tracks were observed in the CR-39 detectors exposed the Pd615.

After reporting these results (in the unit #183) I was informed that the Pd613 generated a lot of excess heat, that the Pd616 also generated excess heat (but considerably less), and that the Pd615 failed to generate excess heat. Could this be a coincidence? Yes it could. The issue of a possible contamination with a natural alpha emitter was raised. The main suspect became the so-called “sauce #1” added to the electrolyte in which the Pd613 was used. Small amount of uranium were known to be present in that sauce. Note that the sauce was added only to the cell with the Pd613, it was not added to cells with the Pd616 and Pd615 cathodes.

I performed these quick checks for contamination:

Check #1: Several drops of sauce #1 were placed on a piece a plastic and allowed to dry. A CR-39 detector was then applied to the dry spot for nearly 22 days. The surface was covered with tracks that looked like those due to alpha particles.

Check #2: The Pd613 cathode was sandwiched between the fresh Cr-39 detectors for the second time (also for nearly 22 days). Absence of tracks in this test would indicate that the previously reported tracks could not be attributed to contamination of the cathode. A contaminated cathode, on the other hand, would produce as many of tracks as before. Without counting I am inclined to conclude that most of the tracks due to the Pd613 cathode were caused by contamination. But this is not the only possible conclusion. I will write about this later. The questions to be answered have to do with the “one side of the cathode being much more active than another” and with a strongly pronounced clustering of tracks. The nearly-axial geometry of the cell (a small cathode surrounded by a spiral anode) should not produce the sidewise asymmetry if contamination was coming from the uranium in the electrolyte.

Check #3: The two CR-39 surfaces, on which tracks from the Pd613 were discovered, were checked for their possible contamination. The motivation for this test (also lasting 22 days) was to rule out a possible of a coincidental contamination of the CR-39 detector itself with a naturally occurring emitter of alpha particles. The results are consistent with the idea that a substance on the surface of Pd-613 was transmitted to the detector that was in contact with the cathode (for 820 hours during the experiment described in the unit #183). The tracks on the CR-39 facing the more active side of the Pd613 are much more numerous than on the CR-39 facing the less active side of the Pd613 cathode. This is consistent with the idea of “transferring by contact.”

Check #4: The more active side of the Pd616 cathode was exposed to the CR-39 detector for nearly 22 days. The detector revealed numerous tracks, similar to those of alpha particles. These tracks can not be attributed to the sauce #1 (that sauce was used with the Pd613 only; it was not added to the electrolyte surrounding the Pd616 and Pd615 cathodes). Ignoring the case in which the sauce #1 were added to the electrolyte one is tempted to think that a process responsible for charged particles, when excess heat was generated, is still going on. On the other hand tracks due to particles emitted by the Pd616 might be due to an unknown source of contamination. That source was present when the Pd616 cathode was able to generate excess heat and it was absent when no excess heat was generated (the cathode Pd615). As far as I know, the electrolyte used in all three cells was taken from the same bottle. Likewise, palladium cathodes were cut from the same larger sheet of pure metal.

The situation is far from being clear. I would prefer to say nothing at this time. But at the conference in Marseilles (last week) I was describing the ongoing tests and promised to post my first observations. Please consider them to be very preliminary. The most reasonable thing to do next would be to use a silicon detector (and a telescope of two detectors) to measure energies and to identify particles responsible for the tracks.

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185) About CR-39 Detectors

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Several people asked me about using CR-39 detectors recently. Here are suppliers of that material that I know about. If you know another supplier then please send me the information about the firm and I will append it to the list below. My e-mail address is; <kowalskil@mail.montclair.edu>.

1) Landauer, Inc. (2 Science Road, Glenwood, IL 60425, USA)
Contact Rose Elza at radon Customer Service
Her phone # is 1-800-528-8327. Her e-mail address is: relza@landauerinc.com
One sheet (for uncut 350 pieces) costs $340. A set of 350 cut pieces (each piece has an engraved ID number) costs $560.

2) Alpha Trak Learning (141 Northridge Drive, Centralia, WA, 98531, USA)
Contact Criag Gabler <radon@alphatrak.com>. Minimum order, $100, covers 100 pieces; each piece is 1 cm by 1 cm.

3) Track Analysis System, Ltd. (H H Wills Physics Lab, Tyndal Avenue, Bristol, BS8 1TL, United Kingdom.)
Contact Peter Fews <Peter.Fews@bristol.ac.uk>
Minimum order for the USA is $500 (it is $100 in Europe). Various sizes (up to 27.5 cm by 28.0 cm) are available. For example, each piece of 2.5 cm by 2.5 cm (1 mm thick) costs $1.75.

4) Intercast Europe S.p.A. (P. Neri, Via Natta 10 / a 43100 Parma Italy
Tel +39 - 0521 - 607555 Fax +39 - 0521 - 607924)
p.neri@intercast.it www.intercast.it

The prices are, for example, 200 euros for the 40 by 70 cm sheet (thickness 0.7 mm) or 300 euros for the 96 by 96 cm sheet (thickness 1.4 mm).

5) Daedalon Corporation (Walter Brown, Daedalon Corporation, P.O. 2028, Salem, Ma, 01970-6228. Phone (978) 744-5310; FAX (978) 745-3065
daedalon@cove.com www.daedalon.com
Five round alpha particles films EN-21 (diameter 8.6 cm) costs $215.

They do not say it is CR-39. They wrote to me: “The film consists of a thin layer of cellulose nitrate coated on a poly ester base. If the film is struck be an alpha particle, it is structurally damaged so that the damaged spot can be etched away in a sodium hydroxide solution. The etched film has clear spots, where it has been struck, with a reddish background. The film has no sensitivity to other radiation.” [They probably mean beta and gamma rays. How can the material not be sensitive to particles produced by neutron radiation?]

6) American Acrylics and Plastics, 300 Benton Rd. PO Box 1045, Stanford Ct, 06497.
Tel: 203-377-0752
Dosimetry CR-39 plastic, $100 for one large sheet (looks like 1 by 1 meter).

7) Fukovi Chemical Industry, Japan.

This CR-39 material was calibrated by A. Lipson et al. (downloadable from the library at www.lenr-canr.org). The calibration consisted of measuring diameters of tracks for monoenergetic alpha particles and protons of various energies. It is valid for one set of specified etching conditions. Tracks due to protons are usually about two times smaller than for alpha particles of the same energy. I will assume that their calibration is applicable to CR-39 from other manufacurers, as the first approximation.

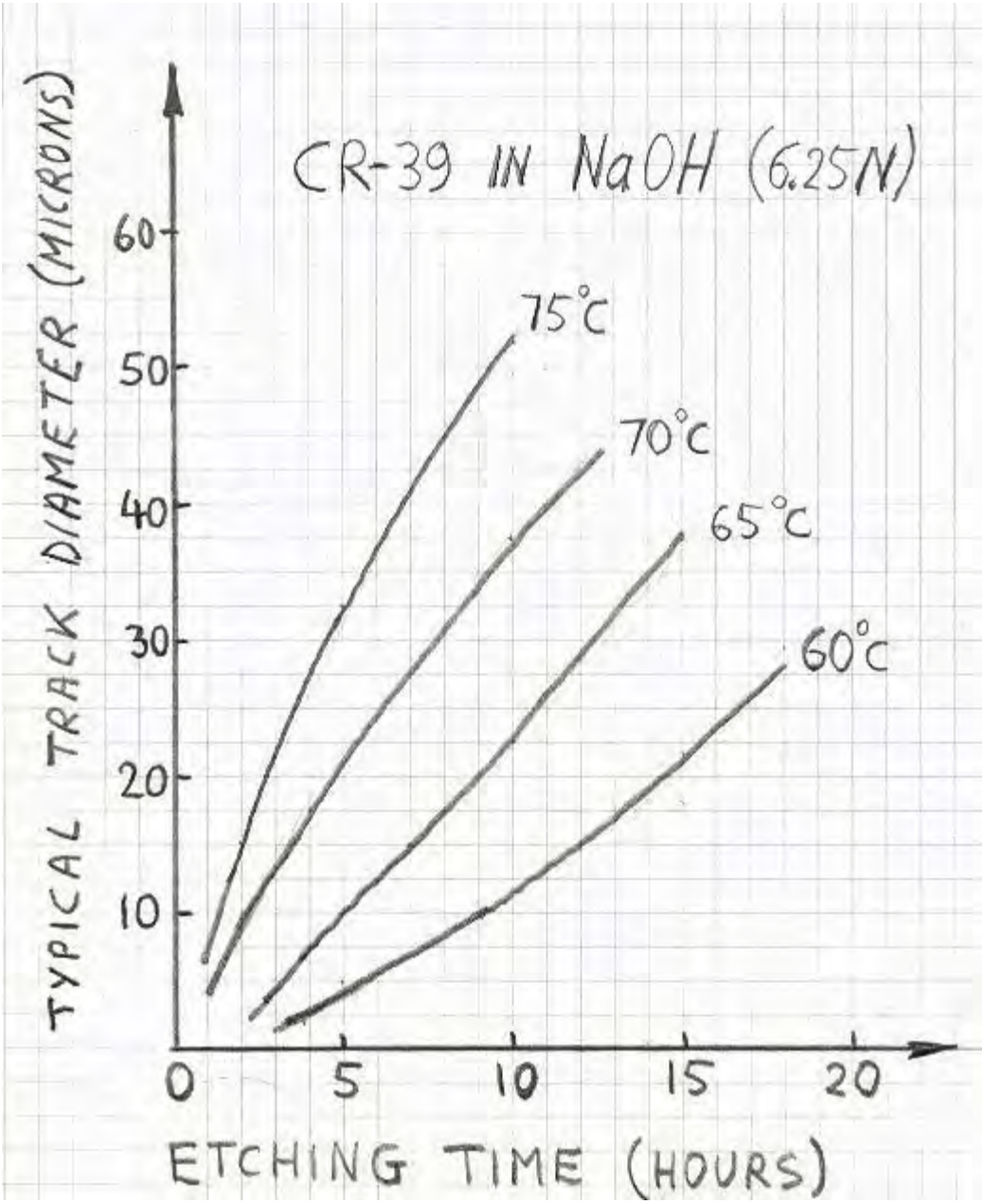
The CR-39 polycarbonate was patented (in 1939?) by an US company Columbia Research. This probably explains the name. Plastic lenses are now made from that material. Another use is detection of alpha particles, for example, from radon. Each particles creates a sub microscopic crater which can be enlarged by etching the plastic in the hot NaOH. After that the craters become observable through an ordinary microscope. Here is a brief description of the use of CR-39, as e-mailed to a friend about one month ago.

" One simply peels off the protective thin plastic from the CR-39 and places the detector (about 1 mm thick) over an object that emits charged particles, such as alphas or protons. The material is not sensitive to electrons. The etching solution is NaOH in water (concentration 6.25 N). Make sure your skin does not come in contact with that solution; if it does then wash your hands at once for at least a minute or two. Preheat the NaOH solution to 65 C and suspend a detector in it for about 5 hrs. Then remove the CR-39 and wash it in water. The NaOH solution etches the surface slowly. At the same time it etches more rapidly along the invisible (latent) tracks of charged particles. That is how tracks become visible. If you etch too long, say 20 hrs, you might take away the layer that contains tracks. If you etch not long enough, say 1 hr, then tracks will be very small. The diameters of circles, due to alpha particles from Am-241, grow about linearly with the etching time (after the fist hour). I settled for six hours. My Am-241 source is from an old smock detector, the radioactive spot is probably covered with something protective. That is why, I suppose, most tracks are circles. Radio Shack sells the radiation-type smoke detectors for about $10."

A very detailed description of various aspects of track detection can be found in a recent review article of D. Nikezic and K.N. Yub. That extensive review was published in "Materials Science and Engineering," R 46 (2004) 51–123. The heading states that the paper is available over the Internet at: <http://www.sciencedirect.com>. In my letter to the friend I continued: "At one time I exposed the CR-39 to a Pu-Be source of neutrons. This produced tracks whose diameters were similar to those from Am-241. They were probably due to (n,alpha) reactions in CR-39. But most tracks had smaller diameters; I think they were due to recoiling protons. A.G. Lipson and A.S. Roussetsky published a calibration curve for the CR-39 from Fukuvi. It shows how diameters depend on energies -- for alpha particles and for protons -- under a specified etching protocol. Their paper can be downloaded from the library at <http://www.lenr-canr.org>

You will need a good optical microscope, preferably with a digital camera attached. Our biology department has such microscope. If you have a choice use a microscope equipped with an objective of low magnification. My lowest magnification is 4 (times 10 from the eyepiece); I would prefer it to be 2. Magnification 20 is fine to examine individual tracks, magnification 2 would facilitate counting of tracks which are rare (because each view is very large). To count rare tracks I would etch for 9 hours. I prefer to count numerous tracks from printed photos (and not from the computer monitor). Using a red pencil I flag the already counted tracks to eliminate double counting. That is about my experience. Google will provide you with more information.

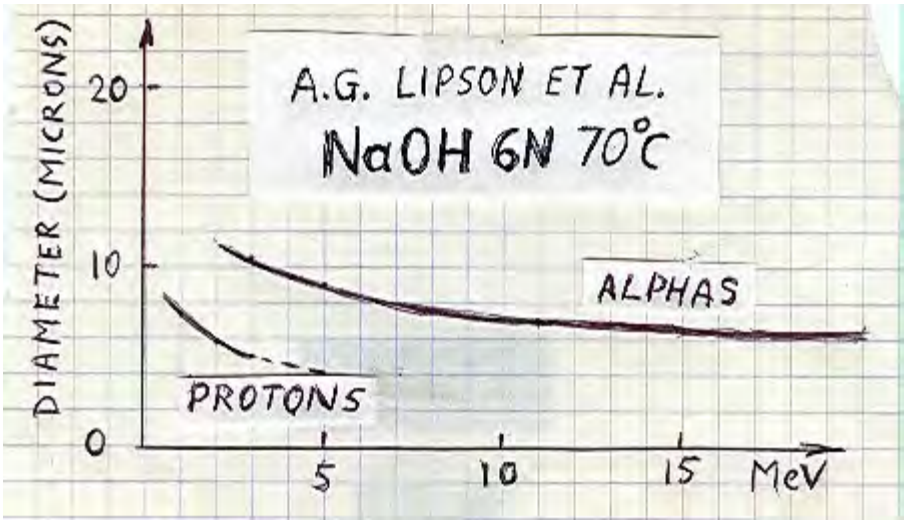
P.S. 12/22/04
After purchassin a large sheet of CR-39 from Fukuvi I conducted a little investigation. Using the 6.25 N solution of NaOH I decided to learn how the diameters of tracks depend on the etching time at four temperature, as illustrated below:



Results of rough measurements

Diameters were measured by using the stage micrometer under the microscopic magnifications of 200 and 400. Typical tracks were selected by inspection. The numbers shown are read from smooth curves.

c) Large diameters (easy to identify) and shorter etching times are desirable but the most important thing is to have as small background as possible. Suspecting that excessive temperatures might produce some track-looking background I decided not to push temperatures. My rule will be 10 hours of etching at 60 C. I looked, without being successful, for the evidence that etching at 70 C increases the number of background tracks. Perhaps shorter etching times at 70 C compensate for the effect of temperature. Most background tracks are real; they are probably due to cosmic protons and neutrons. Radon in air was not allowed to procuce tracks till the sealed envelope was open by me, and till plastic covers were removed. In other words, exposure to alpha particles can be limited to several days, the duration of an experiment. To minimize background due to radon Oriani keeps his open CR-39 chips between strips of masking tape, even when exposure times are limited to hours.



Calibration curves: (CR-39 from Fukuvi) for alpha particles and protons

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186) Transmutations of radioactive isotopes?

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At the last international cold fusion conference in Marseilles (ICCF11, November 2004) I was exposed to new claims that radioactivity can be destroyed very rapidly by microbial and pyrolytic processing. Microbial processing was described by Ukrainian scientists (1) while pyrolytic processing was announced by a businesswoman from Canada, E. Anderson (2).

According to (1), the half-life of ^{137}Cs, known to be 30 years, was reduced to less than one year. This has been accomplished by metabolically active microorganisms. "The process involved transmutation of long-lived active nuclei to non-radioactive isotopes during growth and metabolism of special microbiological MCT." The initial activity of 137Cs was reported as 18,000 bq (0.49 microcuries) and the process was conducted at room temperatures. Measurements of radioactivity, in closed flasks, were made with Ge detectors every five days. Half-lives of ^{154}Eu, ^{155}Eu and ^{241}Am were also affected by microbial action.

Unfortunately, most conference participants were not biologists and the report did not receive the scrutiny it deserves. In my opinion, it should be presented at a conference for microbiologists. It should also be submitted to government agencies responsible for dealing with radioactive waste. They should be interested because ^{137}Cs is a major contributor to radioactivity of spent fuel after it is removed from a nuclear reactor. Ability to transform large amounts of ^{137}Cs into non-radioactive substances would be highly beneficial. My personal reaction to the report was to imagine a metabolic process through which bacteria turn cesium into a gaseous compound escaping from the flask. But I am neither a biologist nor a chemist; the authors of the report probably investigated that possibility and ruled it out on the basis of experimental data.

According to (2), a Canadian company is about ready to start destroying radioactive waste on an industrial scale. She said that the apparatus built for the company is able to destroy 50% of radium, whose half-life is known to be 1620 years, in only 3 days. It also changes thorium into lead. What would Marie Curie think about this claim? She and her husband tried many ways to change the half-life of radium but were not successful. This was about one hundred years ago. According to Coleman (2), the senior science consultant to Monti America Corporation, "With the gradual disappearance of the Ether (Relativity) the Physical Space in which to place the Atom and rebuild its structure also disappears." This strange statement [is there a typing error somewhere in this sentence?] is backed by references authored by R. Monti and G. Monti. Outlining the short history of the transmutation of the waste project Coleman continues: "In 1992 R.A. Monti was given the opportunity to pursue Low Energy Nuclear Reactions at Texas A&M University [with Lin and Bockris]. The goal of the sponsors of this research . . . was to produce noble metals on an industrial scale. This goal was not achieved. The cost of gold and silver produced by transmutation of Lead and Mercury exceeded the ordinary market price. When the Philadelphia Project failed R.A. Monti decided to take another direction in his research. The possibility to cause nuclear fission of stable isotopes by ordinary chemical reactions had suggested immediately that it should also be possible to do this with radioactive materials.

A series of experimental tests made from 1993 to the present have shown that this is indeed the case. Unstable isotopes can be transmuted into stable isotopes. In 1993 R.A. Monti informed the Italian National Research Council about the possibility to get rid of nuclear waste but they rejected the idea. From 1994 to the present, ongoing research has been funded by a group of Canadian companies which ultimately led to the formation of Monti America Corporation in 1998. A proprietary formula for transmuting Lead and Mercury into silver was used as a driver for the transmutation of

radioactive elements. R.A. Monti observed that Thorium . . . can be transmuted into stable elements with 80% success rate, based on total input weight in the right season of the year. [Does the "right season of the year" phrase refer to temperature, humidity, winds, etc. or to factors linked with astronomy?]

During 1995 and 1996 over 50 experiments were conducted . . . The years from 1997 to 2004 have been used to conduct validation tests at independent research facilities and to demonstrate the process internationally. Monti America Corporation is currently working on constructing a pilot plant to prove the commercial viability of the process.

The experimental results of the tests made from 1992 to 1996 were so astonishing that they were by and large rejected out of hand by the scientific community. As a matter of fact the papers . . . do not appear in the Proceedings of these [Cold Fusion] Conferences (rejected by Ikegami, Passell, McKubre, Fleischmann) . . . In 2000 (ICCF8, Lerici, Italy) the paper . . . was rejected by Scaramuzzi (13). Finally, the paper was published in 2004 by Hal Fox (6)." My attempt to obtain a general explanation of the proprietary process from Monti, who was present at the conference, produced nothing more than a statement that the process is "pyrolytic." I remember that this term was used by Bockris, a recognized authority in electrochemistry who was the first to report cold fusion transformations.

By the way, I was not aware that Monti's claims were rejected by leading cold fusion researchers. This fact , however, did not prevent a company from investing heavily in Monti's technology. I talked with Mrs. Anderson and with the president of Monti America Corporation. They are not scientists. But they are totally convinced that their large investment will soon become profitable. To begin with, the company plans to concentrate on low level radioactive waste from petroleum products, and from other industrial operations. They plan to move to highly radioactive waste later. I said that I would like to come and observe the pyrolytic device in operation. They said that they would invite me, probably in several weeks. I hope this will happen; I would be happy to write a unit on the first industrial application of what is, according to Monti, a cold fusion transmutation process. Will they allow me to make some measurements? This remains to be seen. Will they be able to destroy the waste ready to be stored under Yucca mountain? Probably not.

I am convinced that destroying highly radioactive waste by neutrons (from a very strong spallation source) is scientifically possible and practically desirable. This, however, has nothing to do with approaches based on microbial activities or on a pyrolytic process. My advice to potential investors is to wait until such processes are confirmed by recognized experts. Biological experiments with radioactive substances are very recent and scientists performing them are not promising anything useful, as far as I know. The technology developed by Monti, on the other hand, is a promise of something useful. But that promise, as indicated above, is not taken seriously by recognized experts, even within the cold fusion community. That situation would certainly change if Monti America Corporation suceeded in destroing (rather than dispersing) even on radioactive isotope.

The presentation of the paper (2) ended with the following statement. "We have been asked not to release the full details as this involves a private research project on the verge of becoming a commercial interprise. We have been, however, authorized to attach a letter of opinion from a Canadian learned society which gives a general summary of the demonstration." Before showing that letter (see below) let me mention a videotape of the demonstration; it was shown at the conference. I was not convinced by its content. Three other existing videotapes were mentioned in a handout:

1) "Gold from Mercury according to Gabber," by R.A. Monti and G.A. Cesarano Monti.
2) "Gold (light and energy) from Naples," by D. Cirillo and V. Iorio.
3) "Transmutation of nuclear waste in Stockholm" by Monti America Corporation

I hope the contents of these tapes will have more scientific information than the videtape shown at the conference. What follows is the July 24, 2004 letter from the Canadian learned society (The Planetary Association for Clean Energy, Inc.) to Ernest Bauer, President of Monti America Corporation. The abbreviation NORM/NONS, in that letter, stands for Naturally Occuring Radioactive Materi/Naturally Occuring Radioactive Substances.

Dear Mr. Bauer,
The following constitutes our opinion about your achievements to date associated with Monti Process and its application within your Kamloops, British Columbia facility.
As you know, our Association, the Planetary Association for Clean Energy, Inc., is a Canadian learned society that has

developed an international, independent, collaborative network of advanced scientifIC thinking since 1975, under the leadership of the late Canadian scientist Senator, the Hon. Chesley W. Carter.

Our network has been peer reviewing, extensively, and on an international scale, progress with the Monti process, since the 1990s. We continue to do so and consider it as one of the top systems worldwide for the rapid, affordable and efficient decontamination of a variety of nuclear waste materials, including NORM/NONS (which are, as you know, present in rocks, soils, building materials, consumer products such as fertilizers, and a variety of by-products of such common technological activities as the petroleum, the fertilizer, the pulp and paper and the mining industries). Recently we made a presentation before a Canadian Senate Committee, which was received with considerable interest, supportive of the Monti Process for widespread application throughout Canada.

In a step of a series of review procedures that our network has taken to review and facilitate the Monti Process * (footnote), we have attended, in the form of several of our members, and have followed a number of demonstrations and controls in May of this year at the newly established Kamloops facility.

*(footnote) The Monti Process has been tested many times in the past, including at ENEA, the Italian National Agency for New Technology, Energy and The Environment, at the Royal Institute of Technology and Science in Stockholm, Sweden, in Taiwan, and at a previous demonstration in Kamloops, B.C., all of which were monitored by independent scientific observers. In all instances, the results were positive, demonstrating that the Monti process could effectively deplete the radioactivity by up to seventy five percent within three to four days. *(end of footnote)

This facility was established for the purpose of rapidly depleting Naturally Occurring Nuclear Substances (NONS) with the Pyrolytic Monti process. Present at this demonstration as independent scientifIC observers were: John Coleman, BS, MS, Ph.D., Massachusetts Institute of Technology (retired) Domina Eberle Spencer, Ph.D., University of Connecticut Andrew Michrowski, Ph.D., President, The Planetary Association for Clean Energy, Inc. Monique Michaud, M.ScA, CGD, The Planetary Association for Clean Energy, Inc. Mark Porringa, P. Eng., Atomic Energy Canada Limited Philippe Duport, Ph.D., Director, Institute ofthe Environment, University of Ottawa John R. Johnson, Ph.D., IDIAS Inc., University of British Columbia Trevor Beniston, P. Eng., Stuart Hunt & Associates (radiation safety consultants, petroleum industry)

During the May 2004 demonstrations, gas mantle ash containing thorium oxide was used as sample for the rapid reduction of radioactivity. The radioactivity level was reduced by over 50% within 3 days. Measurements for the radioactivity were obtained with sophisticated, state-of-the-art monitors, under fIXed, pre-determined geometric configurations to ensure valid comparisons between measurements made on input material and after the Pyrolytic reactions. The remaining radioactive materials were processed a second time to obtain further depletion of radioactivity.

The independent observers confirmed that no deleterious gases escaped and that the remaining sludge is essentially inert. The metal buttons that remain after firing and cooling are totally inert within 4 days after firing.

We conclude with the recommendation that the Monti Process facility in Kamloops is ready to commence processing radioactive filters from the petroleum industry. We understand that Dr. John Johnson, nuclear safety expert, is currently advising his contacts in various committees, including the ANSI-N13 committee, which is developing a standard for Technically Enhanced Naturally Occurring Radioactive Materials (TENORM) as it affects the radioactivity issue associated with gas and oil production filters. He, being deeply aware of the Monti Process, is indicating that the industry may now submit filters for "ashing", for a practical-size "start-up" run at the Kamloops facility.

We are pleased with the cleanliness, simplicity and rapidity of the process that is now in place for dealing with various NORM/NONS - truly a world first.

We also concur with Dr. Johnson's position regarding the readiness of your facility to deal with certain types of radioactive wastes, indicating that it is in an unique position to resolve the storage problems for the petroleum industry NORM/NONS as well as a solution to eliminate the long-term liability to the industry. The widespread application of the Monti process, in our view, will contribute to the safeguarding of the environment for both present and future generations.

We haw no objection to your showing this letter of opinion to potential users in the hopes that they will consider the advantages offered by the Monti Process and your facility.

Yours sincerily,
Dr. A. Michrowski,
President

References:

1) V.I. Vysotskii, A. Odintsov, V.N. Pavlovich, A.B. Tashirev and A.A. Cornilova; “Experiments on controlled decomposition of water mixture of different long lived active isotopes in biological cells.” That was a paper presented at the ICCF11.

2) E. Anderson, from Monti America Corporation was reading (at ICCF11) a paper of J.W. Colman, R.A. Monti and G.A. Cesarano Monti: “Transmutation of nuclear waste by low energy nuclear reactions.”

P.S. After posting the above I went to Google and typed “Roberto A Monti” This generated a lot of items worth reading. They show the scientific side of Monti. I hope that his contribution to mankind will be real. But I am glad that my money is not invested into his technology. The rule should be "science first, costly applications later." Yes, I know that this rule has often been violated. That is why I remain open-minded.

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187) Role of magnetic monopoles, etc.

Ludwik Kowalski (November 15, 2004)
Department of Mathematical Sciences
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At the last international cold fusion conference in Marseilles (ICCF11, November 2004) I was exposed to new aspect of cold fusion -- magnetic monopoles. Several papers, both theoretical and experimental, were devoted to that topic. I will not comment on these papers because I know very little about magnetic monopoles (1). But one thing is clear -- explaining one mystery in terms of another mystery is not very convincing. As stated by one cold fusion researcher (in a private conversation) this approach is just the opposite from what is widely known as Occam's razor approach.

According Russian and French scientists (2), the Chernobyl nuclear plant explosion was caused by "the impact of a strong magnetic field" (monopole). Such fields, acting on the beta-radioactive fission products, inside the reaction core, decreases the rates of decay of these products. This, in turn, increases the rate of emission of delayed neutrons. Delayed neutrons play an essential role in the control of the reactor. The authors think that "the official explanation of the accident does not seem to be satisfactory." They suspect that the new (monopole) explanation is deliberately suppressed because a large number of existing reactors would have to be closed to prevent similar accidents.

Likewise, according Russian and French scientists (3), the explosion of the French ammonium nitrate plant near Toulouse might have been caused by a magnetic monopole. That 9/21/2001 explosion killed 31 people and resulted in 2500 injuries. The authors state that "in the near neighborhood of magnetic monopoles the magnetic field is in the range of 100,000 teslas." The role of magnetic monopoles in cold fusion was also discussed by a theoretical physicist from Japan (4,5). Another theoretical consideration, related to the above hypotheses (accidents in chernobyl and in Toulouse) was Russian scientists (6,7). The authors show that atomic nuclei can be stable in neutral atoms and instable (K-capture decay) in fully ionized atoms.

Another paper belonging to the category "new to me" was presented by a scientist from Belarus (8). The author thinks that familiar reactions of fusion and fission are 'neutrino-driven." This claim is said to be supported by the experimentally observed correlation between the radioactive decay rates and solar activity. The effect of chemical environment on decay rates also illustrates this point of view. The author informs us that the rates of decay of ^{137}Cs, produced in the Chernobyl accident, were observed to be reduced very considerably in the fallout samples. He also informs us that the "so-called torsion radiation" was observed triggering nuclear transmutations.

References:

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2) D.V. Filipov, G. Lochak, A. A. Rukhadze and L.I. Urutskoev, "On the possible magnetic mechanism of shortening the runaway of RBMK-1000 reactor at Chernobyl Nuclear Power Plant." Paper presented at ICCF11.
3) H. Lehn, L. Urutskoev and P. Stoljarov; "Interaction of magnetic monopoles on polar molecules having a structural instability." Paper presented at ICCf11.
4) T. Sawada, "A brief review of the magnetic monopole and the charge quantization condition." Paper presented at ICCF11.
5) T. Sawada, "Origin of the sporadic nature of the cold fusion and the way to avoid it." Paper presented at ICCF11.
6) D. V. Filipov, A. A. Rukhadze and L.I. Urutskoev; "Effect of atomic electrons on nuclear stability and radioactive decay." Paper presented at ICCF11
7) L.I. Urutskoev; "Low-energy nuclear reactions and the Lochak monopole." Paper presented at ICCF11.
8) V. Filimonov, "Neutrino-driven nuclear reactions of cold fusion and transmutation." Paper presented at ICCF11.

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188) Oriani cell effects

Ludwik Kowalski (November 22, 2004)
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I just returned from a "field trip" to Oriani's lab at the University of Minnesota (UM) in Minneapolis. Who is Richard Oriani? He told me: "my mother and father were both born in El Salvador as I was. He was of Italian descent, and she was of Spanish descent." The family immigrated to the US in 1920s. Richard was born 84 years ago; he thinks he is the oldest active cold fusion researcher. He is also a veteran of the field, exploring it since its very beginnings in 1989. Few were as well prepared for such work as he was. A metallurgist with a Ph.D. in physical chemistry from Princeton, he was, for many years, a director of a large anticorrosion institute at the UM. I met him for the first time at the 2003 cold fusion conference (ICCF10). He and Fisher presented two papers describing the use of CR-39 detectors to detect showers of nuclear particles in the liquid and above the liquid of a running electrolytic cell. That inspired me to start using such detectors. We corresponded by e-mail (see unit #108 at this website) and at one point -- it was several months ago -- I asked if he could help me design a simple and convincing cold fusion experiment for teachers and students. He agreed and last week's trip was the result of that invitation. I wanted to see the experiment with my own eyes.

Oriani has several electrolytic cells. I will describe just one of them. He calls it a "vertical cell;" I will refer to it as an "Oriani vertical cell." It is essentially a glass tube whose bottom (cathode) consists of a metallic foil, for example nickel of 0.125 mm. The tube is half-filled with a solution of Li_2SO_4 in ordinary water. The anode, made from a platinum wire, is inserted into the solution from the stopper, at the upper end of the tube. That stopper has an opening; its purpose is to allow oxygen and hydrogen, produced during the electrolysis of water, to escape into the atmosphere. The electrodes of the cell are connected to a power supply (about 5 volts) and a current of 0.2 amperes is allowed to flow. Nothing unusual so far; it is a simple electrolytic cell producing oxygen and hydrogen from water. Several anomalous effects were discovered with this device. I will refer to them as: (a) Oriani liquid effect, (b) Oriani vapor effect and (c) Oriani solid effect. I was privileged to perform experiments demonstrating each of these effects. Papers (1,2) describing the (a) and (b) effects have been available on the Internet for more than one year. The (c) effect was described in a paper presented at the last cold fusion conference in Marseilles (3).

In what follows I focus on the Oriani solid effect. It consists of demonstrating emission of charged nuclear particles from the nickel cathode during and after the electrolysis. The particles were recorded in a detector placed in air, 1.5 cm below the nickel foil. I have no doubt that some kind of nuclear activity is triggered in the Oriani cell during the electrolysis.

In my opinion, Oriani effects should not be labeled "cold fusion", as defined in 1989 (4); neither palladium nor heavy water are used in the setup. What happens in the cell cannot be a fusion of two deuterium nuclei. This would be highly improbable due to repulsion of positive nuclei and due to the quasi-absence of deuterium in ordinary water. If it were up to me I would replace the term "cold fusion" by the term "anomalous nuclear activity" and would invent separate names for different effects, such as: generation of neutrons, excess heat, production of tritium, accumulation of helium, transmutation of isotopes, etc., etc. But, being realistic, I know that the term "cold fusion" will be used (inappropriately) as an umbrella for a large number of effects (that may or may not be related). That is how I am going to use this term.

The figure below is a simplified drawing of the Oriani cell. I plan to use such cell in my own experiment at home. Note that the nickel foil is squeezed between the two rubber O-rings (dark circles). The two vertical tubes (inner diameter 1.5 cm), into which the O-rings are inserted, are called "flanged joints." The two aluminum plates that press the joints toward each other (with screws) are not shown. The upper tube is partially filled with the electrolyte into which a

platinum wire is inserted. The lower part of the wire, a pancake-like spiral (diameter of about 1.3 cm), is used as an anode.

Chemicals needed (NaOH and Li_2SO_4), are likely to be available in a typical school laboratory. Chemicals can also be purchased, for example from Aldrich Chemical Company, Inc. The estimated cost of major components is:

Two flanged joints, $20
Six pure nickel foils, 0.125 mm, 15 by 15 cm each (enough for 30 cells) $150
Lithium sulfate, 99% pure, 100 grams (enough for hundreds of experiments), $21
Sodium hydroxide, 1 kg, (enought for hundreds of experiments), $26
Platinum wire (0.25 mm diameter and 25 cm long), $30

All these components can be purchased, for example, from Aldrich. The most expensive components, the power supply and an optical microscope, are likely to be available in a typical school laboratory. The microscope magnifications of 40 and 200 are sutable. The clamps holding the joints togrther can be quite expensive, for example, $150, if custom-made in a commercial machine shop. But a pair of large washers, with three matching holes along perimeters, can also be used to push the joints toward each other with a set of three screws. The inner openings of washers should be slightly larger than outer diameters of tubes. A set of two brass wall flanges, from Lido Rail Systems (costing $6.50 in Home Depo), is an ideal clamp for tubes whose outer diameters are between 0.8 and 1.0 inches. This is only an example; any set of rigid curtain rod holders, with holes, would be suitable, provided it matches the tube diameters.

The actual vertical cell used by Oriani is only slightly more complicated than that shown in Figure 1; it has wires to suspend additional detectors, some to be kept in the liquid and others to be kept above the liquid. These additional detectors are used to study the (a) and (b) effects. I plan to replicate only the (c) effect. An experiment for teachers and students should be as simple as possible.

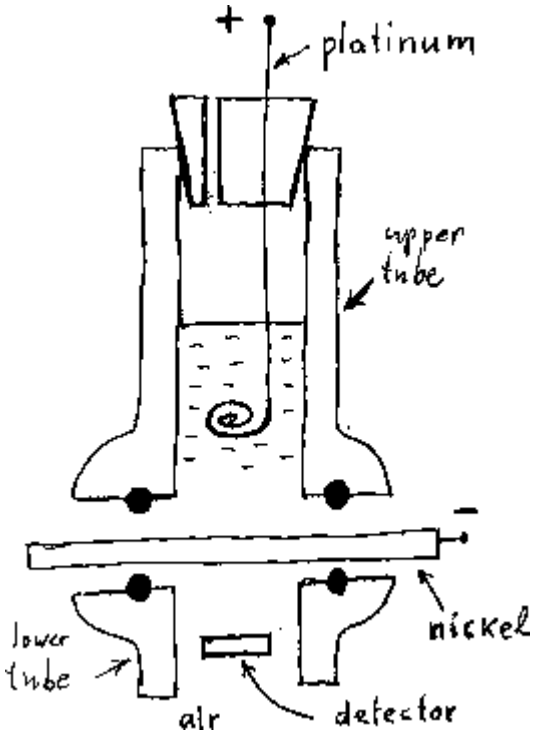

Figure 1

Let me now describe one of the experiments performed in Oriani's lab. We inserted a new nickel plate and prepared fresh electrolyte (2.36 grams of Li_2SO_4 per 100 cc of the ordinary water). The surface of the nickel, to face the

electrolyte, was scratched with a sharp knife. Oriani said “this can not hurt, it might help.” Then the cell was assembled, the electrolyte was introduced into it (with a pipette) and the current was turned on for 72 hours. During the first 24 hours the current was 0.20 A, during the next 24 hours it was 0.27 A, and during the last 24 hours it was 0.33. Oriani said: “I do not know which current is the best so I use several different currents.” A large number of tracks of nuclear particles was recorded by the detector situated in air below the cathode. The distance between the detector and the cathode was 1.5 cm.

After three days the electrolyte was removed and the cell was dismounted. The nickel cathode was at once sandwiched between two CR-39 detectors (see the appendix below) for 26.5 hours. Once again, a large number of nuclear particle tracks was recorded. Figure 2 is a typical photo of tracks seen unser the microscope. The surface area shown in Figure 2 is very small; most of the areas of that size, for control chip measuring the background, display zero or one tracks. Encountering three tracks on such small area is very rare. A typical background for our detectors is 10 tracks per square centimeter (fluctating between 0 and 30).

72 hours of exposure during the electrolysis

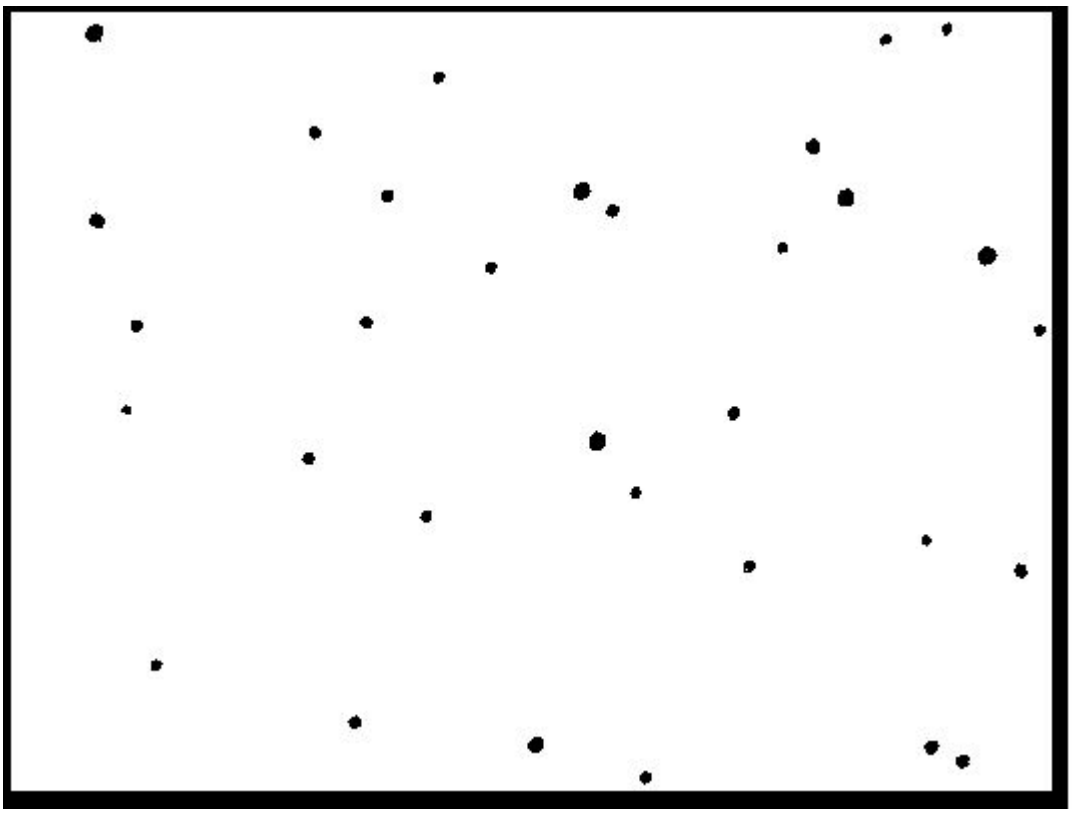

Figure 2

The area of the detector used during the electrolysis was about 0.6 cm^2. The number of tracks recorded on it was found to be 1816. This translates into the track density of 3026 per square centimeter and to the recording rate of 42 counts per hour per square centimeter. That is about 300 times higher than our typical background. In other words, what is shown in Figure 2 is certainly not the residual tracks due to radon and cosmic rays. Was the rate of emission constant during the electrolysis? An experiment in which the CR-39 detector is going to be replaced by an electronic (surface barrier) detector is in preparation. After that experiment Oriani should know the answer to this question. He will also measure the energy spectrum of unexpected particles emitted by nickel during the electrolysis.

Tracks recorded after the end of electrolysis.
The cathode was removed from the cell and immediately sandwiched between two fresh CR-39 detectors for 26.5 hours. After that the detectors were etched and tracks were counted on all four surfaces. Each surface had an area of 1.2 cm^2. The results are shwn below. The terms “front” and “rear” are used by Oriani to identify two surfaces of a single chip.

Front is the side facing the cathode and rear is the opposite side. The cathode also has two distinct surfaces, one called "wet" (because during the electrolysis it was in contact with the electrolyte) and another called "dry" (because during the electrolysis it was in contact with the outside air). The detector facing the "wet" cathode recorded 951 tracks in "front," and 122 in "rear." These numbers are significantely different than what was detected on the "dry" side of the cathode. The detector placed on that side recorded 132 tracks in "front" and 7 in "rear." Note that 951 tracks translates into 30 tracks per hour per square centimeter.

The second test for the emission of particles (with a fresh pair of CR-39 chips) started 27 hours after the end of electrolysis and lasted 70 hours. During this test the detector facing the "wet" cathode recorded 1529 tracks in "front" and 373 in "rear." These numbers are significantely different from what was detected on the "dry" side of the cathode. The detector placed on that side recorded 80 tracks in "front" and 22 in "rear." Note that 1529 tracks translates into 18 tracks per hour per square centimeter. Keep in mind that the geometry of counting during the electrolysis (detector 1.5 cm below the nickel cathode) was very different from the geometry of counting after the electrolysis (when each detector was in contact with the nickel foil).

Considering a possibility that our nickel foils (nominally 99.99% pure before the electrolysis) might contain traces of naturally radioactive materials, such as uranium or thorium, we performed an additional experiment. A fresh piece of nickel, cut from the same large piece as the cathode, was sandwiched between a pair of CR-39 detectors, also for 26.5 hours. After that exposure the detectors were etched and examined. All four surfaces had nothing but the background. This rules out a possibility that particles, detected during the electrolysis, or after the electrolysis, were due to a contamination.

Was the emission of anomalous nuclear particles after the electrolysis uniform during the 26.5 hours of exposure or were most of the particles emitted quickly after the current was turned off? Additional data are needed to answer this question. I will append more information, about our investigations of the Oriani solid effect, as more data become available. Information from the electronic detector is likely to shed much more light on the nature of the unexpected nuclear effect triggered by chemical processes in the cell. For the time being we think that tracks recorded by CR-39 detectors are due to alpha particles and protons. This would be consistent with two distinct groups of track diameters, large and small. Protons and alpha particles have already been observed, in very different cold fusion experiments, by other investigators (5,6). My personal expectation, based also on the experiment described in unit #184, is that some kind of radioactivity, with a long half-life, is somehow induced in nickel. But the only things we know for sure, so far, is that: (a) charged nuclear particles were detected in the air below the nickel cathode during the electrolysis, and, (b) the cathode was emitting such particles after the current is turned off. I will stay away from trying to explain these strange phenomena.

Let me end this unit by inviting other people to replicate our observations. According to Oriani, about 60% of attempts to observe the effect (seeing track densities high above the background) were successful. This is based on about thirty attempts. My personal experience, in Richard's lab, was two undeniable successes in two attempts. Some people say that an experiment should be 100% reproducible before it can be taken seriously. I disagree; even 10% is sufficient, provided a qualified person, such as Jones, Fleischmann or Oriani, is reporting something new and unexpected. To be taken seriously does not mean to be accepted as real; it means to study a phenomenon intensively until it becomes 100% reproducible, at least approximately. A qualified researcher should never be denied access to a scientific journal without an honest peer review process , Ridiculing scientists whose reports can not always be confirmed does not promote our understanding of nature.

The task of those of us who will start studying the Oriani effects would be to identify a factor (or factors) responsible for the lack of total reproducibility. Is it humidity? Is it pressure? Is it time of the day? Is it the number of solar flares? Is it the noise level? Is it proximity of a magnet? Is it the day of the week? Is it wishful thinking? Is it dust? Is it experimenter bias? If the effect is real, and if a large number of people are studying it, then, sooner or later, the experimental conditions needed for the 100% reproducibility will be identified. It is so unfortunate that Oriani's attempts to publish a paper describing similar results was recently rejected by several physics journals. How can science develop if qualified researchers are not allowed to make their findings known? I hope that such abnormal discrimination (of a field of research) will soon be condemned by those responsible for scientific research. When will the DOE appointed investigators publish their report?

P.S.(11/23/04)
Oriani wrote to me today that the vapor effect (b) seems to be more reproducible than the solid effect (c), described in this unit. Should I write a separate unit devoted to the vapor effect? I might, when time becomes available. For the time being I want to stop writing, and start working. I hope to observed mysterious nuclear particles, below the Oriani cell, in my laboratory.

P.S.(11/24/04)
Oriani also discovered tracks of nuclear particles on detectors placed in air ouside the glass tube, during the electrolysis. Referring to these recent findings he wrote to me: "I have found greater reproducibility with chips in the vapor than with chips stuck to the outside surface of the glass of the cell." Then he suggested that I focus on the vapor effect rather on the solid effect. I resisted; in the reply I wrote: "Since you are not categorically against focusing on the solid effect I will focus on it, as originally intended. The advantages are: simplicity (no need for the spot-welded hooks, no need for the 'roof plate' above the liquid, no need for the anicondensational heating of the vapor) and a possibility of replacing the CR-39 chip with an electronic detector. I do plan on using the electronic detector, after confirming your experiment once again at my own place. Your vapor effect deserves its own unit and I will try to compose it later."
The photo of Richard Oriani, taken by me, is shown below.

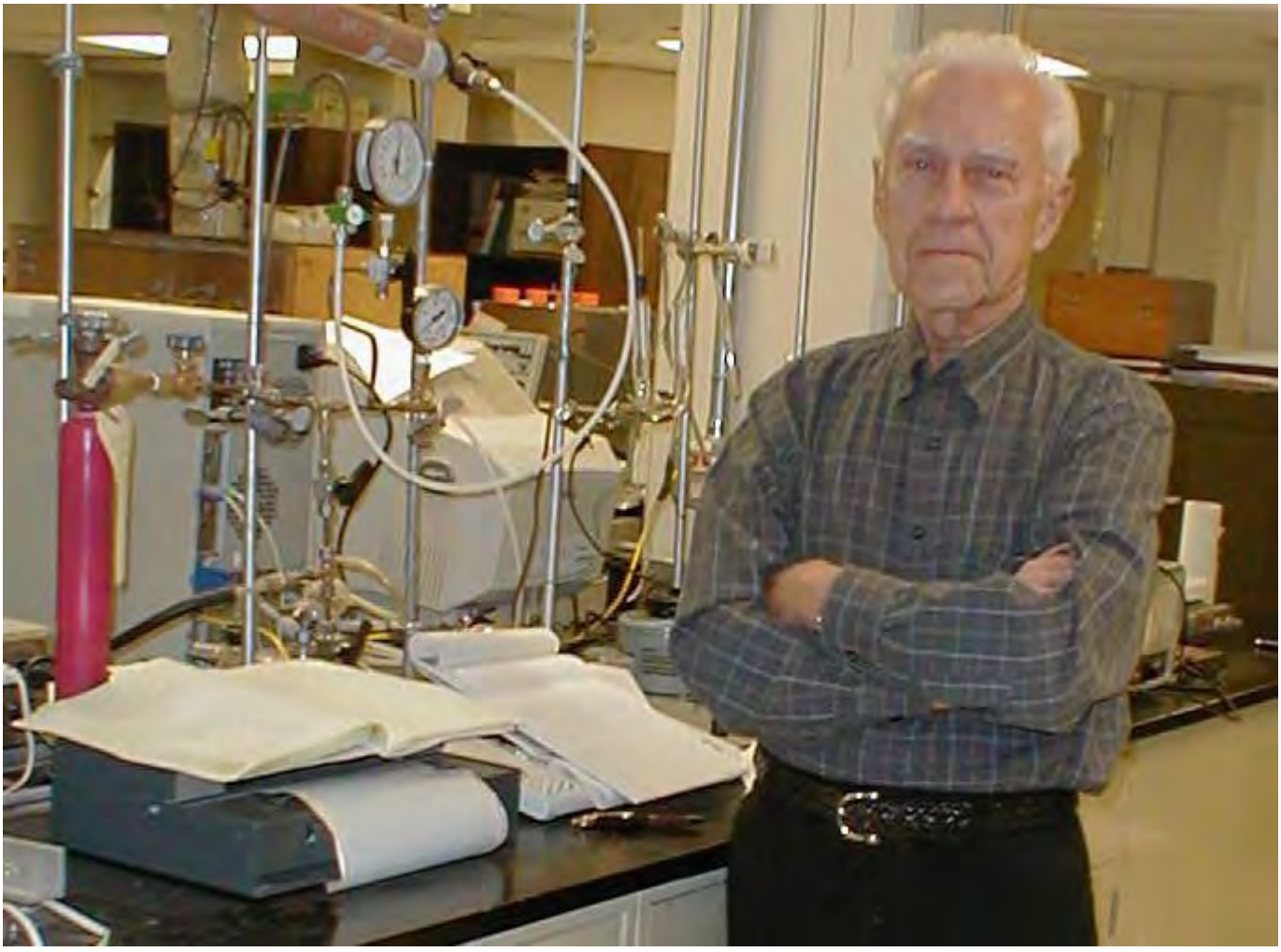

Figure 3

The next picture shows Oriani vertical cell during our experiment. Note the hexagonal plates squeezing the two O rings toward each other; these plates were not shown in Figure 1. The electrolyte in the upper tube (white) is full of escaping bubbles. The space above the electrolyte is heated from the power supply on the left. This prevents condensation of vapors on CR-39 pieces used to study the Oriani vapor effect. The power supply on the right is used to control the current flowing through the cell.

Figure 4

The last picture below was taken at the recent cold fusion conference (November 2004) in France. It shows two theoretically inclined cold fusion scientists (T. Chubb on the left, and J. Fisher) discussing something. Their attempts to make sense out of cold fusion phenomena are very different. They are not alone; I know names of several other scientists advancing their own theoretical interpretations. Note that most cold fusion researchers are old. Young people are usually told to stay away from that field of research. What a shame! They should be actively involved in trying to either validate or invalidate experimental data and emerging theories. In my opinion cold fusion is an ideal ground for practicing scientific methodologies because experiments are relatively simple and relatively inexpensive. Who is responsible for the unhealthy social environement surrounding cold fusion? Those who still think that all cold fusion phenomena are pseudoscientific. Will the situation be corrected after the report of the DOE appointed panel is published? I hope so.

Figure 5

P.S.(11/25/04)
A friend who read this unit asked about the energy balance. My answer was that “no measurable amount of excess heat would be expected on the basis of the number of observed tracks, even if each track was associated with 2000 MeV of heat. This number is eleven times larger that the energy from a single fission of uranium nucleus. The way to demonstrate this is simple. Assuming the average voltage and current are 6 V and 0.3 A, respectively, the input power is 6*0.3=1.8 W. The power associated with the 1816 tracks, recorded in 72 hours, would be (under the above assumption) 2000 MeV * 1816 /72 = 50440 MeV/hour. This translates into $3.11*10^{-14}$ W. Assuming that tracks detected by the CR-39 located 1.5 cm below the cathode represent only 5 % of tracks emitted in all directions the output power becomes $6.2*10^{-13}$ W. This is nearly nothing in comparison with the input power of 1.8 W. In other words, the number of reaction products is too small to produce a measurable amount of excess heat during the electrolysis. This is consistent with earlier observations of other researchers (5,6). The assumption of 2000 MeV of energy per track was deliberately exaggerated in favor of excess heat. The real energy per track is likely to of the order of several MeV.

P.S.(11/25/04)
Would a person who is convince that "cold fusion is nonsense" read my paper? I do not think so. Like many others, including people I know personally, s/he probably thinks that it is a waste of time to read research reports in the area of cold fusion. But suppose s/he was forced to come to our conference. Would s/he use earplugs to avoid listening to what Oriani, or other active scientists, had to say about recent findings? Probably not. And would s/he accept an invitation to assist in a real experiment, like the one I am going to perform next week? That would be an indication that s/he is ready to change previousely held vews in face of new evidence. Why is such step so difficult for many people?

Appendix: The CR-39 detectors
. . . . . .
Most scientists are aware of the role played by track detectors in nuclear physics. A large number of discoveries were made by using first track detectors: cloud chambers and nuclear emulsions. CR-39 detectors, like nuclear emulsions, can be developed to show tracks made by protons and alpha particles. They are made from the same matrial (labeled CR-39) as plastic eye glasses. The process of development consists of etching the exposed material in a hot solution of NaOH (6.5 N at 65 degrees C) for several hours. Our method of detection of the unexpected particles is essentially the same as that used to detect alpha particles from radon, for example in air or water. Fortunally, the CR-39 material is

commercially available (7) in the form of flat sheets or chips, typically ~1 mm thick. An alpha particle, for example from radon in a basement, intercepted by a CR-39 chip, creates a tiny track of damaged chemical bonds that can be made visible by etching (7). Tracks can then be observed by using a common microscope (8). An extensive review of track detectors has been published recently (9)

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189) Summary of my CF research

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My cold fusion investigations, reflected on these webpages, started during the sabbatical year of 2002/2003. It was essentially a literature research project on the topic that I had believed to be pseudoscientific for about ten years. After changing that attitude, on the basis of what I read, I started looking for a simple cold fusion experiment that could be performed by teachers like myself, and by their students.

The first attempt in that direction -- working for one week with H. Fox and A. Jin in Salt Lake City -- was a failure. What we discovered was a poor interpretation of experimental results from an earlier investigation (see units #44 and #45).

The second attempt was a visit to Provo, also in Utah, to talk with S. Jones at Brigham Young University. That was after I met Jones at the 10th cold fusion conference (August 2003), and after I heard his report (see unit #113). The visit led to my investigation of a Ti foil impregnated with deuterium. The sample was sent to me by Jones; it was made by keeping the titanium foil in deuterium gas at high temperature and pressure. We wanted to see if the 3 MeV protons, discovered by Jones, can also be observed by using a CR-39 detector. That approach would be more suitable for a student-oriented project than using sophisticated electronic detectors.

The result of my study confirmed Jones' observations but they were not as spectacular as the results from the investigation of another foil (see the description starting in next paragraph). The number of tracks counted on the face of the CR-39 detector that was applied to Jones' foil (for 55 days) turned out to be 225 The opposite side of the same CR-39 detector (exposed to air) was used to count tracks due to our background. That number of background tracks turned out to be 132. The size of the CR-39 detector was one square inch.

The third attempt to observe "a nuclear signature" in cold fusion was my collaboration with D. Letts, as described in units #182, #183 and #184, at this website. Once again, I used CR-39 detectors (see item #185 on this website). Three palladium cathodes (used in electrochemical cells) were sent to me: Pd613, Pd616 and Pd615. The first one was from a cell that produced a very high amount of excess heat, the second was from the cell that produced significantly less excess heat and the third was from the cell that failed to produced excess heat. Without knowing anything about thermal histories of these cathodes I found a huge number of tracks due to Pd613, much less tracks due to Pd616 and only background tracks due to Pd615.

I was subsequently informed all three cathodes were cut from the same Pd foil, and that the electrolyte used in all three cells was prepared at the same time. But a tiny amount of the so-called "sauce #1" was later added to the cell containing the Pd613 cathode. The X-ray fluorescent analysis of the sauce revealed presence of uranium. (Dennis Lets wrote: "The additive I used on cathode #613 was Cravens sauce #1. This is the same additive titled "Pixie Dust" on the analysis done by Scott Little showing small amounts of Uranium.") Be aware that no sauce #1 was added to the electrolytes used in the other two cells. I asked Dennis to send me a sample of the souse. He did and I dried it under a lamp. Then a CR-39 detector was applied to the thick dark spot of what was left after the liquid has evaporated. After three weeks that detector revealed a huge number of tracks. A test for the contamination of the Pd613 cathode itself consisted of its reexamination after three weeks. The number of tracks observed, in the cluster of highest density, was essentially the same as three weeks earlier. In other words, an attempt to rule out the contamination failed. (I would be able to say that there were no uranium contamination if the track density decreased significantly in three weeks. But this did not happen.)

Facing this situation I decided to ignore the Pd613 data, for the time being, and use the data from the Pd616 and Pd615 cathodes. No sauce #1 was added to the electrolyte when these two cathodes were used in the excess heat experiments. The Pd615 provided an ideal control sample; it was treated in exactly the same way as Pd616 but failed to generate excess heat. A very large difference between the track densities from the Pd616 and Pd615 was a strong indication that an unexpected nuclear effect is indeed associated with generation of heat in Letts experiments.

I think that excess heat demonstartions, designed to convince that something highly unusual (cold fusion) is taking place, should always be accompanied by attempts to display nuclear signatures. That what I wrote to Letts when I asked him for a chance to examine a cathode. After all, there are many ways to get more heat out than heat in, especially at the power level below one watt. A complete examination of all chemical processes taking place in a setup (to convince that the excess heat is not chemical) is much more demanding that using a nuclear detector of some kind. Cold fusion effects, if they are nuclear, must generate nuclear reaction products, either radiaoactive or stable. Nobody argues with this expectation. Scott Little, from Letts' team, wrote to me that they will start using CR-39 detectors. I expect them to apply CR-39 to each cathode as soon as the electrolysis is finished (not much later, as I did). Will they observe a decrease in track densities after subsequent reexaminations of a removed cathode, for example, once every two week? That remains to be seen. The team plans on announcing the results of their recent findings at the end of this year.

Let me now return to the cathode #613. The CR-39 detector applied to the cathode for the second time, three weeks after the end of the first exposure, showed no significant decrease in the track density. I consider this to be a failure to rule out the effect of uranium contamination. But this does not prove that contamination was responsible for most of the observed tracks. It is not apriori evedent that a two or three drops of souce #1, in which concentration of uranium was very low, could produce (after being diluted in the electrolyte) as many tracks as was actually observed. A good way to test for the uranium contamination would be to replace the CR-39 detector with an electronic detector able to identify particles (are they alpha particles or not?) and able to determine their energies (are the uranium-chain peaks observed or not?) Let me now focus on two experimental facts that are not consistent with the idea of contamination.

(a) The electrolytic cell had a nearly axial symmetry. It was a round beaker with a small cathode (about one square centimeter) suspended at the center. The anode was a spiral platinum wire surrounding the cathode. The radius of the spiral, as far as I could determined from a photograph emailed to me, was about three centimeters. The only asymmetry was introduced by the laser beam; one side of the cathode was exposed to that low energy beam during the excess heat experiment while the other side was not. The laser beam was expected to trigger the mysterious excess heat phenomenon.

(b) If the tracks from particles emerging from the Pd613 cathode were due to uranium dissolved in the electrolyte then the track density would be essentially the same on both sides of the Pd613. In reality, however, the number of tracks on the CR-39 detector applied to one side of the cathode was very much higher than on the detector applied to the other side of the cathode. Furthermore, the track density (on the side of the cathode that was more active than the other) was very nonuniform. I suspect that the observed cluster (of maximum track concentration) coincides with the spot at which the laser beam was intercepted by the cathode. It would be interesting to know if that suspicion is confirmed by Letts' team. On the other hand, I am aware that, at least in principle, uranium (from the uniformly dissolved sauce) could prefer to migrate to one particular spot on the surface of the cathode.

My fourth attempt to find a student-oriented experiment showing that a nuclear process can be caused by a chemical process was the most successful. It was described in the unit#188 on this website. The experiment is simple and relatively inexpensive; the total cost of materials to build a cell should not exceed $200. I will have more to say about this experiment after trying to replicate it in our laboratory at Montclair State University. A task of trying to prove, or disprove, a controversial claim made by a reputable scientist can be turned into an educational project. What can be a better way to expose students to the excitement of scientific research? That is why I am trying to convince teachers that Oriani's experiments are worth replicating. Please read my unit #188 and try to replicate the experiment. Then share your results. I would be happy to write about your findings in another unit, perhaps before the end of the current school year.

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190) A better name for “cold fusion?”

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Naming something is an important component (not the most important) of promoting it. This is probably a well known fact among advertisers and propagandists. The label “cold fusion,” as often stated, is not appropriate even as a generic term for a broad group of phenomena. Other names have recently been suggested, for example: LENR (low energy nuclear reactions); CANR (chemically assisted nuclear reactions); and CMNS (condense matter nuclear science). Before suggesting another name let me say why the existing names are not ideal, in my opinion.

(a) The name cold fusion (CF); would be appropriate for a reaction in which two atomic nuclei fuse into one at not-too-high temperatures. (Temperatures can be expressed in units of energy and by “not-too-high” I mean one eV or less. Only stellar temperatures are high enough to make fusion of atomic nuclei observable.) Fusion of two isolated atomic nuclei is highly improbable at ordinary temperatures. That theoretical conclusion (the tunneling effect) is not disputed by cold fusion scientists. But they often argue that, in condensed matter, fusing nuclei are not isolated. I will return to this in (c) below.

(b) The LENR and CANR names are not ideal because they contain the word “reaction. ” That word also implies interactions between two particles. The particles are atoms or molecules, as in chemistry, or atomic nuclei, as in nuclear science. The name LENR, by the way, has already been used for something else. In most nuclear textbooks low energy nuclear reactions are defined as reactions at which the kinetic energies of colliding nuclei do not exceed several tens of MeV. Using the same name for two different things can be confusing.

(c) The most recently introduced name, CMNS, is not ideal because the term “condensed matter” excludes gases. One of the effects discovered by Oriani (see unit #188), does seem to be taking place in a vapor. Likewise, the sonoluminescent nuclear anomaly seems to be taking place in bubbles.

The name I would like to suggest is CANA -- “chemically assisted nuclear anomaly” or “chemically assisted nuclear activity,” depending on how much is understood. The name is broad enough to include all experimentally observed (anomalous nuclear) effects; it has not been used to describe something else, and it is easy to pronounce, at least in languages with which I am familiar.

Of course, each CANA phenomenon should have its own distinctive name. Generation of excess heat, associated with an anomalous nuclear activity, for example, can be named “Pons effect;” it was first announced by Pons and Fleischmann. Emission of neutrons and protons, discovered by Jones et al., can be named “Jones effect” and phenomena described in the unit #188 can be called “Oriani effects.” Associating new effects with names of those who discovered them has been common in physical sciences. Not all effects, however, are named after their discoverers. The photoelectric effect, for example, does not reflect the name of the scientist who was the first to publish its discovery, or its interpretation. Such approach would likely be appropriate for situations in which effects have been discovered, more or less simultaneously, by several researchers.

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191) Polyneutron model of John Fisher

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1) Introduction:
During my visit to Minneapolis (see the unit #188) I asked Dr. Oriani about his motivation for placing CR-39 detectors into the vapor. Most cold fusion researchers think that the unexplained nuclear phenomena (inappropriately referred to as cold fusion) occur in metals. His answer surprised me. He said "I was guided by a theory of John Fisher." Up to that time I thought that not a single cold fusion phenomenon was predicted theoretically, all of them were discovered experimentally first. Who is Dr. Fisher? He was trained as a mechanical engineer, and as a mathematician. His early professional life was spent in a General Electric research center at Schenectady, NY. A photo of that retired theoretical scientist is shown in the unit #188.

John conceived polyneutrons, more than ten years ago, as "particles" responsible for all cold fusion phenomena. Do these theoretical particles really exist? That is an open question. This situation reminded me the history of neutrinos that were also invented theoretically by Pauli. I talked with John briefly during the tenth cold fusion conference and much longer during the last conference in Marseilles. In a private conversation he told me that many cold fusion people do not take his theory seriously. "Their attitude toward me is similar to the attitude of the official scientific establishment toward all of us." That was another revelation; up to that point I was under the impression that cold fusion people do not criticize each other. I thought that they probably support each other in fighting with the common enemy -- the official scientific establishment. That establishment consists of managers of government laboratories, directors of financial support agencies, editors of journals and numerous self-appointed guardians of our current paradigm.

2) Neutrium, the first element:
The first assumption of John's theory is the existence, in our environment, of a not-yet-recognized element number zero. Atoms of that element contain neither electrons nor protons; they are aggregates of neutrons bound by strong nuclear forces. He visualizes them as electrically neutral droplets of condensed nuclear matter. They are neutral atomic nuclei of an element that I would like to be named. All known elements have names and symbols. Symbols are needed to write nuclear reactions. Symbols of elements consist of one or two letters; the first letter is always capital, as in Zn, O, or Fe.

In this essay I will refer to the element # zero as Neutrium and I will use the symbol Nt to represent it. The name neutrium was suggested to me by John Fisher. He refers to isotopes of that element, such as ^{4}Nt, ^{5}Nt, ^{6}Nt, ^{7}Nt, etc., as polyneutrons. The symbol ^{A}Nt is used for a polyneutron containing A neutrons. Unlike ordinary elements, neutrium was not discovered experimentally, it was invented by John to explain anomalies of the so-called "cold fusion." This reminds us of a neutrino, a particle invented in 1930s to make sense of the apparent anomaly in beta decay.

John Fisher thinks that polyneutrons can contain more than 1000 neutrons kept together by strong nuclear forces. A neutron star, on the other hand, is not a gigantic polyneutron because its neutrons are kept together by gravitational forces. John is aware that polyneutrons of size two or three can not exist. He believes, on the basis of reference (1), that the smallest possible polyneutron consists of four neutrons. In trying to explain stability of nuclear droplets made from neutrons, and knowing that two neutrons "do not stick," John brings an analogy with liquid helium. Two or three atoms of helium do not make molecules but liquid helium exists. I would like to know what other people, more knowledgeable that I am, think about that analogy.

In answering one of my questions John Fisher said that "polyneutrons arise in nature as decay products of exceedingly rare precoursor nuclei." This, however, does not necessarily mean that one has to cover a lot of territory to encounter one or two of them. Suppose there is only one precursor nucleus per quintillion (10^{18}) molecules of air. That would certainly make them "exceedingly rare." But, even at such level of concentration, every cubic inch of air would still contain nearly 1000 of them to produce polyneutrons when they decay. The main point here is that "atoms" of neutrium are usually available to produce various effects, provided favorable conditions are encountered. What is meant by favorable conditions is explained below.

3) Unusual nuclear reactions:
Unlike electrically charged projectiles, polyneutrons are not repelled by ordinary atomic nuclei; nuclear reactions induced by them are not inhibited by repulsive coulomb forces. All cold fusion abnormalities, according to Fisher, are due to nuclear reactions that involve polyneutrons.. Another alternative is to think that cold fusion phenomena are due to ordinary neutrons. John rejects this alternative because far too few neutrons are seen in cold fusion experiments. He also rejects models postulating that coulomb barriers can somehow be lowered. In other words, Fisher thinks that the term "cold fusion," as defined in 1989, is totally inappropriate. Taking coulomb barrier arguments very seriously he does not believe that two deuterium nuclei, for example, can fuse at ordinary temperatures.

A nuclear reaction can be either exothermic or endothermic, depending on its Q value. This topic is reviewed in the Appendix 1. Here is an example of a nuclear reaction involving two polyneutrons:

$$^{A}Nt + {}^{18}O \dashrightarrow {}^{16}O + {}^{(A+2)}Nt \text{.....(1)}$$

The letter A is used to indicate the atomic mass number (number of neutrons) of the polyneutron on the left side of the equation. The polyneutron on the right side contains two more neutrons than the polyneutron on the left side. This particular reaction turns ^{18}O into a common isotope of oxygen, ^{16}O. Another possible polyneutron reaction, according to Fisher, is:

$$^{A}Nt + {}^{2}H + \dashrightarrow {}^{3}H + {}^{(A-1)}Nt \text{..... (2)}$$

Here the mass of the polyneutron decreases by one unit while the mass of the hydrogen increases. One can imagine many nuclear reactions of that kind. But not all of them can occur spontaneously. Most are possible only with an accelerator. Whether or not a reaction can take place spontaneously, at an ordinary temperature, depends on its Q value. The well known way of calculating Q values of nuclear reactions is reviewed in the Appendix 1. Only exothermic reactions, those whose Q values are positive, can happen spontaneously. That is why the ability to calculate Q is so important. To calculate the Q value of a reaction involving isotopes of neutrium, such as:

$$^{A}Nt + {}^{18}O \dashrightarrow {}^{16}O + {}^{B}Nt + {}^{C}Nt \text{.....(3)}$$

one must know exact masses, or mass excesses, of all participants. The concept of the mass excess is reviewed in the Appendix 1. Note that in this reaction ^{18}O is transmuted to ^{16}O and a polyneutron of size A is split into two smaller polyneutrons. Naturally, the sum B+C must be equal to A+2. Mass excesses of ordinary nuclei are well known; they can be found in many nuclear physics textbooks (3). But how can the mass excess of a polyneutron, with atomic number A be determined? According to Fisher that quantity, , the mass excess $\Delta(^{A}Nt)$, is given by:

$$\Delta(^{A}Nt) = k*A$$

where A is the number of neutrons in the polyneutron and k is an unknown constant. This formula, based on the liquid drop model, is only an approximation. The term associated with the surface energy is ignored; only the volume energy term is retained. To impose a limit on the numerical value of k John notes that from the beginning of his theory he "had the idea that chain reactions were involved and that atoms such as ^{7}Li, ^{18}O, and ^{2}H might serve as fuels. When Oriani and I later observed a shower of about 150,000 alpha particles in the oxygen/hydrogen vapor over an active electrolysis cell, I assumed that oxygen could support a chain reaction, which requires that the Q value of reaction (1) must be positive." The initial statement that the Q of the reaction (1) is positive is essential in Fisher's theory. Should we say that this statement is based on experimental facts or should we say that it is a very clever arbitrary, assumption? I want to know what a philosopher of science would say about this.

If we accept that the Q of the reaction (1) is larger than zero then, as show in the Appendix 2, k must be smaller than 1.98 MeV/u. Note that the unit of $\Delta(^{A}Nt)$ is MeV while the unit of A is u (atomic mass unit). That is why, the unit of k is MeV/u.

"The other limit imposed on k i based on another experimental observation. Spontaneous generation of tritium from deuterium has been reported by cold fusion researches." Assuming that this transmutation is due to the reaction (2), Fisher states that the Q of this reaction must also be positive. This, in turn, as explained in the Appendix 2, means that k must be larger than 1.82 MeV/u. These two limits narrow the range of values of k to $1.82 < k < 1.98$. The middle of that narrow region is 1.90 MeV/u. On that basis John assumes that k is equal to 1.90, plus or minus about 4.5%. By knowing k he is able to calculate Q values of many reactions involving polyneutrons, as illustrated in the Appendix 2.

4) The chain reactions process:

The ^{18}O isotope exists in air, water and mineral surrounding us; about 0.2% of all oxygen on earth is ^{18}O. Reaction (1) is a growth reaction. Colliding with successively encountered ^{18}O nuclei a small polyneutron can grow and grow. Reaction (3) is a splitting reaction. Here a single polyneutron disappears and two smaller polyneutrons appear. The combination of reaction (1) and reaction (3) amounts to a chain process. Under favorable conditions (to be explained later) a polyneutron grows through frequent encounters with ^{18}O. When it become large enough (typically when A becomes several hundred) the reaction (3) occurs and the large polyneutron splits into two smaller polyneutrons.

These two also start growing through the reaction (1) till they split through the reaction (3). After that we have four polyneutrons. Subsequent repetitions of reactions (1) and (3) bring the number of neutrons to 8, 16, 32, etc. Both reactions (growing and splitting) are exothermic and nuclear energy is released in this chain process. John compared it to a chain reaction in a typical nuclear power plant. The ^{18}O, according to Fisher, is not the only fuel able to support a chain process. In a recent message he illustrated this with an interesting example:

$$^{A}Nt + {}^{7}Li \dashrightarrow + {}^{4}He + {}^{1}H + {}^{(A+2)}Nt \quad \text{.....(1')}$$

where a stable isotope ^{7}Li, interacting with ^{A}Nt, brakes into ^{4}He and ^{1}H. This is accompanied by the growth of the polyneutron, as in reaction (1). The Q value of this exothermic reaction is 1.39 MeV. A sequence of many (1') reactions, followed by the reaction (3), is a single step of a chain process. The fuel in this energy generating process is ^{7}Li. Is it conceivable that accumulation of helium, reported by several cold fusion researchers is due to the reaction (1')? Answering this question John wrote: "Yes some helium comes from the ^{7}Li reaction. But some comes too from the decay of poisons and some from the decay of polyneutrons. So depending on circumstances the ratio of helium to energy can vary quite a lot from experiment to experiment." The meaning of the term "poison," in this context, is explained in the next section.

5) Poisoning processes:

Suppose that the above described chain process develops spontaneously somewhere in the atmosphere, or in the ocean. Then we would have a tremendous nuclear explosion on a planetary scale. Fortunately for us, this can not happen. Why

not? For the same reason for which a chain reaction in a power plant can not go on indefinitely, because the chain generates reaction products that act as poisons. In the context of a power plant "poisons" are fission products; they trap neutrons that would otherwise be available for the chain reaction. In the so-called "cold fusion" context poisons are structures composed of a polyneutron and a ^{16}O nucleus that are stuck together like tiny drops of oil and water, each retaining its identity, but held together by a reduction of surface energy over the area of contact. Poisons can form in reactions such as:

$$^{A}Nt + {}^{18}O \text{ --> } {}^{16}O\ {}^{B}Nt + {}^{C}Nt \text{ . . . . . } (B + C = A + 2) \text{.....(4)}$$

Which is just like reaction (3) except that one of the product neutrium atoms sticks to the ^{16}O. The $^{16}O\ {}^{B}Nt$ composite nuclei are massive oxygen isotopes and they are the poisons. They absorb polyneutrons that would otherwise be available for the chain reaction and remove them from the system:

$$^{A}Nt + {}^{16}O\ {}^{B}Nt \text{ --> } {}^{16}O^{(A+B)}Nt. \text{ .....(5)}$$

The ^{A}Nt and ^{B}Nt nuclei, merged into a single droplet $^{(A+B)}Nt$, are still stuck to the ^{16}O. The reaction is exothermic and the released energy consist of gamma rays.

These poisons rapidly accumulate and by capturing polyneutrons they extinguish chain reactions at the very early stages of development. They prevent catastrophic explosions in our environment. Note that only exothermic reactions are allowed to take place. The symbol $^{16}O^{A}Nt$ refers to what John calls a composite nucleus. It is an ordinary nucleus, in this case ^{16}O, that is stuck to a polyneutron in the same way that a drop of water and a drop of oil can be stuck together by a reduction of surface energy over the area of contact. If A=300, for example, then the the composite nucleus (on the right side) can be said to be a superheavy isotope of oxygen; its atomic mass number is 316. It is a nuclear molecule. After trapping another polyneutron, that merges with the polyneutron already there, the atomic mass of the composite may become 800, or 1300, depending on the size of the captured aggregate. Note that the composite nucleus is a positively charged particle; it is repelled by ordinary nuclei, such as ^{18}O, and also by other composite nuclei. Only polyneutrons are neutral.

As indicated above, composite nuclei are poisons; they soak up polyneutrons that would have otherwise been available to the chain reaction process. Poison-creating reactions (capturing more and more polyneutrons) are exothermic. As composites increase in number, via reaction (4) they soak up polyneutrons more and more rapidly. That is why chain reaction processes extinguish themselves, sooner or later.

In comparing chain reactions based on fission of uranium with chain processes based on reactions (1) and (3) John said that what takes several months in the first case (accumulation of fission products) takes only several milliseconds in the second case (accumulation and growth of composite nuclei). This, he said, is due to very large cross sections (high probabilities) of reactions involving polyneutrons.

6) Favorable conditions:

To sustain a chain reaction process in a small region of space, as described above, the fuel must be supplied and poisons must be removed. That is what constitutes favorable conditions, according to Fisher. He thinks that rapid stirring to remove poisons might be a mechanism necessary to sustain generation of nuclear energy through the chain process. This might happen, for example in the bubbling region in an electrolytic cell, near a water fall, in a geyser, along a mountain stream, etc.,. Favorable conditions can also be created when a flow of deuterium ions is imposed on a material structure, as in Iwamura experiments. More favorable conditions allow to sustain chain reaction at a high average rate. A useful cold fusion reactor would require a setup in which favorable conditions are sustained in sufficiently large regions of space.

At one point an analogy was introduced to further clarify the meaning of "favorable conditions." Consider a glowing ember, said John. It becomes dimmer and dimmer; it will extinguish itself if left alone. . But blow on it and the chemical reaction responsible for the glowing is intensified. Why does this happen? Because we are delivering oxygen, and removing poisons (carbon dioxide and ashes blocking the unconsumed charcoal.) In the same way a chain reaction based on ^{18}O fuel is intensified (reignited) when favorable conditions are reestablished. Without favorable conditions chain processes are practically inconsequential. The first sustained chain reaction, based on the ^{18}O, ^{7}Li, ^{6}Li and ^{2}H fuels, was accidentally discovered by Fleischmann and Pons, according to Fisher. It is very likely that such processes also happen in our natural environment, for example, in places where fluid agitation is present.

7) Radioactivity of polyneutrons:
John assumes that each neutron in a polyneutron can turn into a proton. This well known and relatively slow process, called beta decay, is associated with the emission of an electron and an antineutrino. After two consecutive beta decays a polyneutron is transformed into a nucleus containing two protons. This makes it possible to eject an alpha particle through a highly exothermic process. The atomic mass of the polyneutron is reduced by four units. The sequence of two consecutive beta decays followed by an alpha decay can then be repeated. This happens over and over again till the size of the polyneutron is reduced to nothing.

A similar process is postulated by John for large composite nuclei. The presence of the ordinary nucleus inside brings some complications but, provided the number of neutrons is large, the shrinking through a relatively slow sequence of beta-beta-alpha decays is possible. Sooner or later, when not too many neutrons are left, even the first beta decay becomes an endothermic process because of the charge on the oxygen member of the composite.

The only way to change the composition, after that stage is reached, consists of a very complicated and a very slow process, the coordinated simultaneous beta-beta decay and emission of an alpha particle.

$$^{16}O\ ^{A}Nt \rightarrow\ ^{16}O\ ^{(A-4)}Nt + {}^{4}He \quad .....(6)$$

The beta-beta-alpha decay half-life becomes very long, for example, 10 years, or so. Slowly decaying composite nuclei can migrate (in air or in water) over long distances before they disappear, or before favorable conditions are found to start a chain reaction.

8) Where do original polyneutrons come from?
A chain reaction based on neutrons must be started with neutrons that are produced in some other way. Likewise, polyneutrons must initially exist to trigger a chain process based on reactions (1) and (3). Where do the first polyneutrons come from? Consider a composite nucleus at the ending stage of its beta-beta-alpha sequence. It might be far away from where it was created (as a poison). Suppose this is a bubbling environment in which conditions are favorable to start a chain process. The only thing needed is a single polyneutron. The decaying composite nucleus is not a polyneutron but it can occasionally emit a polyneutron. This might happen, according to John's theory, through a rare process related to reaction (6) including the emission of an alpha particle.

$$^{16}O^{A}Nt \rightarrow\ ^{12}C^{(A-B)}Nt + {}^{4}He + {}^{B}Nt \quad .....(7)$$

If this polyneutron is emitted in a place where conditions are favorable it will start a new chain reaction.

9) How are the reported experimental facts explained by Fisher's theory?
a) Excess heat, reported by many cold fusion experimentalists, according to Fisher's theory, is mostly generated thorough reactions like (1) and (3), often with ^{7}Li, ^{6}Li and ^{2}H fuels. The outcomes will become reproducible only when we learn how to control favorable conditions.

b) Production of tritium, reported by many cold fusion experimentalists, is, according to Fisher's theory, generated thorough the reaction (2).

c) Production of helium, reported by many cold fusion experimentalists, might, according to Fisher's theory, occur is several ways. Reaction (1') is only one of them. Sequential beta-beta-alpha decays, taking place in the region in which the chain process is going on, is another mechanism for generating helium.

d) Showers of alpha particles, discovered in electrolytic cells by Oriani and Fisher, are consequences of rapid sequences of beta-beta-alpha decays of common composite nuclei.

e) Much larger showers, in the vapor over an electrolyte, also reported by Oriani and Fisher, can be explained as chain reactions supported by reactions (1) and (3), triggered by a polyneutron from the decay of a long-lived composite via reaction (7). Such chains can generate over 100,000 alphas before they are quenched by buildup of composite nuclei in the quiescent vapor.

f) Another way to generate alpha particles, for example in an electrolytic cell containing ordinary lithium, is the following exothermic sequence:

$$^{A}Nt + {}^{7}Li \rightarrow {}^{(A+2)}Nt + {}^{5}Li \quad \text{.....(8)}$$
$$^{5}Li \rightarrow {}^{4}He + {}^{1}H \quad \text{..... (8b)}$$

g) Nuclear transmutations are, according to Fisher's theory, produced through collisions of polyneutrons with ordinary nuclei. The above sequence transmutes lithium into helium and reaction (1) transmutes ^{18}O into ^{16}O. Changes in isotopic compositions of numerous elements were reported by many experimentalists.

I suppose that an appropriate exothermic nuclear reaction, or a sequence of several such reactions, can be found to explain appearance of particular products. Polyneutrons are not repelled by ordinary nuclei and reactions must often be characterized by large cross sections.

h) How would an alchemist, familiar with properties of neutrium, plan to make gold, ^{197}Au, from a common isotope of mercury, ^{202}Hg? The first step would be to realize that the mercury isotope, ^{197}Hg, turns into gold through the well known process of electron capture. This has nothing to do with polyneutrons. The challenging part would be to transmute ^{202}Hg into ^{197}Hg. That is where the use of polyneutrons would probably be suggested, according to the following reaction:

$$^{A}Nt + {}^{202}Hg \rightarrow {}^{(A-5)}Nt + {}^{197}Hg \quad \text{..... (9)}$$

Once produced, the ^{197}Hg will certainly turn to gold. But is the reaction (9) possible? The answer depends on its Q value. According to (2) the mass excesses of the ^{197}Hg and ^{202}Hg are: -30.436 MeV and -27.356 MeV, respectively. Using these numbers one finds that the Q value of (9) is +12.6 MeV. The positive sign indicates that the reaction is energetically possible. A conventional nuclear scientist, who does not believe in polyneutrons, would probably suggest to produce ^{197}Hg from ^{196}Hg with ordinary neutrons. This way of manufacturing gold from mercury is indisputably feasible but too expensive. It is based on conventional nuclear science.

10) Post scriptum:

Fisher's presentation at the conference in Marseilles (5) had no equations. Puzzled by its content I asked John for a private lecture. He agreed. That recorded lecture was attended by three other conference participants: Russ George, William Collis (who said that he learned about Fisher's polyneutron model ten years ago) and by Bielobrzeckaja-Costalariza-Nikolajeva. Most of what I wrote here is my understanding of that lecture, and of the references (4,5). I am

a retired teacher who likes pedagogical challenges. Learning the model and describing it was a real challenge. The content of this essay will be modified to reflect evolution of my understanding.

Appendix 1:
Energy in nuclear reactions.

All nuclear transformations, both reactions and radioactive decays, have an energy aspect. A nuclear reaction, in general, can be described symbolically as:

$$L + K \rightarrow R + S$$

where L and K are isotopes called reactants while R and S are isotopes called products. The number of participants does not have to be limited to two on each side. A transformation with only one term on the left side is called radioactive decay. Similarly, a process with two terms on the left side but only one term on the right side is called fusion. Electrons and photons are not reaction participants in this simplified description. The energy aspect of a nuclear transformation is characterized by the so-called Q value of the process. By definition, the Q value is the difference between the sum of the masses on the left side and sum of the masses on the right side, multiplied by the square of the speed of light, c.

$$Q = [(\text{sum}(L+K) - \text{sum}(R+S)]*c^2$$

A nuclear process is said to be exothermic (releasing energy) when Q is positive; it is said to be endothermic (using energy) when Q is negative. Exact masses of known nuclear isotopes have been determined with great accuracy; their values can be found in most nuclear science textbooks (2, 3). Note that Q is positive when the sum of the masses on the left is larger than the sum of the masses on the right. One way to describe this, no longer popular, is to say that Q is positive when the mass is reduced in a process and Q is negative when the mass is increased in it. The term inside the square brackets, often represented by Dm, is called the reaction mass difference. Using this term the equation (10) can be written $Q=Dm*c^2$. Note that Q is in joules when m is in kilograms and c is in meters per second. In nuclear physics, however, Q is usually expressed in MeV and Dm in so called atomic mass units, u. Using the well known conversion factors associated with these practical units one finds that the value c^2 must be equal to 931.48 MeV/u. Thus Q=9.31 MeV when Dm is 0.01 u, and vice versa. Exact masses in reference (2), given in u, can also be expressed in the units of energy. The mass of ^{58}Ni, for example, 57.935346 u, can be expressed as 53965.616 MeV.

Reference (3), on the other hand, does not give exact masses for each isotope. What it gives instead is called the mass excess. By definition, the mass excess for an isotope (not to be confused with Dm of a reaction) is simply a difference between the exact mass of that isotope, M, and the nearest integer I. It is usually expressed in the energy units, MeV. The mass excess for ^{58}Ni, for example, is -60.224 MeV (3). That value can be used to calculate the exact mass, M, if needed. To illustrate this let me write:

$$(M - I)*931.48 = -60.224$$

where I is 58. This gives; M=57.93534 u found in (2). It turns out that Q values of nuclear transformations can be calculated from the defining equation (10) by using mass excesses instead of exact masses. To illustrate this consider the following reaction:

$$^{1}n + ^{27}Al \rightarrow ^{4}He + ^{24}Na$$

where ^{1}n represents a neutron. The excess masses on the left, according to (3), are +8.071 MeV for ^{1}n and -17.194 MeV for ^{27}Al. Likewise, excess masses on the right are: +2.425 and -8.418 MeV, respectively. This gives -9.123 MeV for the sum of the left and -5.993 MeV for the sum on the right. The Q value of the reaction, is the difference between the sum on the left and the sum on the right; in this case it is equal to -3.13 MeV. Negative Q indicates that the reaction is endothermic. That reaction becomes possible only if at least 3.13 MeV of energy is delivered, for example, in the form of kinetic energy of the neutron.

Appendix 2
Excess masses of polyneutrons etc.

The value of k:

The mass excess of a polyneutron composed of A neutrons, as explained in the main part of this essay, is $\Delta(^{A}Nt) = k*A$. The purpose of this appendix is to show how the Q values are calculated for the reactions involving polyneutrons. Let us see how the numerical value of k 1.90 MeV/u) was established by John Fisher. If the reaction (1) is exothermic, as assumed by Fisher, then its Q must be larger than zero. This implies that the sum of the excess masses on the left side of the equation (1) must be larger than the sum of the excess masses on the right side. Or, symbolically,

$$[D \text{ of } ^{18}O] + [D \text{ of } ^{A}Nt] > [D \text{ of } ^{16}O] + [D \text{ of } ^{(A+2)}Nt] \ldots\ldots\ldots\ldots (10)$$

The excess masses of ^{18}O and ^{16}O are known (3); they are -0.783 and -4.737 MeV, respectively. Furthermore, [D of $^{(A+2)}Nt$] is only 2*k larger than [D of ^{A}Nt] Therefore:

$$-0.783 + k*A > -4.737 + (k*A + 2*k)$$

This reduces to the inequality $k < 1.977$. The upper limit of k is established by John on the basis of reaction (2). Assuming that the Q value of that reaction is positive (otherwise the reaction would not occur spontaneously) one has:

$$[D \text{ of } ^{2}H] + [D \text{ of } ^{A}Nt] > [D \text{ of } ^{3}H] + [D \text{ of } ^{(A-1)}Nt] \ldots\ldots\ldots\ldots (11)$$

The excess masses of ^{2}H and ^{3}H are known (3); they are 13.13 and 14.95 MeV, respectively. Therefore,

$$13.13 + k*A > 14.95 + k*(A-1)$$

The k*A cancels, because it appears on both sides of the inequality, and one has; $k<1.82$ MeV/u. In other words, positive Q values of the reactions (1) and (2) implies that k must be confined to a narrow range of values:

$$1.82 < k < 1.98$$

The value used by John, 1.90, is in the middle of that range. Knowing k we can calculate the Q value of any nuclear reaction involving polyneutrons. This in turn, allows us to predict which reactions can occur spontaneously and which can not.

Only reactions whose Q values are positive can take place spontaneously. Reliability of such predictions, however, are only as good as the reliability of Q values. That is where a difference between ordinary nuclear reactions and reactions involving polyneutrons become obvious. Exact masses of ordinary nuclei have been measured before the theoretical model (for calculating them) was introduced in 1930's. The model has several numerical coefficients; their values were chosen to match the experimentally measured masses. Polyneutrons, on the other hand, are hypothetical particles whose exact masses are unknown outside the theoretical model. What would the range of k be if another pair of hypothetical reactions, instead of (1) and (2), were chosen? How to decide that one choice of possible defining reactions is better than another? I suppose that such questions are premature at this preliminary stage of development. It is a miracle that a pair of reactions narrowing the range of possible values of k to plus or minus 0.4% was found by John.

Fisher's theory, described in the main text of this essay, introduces molecule-like structures called composites. The $^{16}O^{B}Nt$, appearing in the reactions (4) and (5), is a typical example. The excess mass of such loosely bound structure is assumed to be the sum of the excess masses of the ordinary nucleus, such as ^{16}O, and of the excess mass of the polyneutron, such as ^{B}Nt. What is the Q value of the reaction (3)? The first step, as always, is to write down the defining equation:

$$Q = \text{sum (of all D on the left side)} - \text{sum (of all D on the right side)}$$

or, more specifically,

$$Q = [\ D \text{ of } {}^{A}Nt + D \text{ of } {}^{18}O] - [D \text{ of } {}^{16}O + D \text{ of } {}^{B}Nt + D \text{ of } {}^{C}Nt]$$

Note that to balance the numbers of nucleons the (A+18), on the left, must be equal to (16+B+C) on the right. In other words, A+2 must be equal to B+C. The excess masses of ^{18}O and ^{16}O, according to the reference (3) are: -0.783 MeV and -4.737 MeV, respectively. This gives

$$Q = [k*A + (-0.783)] - [(-4.737) - k*B - k*C]$$

or

$$Q = 3.254 + k*(A - B - C) = 3.254 + 2*k = 7.054 \text{ MeV}$$

The reaction is exothermic. In the same way one can show that the Q value of the reaction (4) is equal to 4.454 MeV. Answering one of my e-mail questions John wrote:

"The excess masses you are using for ^{1}Nt, ^{4}He, and ^{16}O are correct. They are respectively +8.071 MeV, +2.425 MeV and -4.737 MeV. The mass excess of ^{18}O is -0.782 MeV. My best estimate of the mass excess of polyneutron with A neutrons is 1.90*A MeV. This makes the mass excess of ^{16}O stuck to a polyneutron with 100 neutrons, $^{16}O^{100}Nt$, equal to -4.737 + 190 -b where b is the binding energy of the oxygen to the polyneutron drop. I don't know what b is, but I assume it is small. In any event it does not matter for most reactions. Consider for example the polyneutron growth reaction (1):

$$^{100}Nt + {}^{18}O \rightarrow {}^{102}Nt + {}^{16}O$$

wherein the polyneutron grows by two neutrons. The energy generated in this reaction is:

$$(190 - 0.78 - b) - (193.8 - 4.74 - b) = -3.8 - 0.782 + 4.737 = 0.16 \text{ MeV}.$$

so the polyneutron growth reaction is exothermic and ^{18}O is capable of supporting a chain reaction. Note that the b's cancel out. Now consider the decay of the composite, $^{A}Nt\ ^{16}O$, where a polyneutron and ^{16}O are stuck together. The composite can decay by simultaneous double beta decay and alpha decay in a reaction with the overall formula:

$$^{A}Nt^{16}O \rightarrow {}^{(A-4)}Nt^{16}O + {}^{4}He$$

Here the energy generated is:

$$Q = [1.90*A - 4.74 - b] - [1.90*(A - 4) - 4.74 - b] - 2.42 = 5.18 \text{ MeV}.$$

Keep your questions coming. I will answer them all if I can, on the first try if possible."

Appendix 3
Other chain processes?.

1) Tables of excess masses are often shown in nuclear physics and nuclear chemistry textbooks. Here are some of the excess masses from the reference (3). They can be used to calculate Q values of reactions mentioned in my essay.

Atom Δ(of atom) in MeV
==========

^{1}Nt (neutron) 8.071
^{1}H 7.289
^{2}H 13.126
^{3}H 14.950
^{4}He 2.425
^{5}Li 11.680 (According to J. Fisher; ^{5}Li is not listed in reference (3))
^{6}Li 14.087
^{7}Li 14.908
^{12}C 0.00000
$^{16}0$ - 4.737
^{18}O - 0.783
$_{27}Al$ - 17.194
^{58}Ni - 60.224
^{93} Kr - 65.6

^{133}Cs - 88.089
^{142}Ba - 77.82
^{197}Hg - 30.735
^{197}Au -31.567
^{202}Hg - 27.356
^{235}U 50.572
^{238}U 47.307
^{239}Pu 48.585
^{A}Nt (large A) 1.90*A

To practice with calculations of Q values the reader is invited to verify that the two reactions shown below are endothermic (negative Q).

$$^{A}Nt + {}^{7}Li \rightarrow {}^{5}Li + {}^{(A+2)}Nt \quad \text{.....(12)}$$
$$^{A}Nt + {}^{7}Li \rightarrow {}^{6}Li + {}^{(A+1)}Nt \quad \text{.....(13)}$$

Show that their Q values are -1.80 MeV and -0.57 MeV, respectively. The reaction (14), however, is exothermic; its Q value is 9.32 MeV. That indicates that polyneutrons can grow by interacting with deuterium, for example, in heavy water.

$$^{A}Nt + {}^{2}H \rightarrow {}^{(A+2)}Nt \quad \text{.....(14)}$$
$$^{A}Nt + {}^{2}H \rightarrow {}^{B}Nt + {}^{C}Nt \quad \text{..... (15)}$$

In other words, heavy hydrogen (^{2}H), like heavy oxygen (^{18}O), can be a fuel in the chain process. A sequence of reactions (14) followed by the reaction (15) constitutes a single step in the energy generating chain process. This is similar to the process based on the reactions (1) and (3).

2) Suppose an electrolytic cell is used to create “favorable condition” for the chain process based on either reactions (1) and (3). or reactions (14) and (15). Excess heat is produced coninuously because the exothermic chain reaction is going on. Fresh fuel, ^{18}O or ^{2}H, is supplied and poisons are removed, as envisioned by Fisher. Suddenly one gram of ^{238}U is introduced into the electrolyte, for example, in the form the powdered uranium nitrate salt. I am speculating about production of ^{235}U from ^{238}U via the following reaction:

$$^{238}U + {}^{100}Nt \rightarrow {}^{235}U + {}^{103}Nt$$

Is this possible? No, it is not possible, because Q is equal to -8.95 MeV. The reaction is endothermic; it will not occur spontaneously, fortunately for our safety.

3) Then I envision a well known precess -- spontaneous fission of ^{238}U according to the following reaction:

$$^{238}U \rightarrow {}^{142}Ba + {}^{93}Kr + 3*({}^{1}n) \quad \text{.....(16)}$$

Uranium decays by emitting two fission fragmens (barium and krypton) and three free neutrons. The above fission products are known to be beta radioactive. Using the excess masses of participants one finds that Q=166 MeV. The process is strongly exothermic but the probabilty of its occurence is extremely low (due to the fission barrier of about 6 MeV). But can this process be speeded up by polyneutrons? I am imagining, for example:

$$^{100}Nt + ^{238}U \rightarrow ^{142}Ba + ^{93}Kr + ^{103}Nt \text{.....(17)}$$

The Q value of that reacion is 190.5 MeV. Conceptually it is similar to the reaction (1') where ^{7}Li was split into two fragments (^{4}He and ^{1}H) by interacting with a polyneutron. In both cases the polyneutron grows and energy is released. The released energy in the reaction (17), however, is much larger than in the reaction (1'). The reaction (17) implies that the fission barrier of the ^{238}U is somehow overcome in the presence of the ^{100}Nt. Note than the polyneutron of mass 103, escaping from the reaction, might interact with another ^{238}U, etc. Is it conceivable that one can have a fast chain reaction in which ^{238}U is generating excess heat? I do not know how to answer this question.

Addendum (5/27/05):
A revised version of the polynutrons theory, as presented iby John Fisher in Siena (May 2005), is described in a paper that can be downloaded from his website:

http://www.markfisher.net/johnfisher/papers/neutron_isotopes_slides.pdf

My comments about the new version can be seen in unit #226.

Inserted on on 7/14/2009:
A recent qoute of what John Fisher wrote can be seen at the beginning of Unit 364. And here is another message that John posted on CMNS list: Responding to a question about polyneutrons, he wrote: "I have returned from vacation after several weeks and now can answer your question regarding the sticking force between neutrons in a polyneutron.

First note that two neutrons are almost bound into a di-neutron. They form a resonance with energy about 0.12 MeV over a potential well about 35 MeV deep. (For references see Treatise on Heavy-Ion Science, Vol. 8, Nuclei Far From Stability, Edited by D. Allan Bromley, Plenum Press, 1989, p 313 for 0.12 MeV resonance, and also Kenneth S. Krane, Introductory Nuclear Physics, John Wiley & Sons, 1988, p 82 for 35 MeV potential well.) We see that two neutrons are very strongly attracted to each other, but not quite strongly enough to be bound.

Consider now a large cluster of neutrons having density comparable to that of ordinary nuclei. Except for surface neutrons, each neutron is surrounded by 12 other neutrons with which it has nearest-neighbor interaction. This implies a 12-fold increase in the potential well that it sees and assures that it is strongly bound to the cluster. Neutral pions are the particles to which most of the attractive force can be attributed. Although the binding energy cannot be computed from first principles (it cannot be computed for the neutron-proton clusters of ordinary nuclei either) it can be approximated by a liquid drop model with a negative volumetric energy term and a positive surface energy term. The strengths of these terms can be determined from experiment (LENR experiments) just as they have been determined by ordinary experiments for ordinary nuclei.
From LENR experiments I have been able to determine an approximate value for the volumetric binding energy term, and find it to be about half of that for ordinary nuclei."

References:
1) F.M. Marques et al., "Detection of neutron clusters," Physical Review C, vol 65,044006, 2002

2) K.S Krane, "Introductory Nuclear Physics," John Wiley & Sons, Inc., New York, 1987.

3) G. Friedlander, J.W. Kennedy, E. S. Macias and J. M. Miller, "Nuclear

and Radiochemistry" third edition; John Wiley & Sons, Inc., New York, 1981.

4) J.C. Fisher, "Theory of low-temperature particle showers," This paper was presented at the 10th International Conference on Cold Fusion, August 2003. It can be downloaded from the library at <www.lenr-canr.org>.

5) J.C. Fisher, "Neutron isotope reactions," This paper was presented at the 11th International Conference on Cold Fusion, November 2003. It will be published in the proceedings of that conference.

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192) Oriani solid effect at my lab

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One can consider this to be a continuation of my unit #188. It is an open electronic logbook to tell what I am doing in trying to observe Oriani-solid effect for the third time. Part of the experiment is to be performed at my home (electrolysis) and part at Montclair State University (where the microscope is located). Here is what I have done so far.

1) CR-39 detectors were put in contact with everything to be used (for 7 days) in the cell: (Ni foil, glass tube, O-ring, powdered Li2SO4, water and platinum.) I will etch these detectors tomorrow . But will not waste time on counting tracks until this becomes important.

2) The cell was assembled (as illustrated in Figure 1 of the unit #188) and I used water to make sure there is no leak. The Pt wire (0.5 mm diameter is rigid enough to be used without a stopper. About 13 cm of the wire was enough to make a horizontal spiral and the vertical lead with a hook). The hook goes over the rim of the upper tube; that keeps the cathode in a fixed position. The horizontal spiral, anode. is 13 mm above the nickel foil (cathode). The amount of electrolyte above the cathode is about 4 cm.

3) The experiment started at 23:30 today (December 1, 2004). To get the current of 0.10 A I had to raise the potential difference across the cell to 7.5 V. That is how the cell is running now. At the very beginning I checked how the current depends on the voltage; the cell needs 16.5 V to have the current of 0.30 A. The cell does not behave as a linear resistor. I noticed that it is not very stable. Each time I return to the same current I need a slightly different voltage. I guess this has to do with the fluctuating number of bubbles. The electrolyte of milky, as it was in Minneapolis.

4) Four CR-39 detectors were positioned: one below the cathode and three along the outer walls of the upper tube. Oriani told me that he often detected particles in detectors facing the tube. Note that the upper tube is open and that the detector placed below the Ni cathode (in the air) is only 1.5 mm away. In that way the geometry of exposing the cathode to CR-39 will be essentially the same during the electrolysis and after the electrolysis.

5) More tomorrow; it has been a busy day (the day on which the DOE report was finally released).

December 2, 2004

6) It is hard to sleep when a decisive experiment is running. The voltage needed to keep the cell current at the 0.10 A level is now very steady; it is 7.0 V. It was 7.5 V when I went to sleep. Also bubbling is much less intense. I suppose that the bubbles seen during the beginning of the electrolysis were mostly air. (P.S. some water evaporated. I ˇrestored the level of electrolyte by adding about ten drops of water. This was done with a pipette while the cell was running. In this way the concentration of the electrolyte was restored. The voltage needed for 0.1 A is again close to 7.5 V. The volume of the electrolyte in my cell is 9 cc; most of it is in the column above the anode.)

7) I think that I should place a fresh CR-39 chip below the cathode when I double the current. I can easily do this without turning the current off. Why not? If particles I expect to detect are as numerous as they were in Minnesota then my track density after 24 hours is much higher, because my detector is nearly in contact with the cathode. And I might see particles that would be filtered by 1.5 cm of air.

8) The volts-amps relation for my cell (Ni cathode and Pt anode separated by 13 mm) is not linear. Why? The electrolyte was made by dissolving 2.36 grams of Li2OH4 in 100 cc of H2O. Does it imply avalanching of ions? Keep in mind that this open electronic logbook Kis for those who are interested in details and, perhaps, performing the experiment at the same time. I hope some of them will answer my question (by sending a message to me at , or comment on what I am doing.

9) Richard Oriani who I asked about the voltage needed by cell seems to be larger than that needed by his cell, wrote: “A voltage of 7.5 volts is perfectly acceptable. You must remember that most of the voltage is used to overcome the resistance in the electrolyte. Apply whatever voltage is necessary to get the current that you want.” But then he added. “I hope that you etched and counted preexisting tracks before the electrolysis.” No I did not do this; I have a control chip, several meters away from the cell. It will give me the background to subtract. Oriani’s approach is essential when one d Ieals with situations in which the signal is only several times larger than the noise (background). The success for me, in this first experiment would be a signal similar to that we had from two experiments

in Minnesota. Repeatable strong effect is needed to convince skeptics. Yes, I know that even this is likely to be “not enough.” What motivates me, at this stage, is confidence in scientific ways (as opposed to bureaucratic ways) of solving controversies in natural sciences.

10) It would be so much easier to work with an electronic (surface barrier) detector. Instead of waiting days I would know what happens each hour. I hope that somebody with that instrument, after reading this logbook, will construct a similar cell to study Oriani-solid effect. Please send them to me and I will be happy to post them in a dedicated unit.

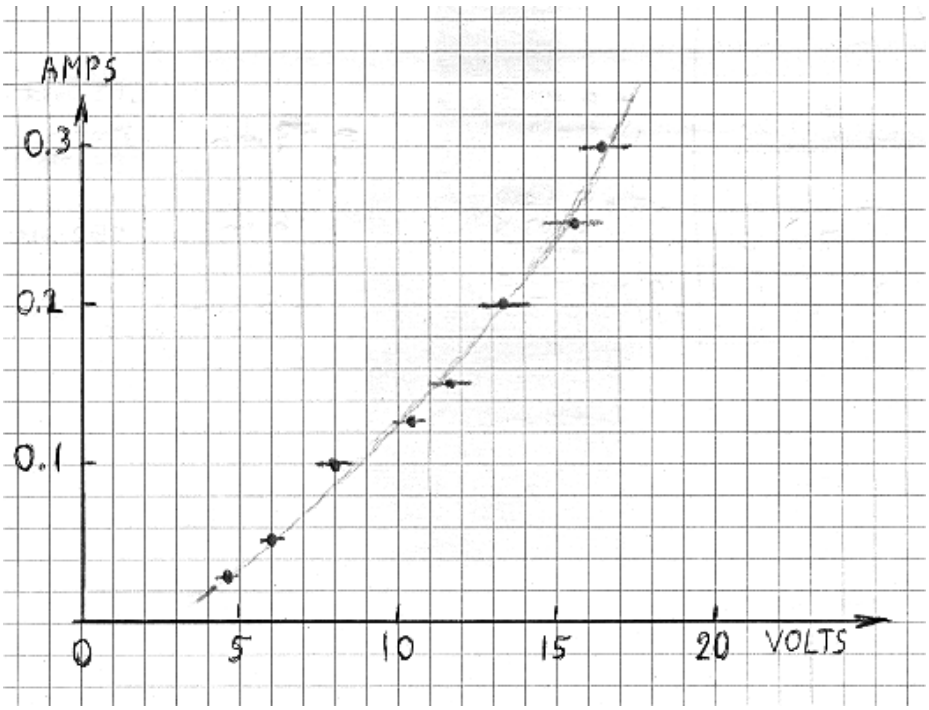

Figure 1

11) Pictures showing my cell. Figure 2 is the operating cell. Note the three CR-39 detectors (two very small and one much larger) facing the upper tube. The detector below the cathode is not visible, only its pedestal. The screws pressing the flange joints toward each other are also the legs on which my open cell is standing. Figure 3 shows the cell and its spiral platinum wire (before the unused part of it was cut). The HCl (1 N water solution) was used to wash platinum (for 24 hrs) to remove possible dirt introduced by my fingers (in making the spiral cathode). That what Oriani does.

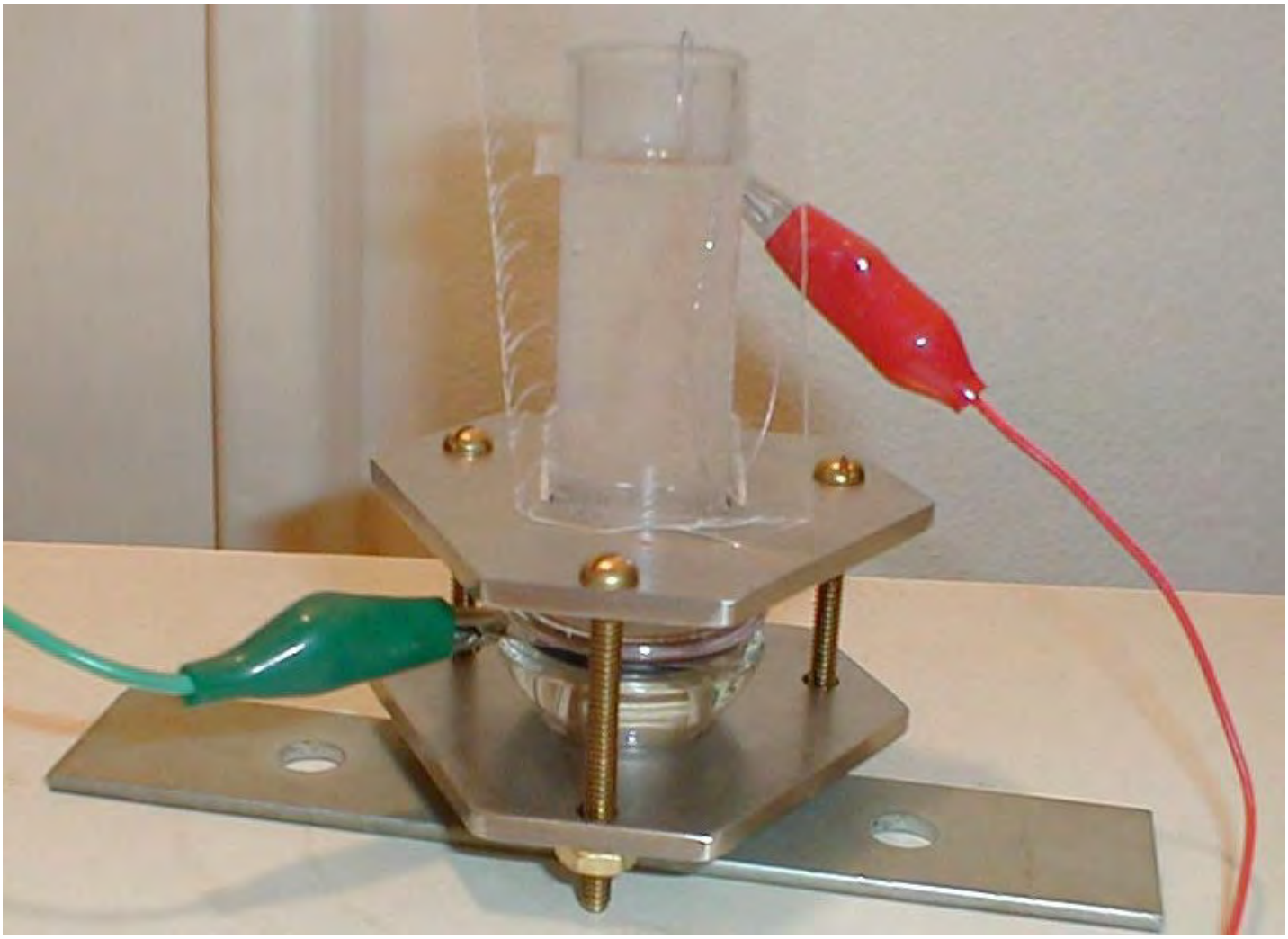

Figure 2

12) The voltage needed to keep the current constant changes from time to time. Sometimes it goes up or down by 10% or so. I suppose this has to do with changes occuring at the surface of the cathode. To keep the concentration of the electrolyte constant I add drops water, every several hours or so.

13) In the spirit of trying to reproduce the November 16 experiment in Minneapolis I decided not to change CR-39 detectors when I change currents, first to 0.20 A (tonight) and then to 0.30 A (tomorrow night). The third detector will remain in place for 24 hours (when I will be 0.3 A). Then the current will be cut off and the fourth detector will be used for about 24 hrs. This will be the first post-electrolysis exposure. It will be followed by the second and third exposures, for example, 24 hours each. Stay with me to share the excitment. I do not know if that excitment will come from a confirmation of the two effects or from the failure to confirm. The effects are: (a) emission of nuclear particles from the cathode during the electrolysis and (b) emission of such particles after the electrolysis.

14) An interesting observation. In watching the cell and the ammeter for about 15 minutes I noticed repetitive oscillations of the current. The current remains constant for about 3 minutes. During that time tiny bubbles are rising along the upper tube, like smoke in air. This must be oxygen and hydrogen. Then, for about one minute, the current starts decreasing monotonically (from 0.10 A to about 0.6 A). The tiny bubbles are rising as before. At one moment, however, I see a big bubble of gas (perhaps one cc) escaping through the tube. It is probably created below the cathode. Once this big bubble is out the current returns to 0.10 A and the cycle repeats itself. I suppose that the drop of current reflects the growth of the big bubble in the electrolyte between the cathode and the anode. (it changes the resistance). But can big bubbles be explained? I have no idea. If they are mixtures of hydrogen and oxygen then they are probably dangerous.

15) That is not what I observed after changing the current from 0.10 to 0.20 A (at 20:30 after using the current of 0.1 A for 21 hours). The intensity of tiny bobbles did increase, as expected, but big bubbles were popping up again, very regularly. This time I measured durations. After remaining constant for 43 seconds (I=0.20 A) the current started to go down to 0.12 A for 35 seconds. Then it jumped

back to 0.20 A and the cycle repeated itself. Sudden returning to 0.20 A always coincided with big bubbles. Perhaps the shape of the anode could be designed to prevent this. On the other hand, the big bubbles may be growing on scratches Oriani said might play a role. In any case I observe a dynamic process that may somehow be responsible for a nuclear phenomenon. After all, some well known phenomena depend on the rate of change of current rsther than on the current itself. Yes, I know, such speculations are more poetic than scientific. I do not have a theory to guide me; this is a facts-gathering experiment. The task is to confirm, or to cotradict, a clame made by Oriani. The claim is that an electrochemical process induces emission of nuclear particles.

16) Aha; by watching at a proper angle (through the conical part of the flange joint) and after optimizing the illumination, I was able to observe growing of large bubbles. This happens below the spiral anode, at a point there the cathode and the inner surface of the tube touch each other. Just before escaping the layer of gas covers abour 1/4 of the surface of the anode. I guess the diameter of my spiral anode should have been slightly smaller, and spaces beteen the widings a little larger. Then there would be no current fluctuations due to big bubbles.I guess the diameter of my spiral anode should have been slightly smaller. Then there would be no bubbles, I think. The temperature of the electrolyte, at 0.20 A, is 28 degrees C (measured by inserting a mercury thermometer for about two minutes).

December 3, 2004

17) So far the current was 0.10 A during the first 21 hours and 0.20 A during the next 24 hours. I just changed it to 0.30 A. The big bubbles now come every 40 seconds; During that time the current changes from 0.30 A to 0.1.80 A. My average current is thus close to 0.24 A but I will call 0.30. In the same way the so-called 0.20 A was actually a little bit smaller.

18) Before changing the current I removed one of the two small CR-39 detectors that were positioned next to the glass tube in air. A fresh Cr-39 detector was placed into the same position. I want to know if the higher current produces more tracks per hour than the lower currents. I still have two detectors that will be been exposed to glass for the entire time of electrolysis. And the detector under the nickel cathode will be replaced only after the current is turned off.

19) It is clear that the water level goes down more rapidly than before. Water escapes after being decomposed into oxygen and hydrogen; its temperature is now 49 C. It takes about three hours to lower the level of water by one centimeter. I will have to add water more often now (to keep the concentration more or less stable), for example every two or three hours.

20) What is the etching time? I already know, form experience, that the NaOH, whose temperature and concentration are 65 C and 6.5 N, etches very well in about 6 hours. Etching times that are too short produce tracks that are too small (hard to distinguish from dirt). Too long etching times might destroy shallow tracks (by taking away the layer in which they reside). To decide the optimum etching time (CR-39 from two different suppliers) I irradiated small detectors with alpha particles and etched them for times of 2, 4, 7, 9.3, 12.5 and 14.5 hours. A similar task, for CR-39 from the third supplier, was assigned to a student working with me on something else (radon detection project). One CR-39 chip, from the material she is using, was etched for 5.5 hours.

I will examine all these chips tomorrow. But how do I know that what is best for alpha particles (my Am-241 source was removed from an ionization fire alarm) is also best for the unknown particles coming out of the cathode? That is the dilemma. Without knowing what these particles are one has to be lucky to make the best compromise. It is possible that the numbers of tracks counted by Oriani were due to a small fraction of particles that were actually intercepted by the detector. Some of his observations (not yet published) are consistent with this. That is why I decided to position my detector only 1 mm from the cathode (rather than 15 mm as we had in Minneapolis) two weeks ago. No harm can result from bringing the detector closer.

December 4, 2004

21) Today I measured diameters of pits (tracks) made in CR-39 by alpha particles emitted from my Am-241. Etching times were from 2 hours (the shortest) to 14.5 hours (the longest). I used the NaOH of 6.5 N and 65 C; the chips were from Landauer. (see item #184). Tracks become visible after 2 hours, but their diameters were too small (about one micron). Such tracks are hard to recognize because other small dots (dirt, and surface deffects) are also present. At four hours the track sizes are close to 3 microns. Then their sizes grow linearly with up to about 14 microns for the etching time of 14.5 hours. I would not be surprised to see a different curve for 2 MeV because their tracks are shallower. I will etch my detectors for ten hours; this produces pits which are easy to recognize, even when the surface of the detector has scratches and dirt. Cr-39 from two other suppliers (see item #184) produced essentially the same track diameters as those from Landauer after the etching time of 5.5 hours.

My advise to those who want to study CANA phenomena (Chemically Assisted Nuclear Activity) with CR-39 detectors should always produce a chip irradiated by alpha particles. That chip should be etched together with other experimental detectors. In that way one can always be sure that etching conditions were satisfactory. Trivial mistakes, such as wrong concentration of NaOH, or using a plastic chip that is not CR-39, would be immediately recognized.

22) I changed my mind, after writing the above. I will etch my chips twice, first for 6 hours and then for another 6 hours. This will give me a chance to examine the chips twice; I do not want to miss a chance of seeing tracks that might disappear after 10 hours of etching.

23) I will end the electrolysis tonight. For some reason the current is no longer oscillating; the big bubbles stopped popping up. perhaps the room temperature, or pressure, is not the same as yesterday. Yes, I know that a pathological skeptic might say: “Your experimental results can not be taken seriously because you are not controlling all parameters.” But s/he would say this only if a lot of nuclear particles were detected; the failure to confirm Oriani’s discovery would be applauded. Why wasn’t scientific methodology used by scientists appointed by the DOE to reevaluate cold fusion claims? Instead of trying to replicate selected experiments, such as generation of helium or emission of protons, they asked for written reports. Then they wrote what they think about the reports. I would prefer them to be investigators, nor jurors.

Scientific disputes should be addressed scientifically. The Oriani effect I am investigating (with nickel and ordinary water) can not be “cold fusion.” I never met a scientist who thinks that “cold fusion,” a reaction in which two isolated deuterium ions fuse at ordinary temperatures, is possible. Was the effect Oriani effect examined by the DOE jury? What did the jurors know about it? One day before the DOE review report was published -- what a coincidence -- I sent an e-mail massage to the DOE investigators (via Dr. Decker). It was a reference to my already-posted unit #188. This was too late to influence the outcome of the DOE report. But was my message delivered to them? I do not know.

27) The current of 0.30 A has been flowing through the cell for nearly 25 hours; it will soon be cut off. Before doing this I want to describe what I am going to do next.

a) Removing the Pt anode and applying a detector to it.

b) Emptying the electrolyte into a small beaker and dropping a detector into it.

c) Removing the upper tube and applying four detectors to the nickel cathode, on the side that was wet during the electrolysis. Each of these four detectors has the area of one square centimeter, just enough to cover the entire foil (including the areas that were not in contact with the electrolyte).

d) Removing the detector that was below the nickel foil during the electrolysis and replacing it with a fresh detector. The counting geometry will be exactly the same as during the electrolysis. All fresh detectors will remain in place till tomorrow. After that they will be removed and replaced by fresh detectors. Then the removed detectors will all be etched and examined.

The name of the game is good labeling and good documentation. Preliminary results of observations will be posted here, probably not later than on Monday. Further plans will depend on these results. My wife said that since the effect is known to be only 60% reproducible, and since I was lucky to see it twice, then “it is about time” for a failure. She said this laughingly, of course.

28) All went as anticipated. The current during the electrolysis was 0.10 A during the first 21 hours, 0.2 during the next 24 hours and 0.30 in the remaining 25.5 hours. The end of the electrolysis was at 22:00 on 12/4/04. That will be called “zero time” for the rest of the experiment (in case particles are emitted after the end of electrolysis), as they were in Minneapolis.

December 5, 2004

29) Five Cr-39 chips are being etched now:

```
(A) was below the nickel cathode (1 mm away in air).
(B) was several meters away to measure the background.
(C) was in contact with glass (outside the tube) at all three currents.
(D) was in contact with glass (outside the tube) only at I =0.1 A and 0.2 A.
(E) was in contact with glass (outside the tube) only at I =0.3 A.
```

The first post-electrolysis accumulation of tracks (on chips listed below) will continue. For how long? It depends what I will see on the chip (A) after 6 hrs of etching (in about an hour). If I see a lot of tracks then I will stop the accumulation at once and will start another one with fresh detectors. Otherwise I will wait another day or two.

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(F1, F2, F3, F4) are in contact with the cathode (on the side that was wet).
(G) is in contact with the cathode (side that was dry during the electrolysis).
(H) is in contact with the platinum anode (on the surface that was facing the cathode).
(I) is in contact with the electorolyte (floating on top of in a beaker).
(J) is in contact with the inner wall of the upper tube.
(K) is outside to measure the background.
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30) I did stop etching after 6 hours and examined the chip (A). I did not see any obvious tracks. Therefore I decided to etch all five chips (from A to E, as above) for another six or seven hours. I would have to wait till tomorrow to know if the expected effect was confirmed in another attempt to replicate it. If it is not confirmed then I will repeat the experiment two or three more times. Failure to confirm several times could be an indication that the cell used in Minneapolis was somehow contaminated with an alpha radioactive substance. One does not need a lot of thorium, or uranium, to generate several alpha particles per hour. On the other hand, the idea of contamination seems to be conflict with the fact that Oriani often fails to observe the effects in his cell. A contaminated cell would always produce

excessive tracks, not only 60% of the time.

31) The following message from Oriani, received last night, might provide another explanation of my anticipated failure. Richard wrote;" . . . In view of our ignorance of what concentration is best for the nuclear effect it is probably wise to let the concentration vary over a large range by letting the electrolyte volume decrease a great deal before adding water to replenish the solution....." By adding water frequently (to keep the level of the electrolyte constant) I was not allowing the concentration to change. In other words, my first experiment at home might have missed the necessary concentration. Another explanation might be in water. Trying to follow Richard's footsteps I used tap water. But who said that water in New Jersey contains same impurities as water in Minnesota? The secret might be in the impurities.

I have to wait till tomorrow, and examine other chips, before recognizing failure to confirm the effect in this particular experiment. Well, I should not speculate too early. Perhaps I will see tracks tomorrow, or in the next experiment.

32) Yes, I do have to wait till tomorrow but I already know what to expect. After the end of the second etching, this afternoon, I examined two CR-39 chips: the one that was exposed to alpha particles from Am-241 and (A) that was below the nickel cathode during the electrolysis. I saw a lot of alpha tracks on the first chip, as usual, but only some tracks on (A). The (A) chip was etched for 6+6 =12 hours but the alpha particle chip was etched for the last 6 hours only. (Unfortunately, I forgot to use an alpha irradiated chip to show that etching was normal during the first 6 hours.). It will be a matter of counting tracks on the (A) and (B) chips. Is the track density on (A=signal) significantly larger than on (B=noise)? I will know tomorrow. The situation reminds me the analysis of the TiDx foil from Steven Jones; it is definitely not comparable with what was observed from the Pd613 or Pd616 cathodes sent to me by Dennis Letts.

Why didn't I see tracks on (A) after the first 6 hours of etching? Perhaps the temperature went down after it was initially setting it to 65 C. I was not adjusting it all the time, as during the second etching. To compensate for the possibly too low temperature the second etching was at performed at 68 V. Or perhaps an error was made in the preparation of NaOH (imposing a concentration that was lower than 6.5 N). I will never know because I forgot to etch an alpha irradiated chip.

December 6, 2004

33) Yes, today I have the bad news and the good news to share. But before doing this let me ask you for a favor. If you are among those who read this logbook from day to day -- I suppose there are such people -- then please send me an Emil note at: And I would very much like to hear from those who are trying to replicate Oriani solid effect, as I am. This experiment should not be performed by students without supervision. Chemicals are dangerous, especially hot and concentrated NaOH used for etching. I am sure that many teachers would be happy to supervise student experiments, if you ask.

Using the Internet to share an ongoing experiment, and receiving feedback (criticism, advise, comments), is something I did not do before. Yes, it does take some extra time. I want to know if this time is worth spending. That is why I want to hear from you. Please let me know what you think about the "lab over the Internet" idea. I think that a project in which several people perform the same experiment at the same time and communicate constantly (which did not materialize this time), is worth trying.

34) And now back to our project. Let me tell you what I did today.

a) **Sample (A)** --> 21 tracks (plus or minus 6) on the entire area of 0.7 cm^2.

b) **Sample (B)** --> 28 tracks (plus or minus 10) on the entire area of 0.5 cm^2.
In other words, the detector below the cathode registered nothing but the background. The effect observed in Minneapolis was not observed in Montclair. Perhaps the scratches I made on the nickel foil were not deep enough for the particles to come out. The "plus or minus," by the way, refers to border cases; it is not always easy to decide which dark spots are tracks and which are not. i call this a threshold of rejection error. That error can be reduced to nearly nothing by using an approach invented by Oriani. He etches the chips before the experiments and after the experiment. After the first etching he photographs detector's surfaces, field by field (through a microscope equipped with a digital camera). After the second etching he photographs the same fields. This gives him two pictures for each field. He compares them and counts only those tracks that were absent before the experiment. Such labor-intensive method is much better that my approach -- using one detector for the "signal" and another detector for the "noise."

Expecting a strong signal, similar to that seen in Minneapolis, I decided to follow a much less labor-intensive approach.

c) **Sample (C)** --> 34 tracks (plus or minus 10) on the entire area of 0.6 cm^2. In other words, one of the three CR-39 detectors that were in contact with the outer surface of the upper tube (during the electrolysis), registered nothing above the background.

d) **Sample (D)** --> 47 tracks (plus or minus 15) on the entire area of 0.6 cm^2. A small excess of tracks, with respect to the background is not significant. In other words, the second CR-39 detectors that were in contact with the outer surface of the upper tube (during the electrolysis, when the current was 0.10 and 0.20 A), registered nothing significant above the background.

e) **The big surprise** came when I turned to the third detector (E) that was also exposed to the outer surface of the upper tube. But it was exposed only when the current was 0.30 A, not during the entire electrolysis time, as detector (C). Here I at once noticed a large number of tracks. At first I thought it was a very high background because tracks were present not only on the surface facing the tube but also on the surface facing the air in the room. All other detectors were from Landauer; the (E) detector, received from Oriani, was from Italy (see the unit #185). Its thickness was 1 mm. Before using this new CR-39 material I irradiated another chip with alpha particles and etched it for 5.5 hours. The pit diameters were essentially the same as in the material from Landauer.

Fortunately, the background for the new material could be determined by examining the other surface of the irradiated field. I did count background tracks; they turned out to be much less numerous (about a factor ten or so) than on surfaces of the (E) detector.

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The average number of tracks (per field of observation) were:

11.1 for the surface of (E) that was facing the outer surface of the glass tube.
14.5 for the surface of (E) that facing the room air.
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The number of tracks on the first surface was 133 (12 typical fields) and the number of tracks on the second surface was 189 (13 typical fields). The work was done under low magnification of 40. The area of each field of observation, on the monitor connected to the digital camera, was 1.30 square millimeters. In other words, track densities were 850 and 1110 per square centimeter, per 25 hours (time during which the current was 0.30 A). That counting rate, about 40 per hour per square centimeter, is of the same order as reported, for a different setup, by Steven Jones (2).

35) The most surprising fact is the presence of tracks on two detector surfaces separated by one millimeter of plastic material. I think I know how this can be interpreted. Last year, in preparation for detection of protons from the TiDx foils, I often irradiated CR-39 chips by fast neutrons emitted by our Pu-Be source (no longer available). I always observed tracks on both surfaces, more on the second than on the first. Smaller tracks were interpreted as due to collisions between neutrons and protons; about two times larger tracks, much less common, were attributed to alpha particles from (n,a) reactions.

I think that tracks recorded in the (E) detector indicate emission of neutrons, from somewhere within the cell. But why were similar tracks not recorded in the (C) and (D) detectors? I do not know how to answer this question. One way to check if tracks are due to neutrons would be to etch the chip for a very long time (for example, till its thickness is reduced to 0.5 mm). Latent tracks due to neutrons must exist within the entire CR-39 volume. Therefore, new tracks would appear as the old tracks are etched away from exposed surfaces. In other words, visible tracks would be present, even after very long etching. Tracks due to a contamination of both surfaces, on the other hand, would disappear. I will perform this convincing test. Yes, I know, reading reports like this one is below the threshold of dignity of many scientists. The curse put on the entire field of CANA research will continue affecting attitudes toward it. By CANA I mean “chemically assisted nuclear activity.”

36) What Oriani and I have done, after the last conference, would be sufficient to publish a paper, and to apply for a research grant in any other field of natural science. But in the CANA field the only way to communicate with other scientists is the Internet. We know that most of them will not read our reports. And we will continue using our retirement money on research and traveling. That is not how serious research is conducted in other areas of science. Why should it be so? I expected the situation to change after the second DOE-sponsered investigation. But now I know that this will not happen very soon.

I wish I had access to a neutron detector. Another useful thing to do is to cover the entire tube with CR-39 detectors and then look for the distribution of tracks. This might help to localize the origin of neutrons. By the way, unlike the (C) detector, the (E) detector was on that side of the tube where the vertical part of the platinum wire was located inside the electrolyte. The wire was only 2 mm away from (E) but 20 mm away from (C). Can this be significant? But questions like that are for later. At present the most urgent task is convincing others that CANA phenomena are real. How can this be accomplished? I suppose that many researchers are now trying to answer this question. My answer is; “by finding one simple to perform experiment that is very reproducible and by offering it to students.” That what I am trying to do. The next step will be to etch the chips that have been applied to the cathode (and to other cell components) after the electrolysis. I will do this after two more days of exposure.

December 7, 2004

37) I decided not to rush with processing the post electrolysis CR-39 chips. Let them accumulate tracks (if any) for several more days. Being curious in not a good reason to start etching too early. I will accumulate for seven days.

38) In a message sent to me today Richard wrote:". . . Today I completed electrolysis, etching, and cursory examination of the chips. The one under the Ni cathode has plenty of tracks, and two of the chips in the vapor over the electrolyte also have goodly numbers of tracks. I have not yet done a careful examination....." That is good news. i was less lucky in the first experiment at home. Is it possible that the reason for my failure was too short distance between the cathode and the CR-39 below? My distance was 1 mm while his distance was 15 mm, as before. This does not make much sense if track-creating particles come from nickel; I would see more of them, not less. But what if these particles are produced in air, due to something happening in the cell? In that case I would see practically no tracks. Next time I should return to the original distance of 15 mm.

In any other field the above mentioned tentative conclusion would be highly indicative. But in the field in which reproducibility can not be taken for granted everything is fluid. Nothing is really indicative to me here unless observed several time. And after that, as in the case of Oriani, one has to face hostility on the part of those who control publications in scientific journals. What a shame.

December 9, 2004
39) Nothing new to report, except that I found another teacher, over the Internet, who might be interested in replications of Oriani solid effect. I know, from frequent postings on the discussion list for teachers, that he is a very knowledgeable experimentalist and an excellent communicator. I will be happy to introduce him here, when he approves it. Working in parallel with several teachers is highly desirable, especially when phenomena are not 100% reproducible. The goal will be to develop a setup for student-oriented explorations. And, who knows, we may be able to discover something important.

December 13, 2004
My attempt to "popularize" Oriani cells (among teachers and students) by writing an article to The Physics Teacher failed. This is illustrated by the message from the editor of that journal (see below). It was a reply to my informal inquiry (see text qouted by the editor.)

Dear Professor Kowalski:
Our editorial staff has examined the discussion given at the URL provided in your message. While the materials required to conduct the Oriani (c) experiment might be accessible to schools, we believe that too few of our readers would consider the exercise to be appropriate for inclusion in their introductory-level physics courses. Because of space constraints, we are now able to publish fewer than one-third of the submissions we receive. We must therefore take care to select those which we believe would be of the greatest benefit to our readers. While we would be willing to review a completed manuscript of the sort you describe, we are not optimistic that it would be accepted for publication.

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Ludwik Kowalski wrote:


> As you might recall (see the message quoted below) I wanted to publish
> a paper "Cold Fusion 15 Years Later," several months ago. A that time
> I was essentially an uncommitted observer of the field. My review paper
> is still waiting to be modified after the DOE panel report, as you
> suggested. The purpose of this message is to inform you that I have
> recently replicated two "cold fusion" experiments of Dr. Oriani, at the
> University of Minnesota. The label "cold fusion" is not appropriate
> here because neither heavy water nor palladium were used in the setup.
> And we do not believe that two isolated atomic nuclei could fuse at a
> low temperature to produce an observable effect. The probability of
> fusion, due to the tunneling effect, is too small for this. The only
> link between the concept of "cold fusion" and what we observed is a
> totally unexpected nuclear effect caused by chemical activity in an
> experimental setup. The Oriani effect was replicated by me twice (out
> of two attempt), as described at:
>
>     http://csam.montclair.edu/~kowalski/cf/188oriani.html
>
> I know that most nuclear physicists are convinced that chemical processes
> cannot produce nuclear effects. But I also believe that experiments have
> priority in Physics. A task of trying to prove, or disprove, a controversial
> claim, made by a reputable scientist (such as Dr. Richard Oriani), can be
> turned into an educational project. What can be a better way to expose
> students to the excitement of scientific research? Please let me know
> if we should submit a paper, based on the above webpage, for a possible
> publication in The Physics Teacher. I am now trying to get ready for one
> more experiment, with a totally new cell, with new chemicals and at our
> university laboratory. The article will be submitted if the number of
> tracks is again much larger than the background.
>
> The purpose would be to inform teachers about Oriani effects, and to
> encourage them to test one of them. Many high schools and universities
> are equipped to perform the simple experiment described in my webpage.
> Hopefully, some of them will attempt to replicate it. Please reply as
> soon as possible; we hope you will give our paper a chance of being
> published in TPT. The cold fusion, and the controversy about it, will
> not even be mentioned in our draft.
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I am not happy; an anticipation of bringing Oriani's experiments to the attention of teachers and students is part of my motivation. I should be able to accomplish this, eventually. Such experiments are indeed ideal for exposing students to the excitement of scientific research. They deserve to be developed; they deserve to be published. I will not give up.

A crown of etching hooks (made from the 0.038 mm nickel wire and attached to a thick copper wire ring) forces me to drill a tiny hole in each chip. This is a delicate operation. The hole is used to suspend the chip from the hook. Is this method, copied from Oriani, is really better than dropping all the chips into my constantly stirred electrolyte? The answer depends on whether or not one believes that occasional scratches, and other surface defects, can be produced on the chips in the whirling electrolyte. The new method is great for removing the chips and placing the crown into water, if desired, at the same time. In about an hour I will end the first 6 hours of etching

and go to school to examine the chips. Then I might decide to etch for another 6 hours. The chips to examine have already been described in item 29 above. This time I did not forget to suspend a chip irradiated with alpha particles, my indicator of the quality of etching.

42) My "open letter" to scientists, who were chosen by the DOE to reevaluate cold fusion claims, was posted this morning; it is the unit #196. Will they be informed by somebody about my letter? Will they read it? Will some of them answer to my questions? I hope so.

43) I also nearly finished the self-imposed task of describing Fisher's polyneutron hypothesis. The already posted draft is going to be corrected, if necessary, by John Fisher. He complemented me for this work. The only suggestion, so far, was to change the name of the "element # zero." My choice for the name for the first element was Adamium (symbol A) but John prefers Neutrium (symbol Nt). He invented this element to explain the observed nuclear anomalies. The draft has already been posted as the unit #191. I will modify it after receiving Fisher's recommendations. What a strange set of speculations! The theory is simple; that is its advantage.

44) I examined all the chips (after 6 hours of etching) and decided to etch for another 6 hours. This is going on right now. Nothing convincing (at least 10 times above the background) tracks but seem to have smaller diameters than tracks seen on the chip irradiated with Am-241. I do not expect anything spectacular tomorrow.

December 14, 2004

45) Learn by making mistakes is not unusual. That what happened last night and today. I was anticipating to stop etching at 10 p.m. But I forgot about this and came one hour later. Etching for 13 hours instead of 12 is not a big deal. But something else happened. The temperature of NaOH was 73 C (instead of 65 C) and the level of the liquid was at 175 cc (instead of 200 cc). In other word, the chips were over etched by time, by temperature and by concentration. They were still transparent but had dull surfaces. Looking through them was like looking through a light fog. For a moment I thought that all was lost.

Fortunately, the chip that was irradiated with alpha particles was also over etched. I examined that chip and found about as many tracks of alpha particles as after the first 6 hours of etching. The circles ware larger than before; they often touched each other. Then I look at other chips and saw more tracks than I was able to count before (after 6 hours of etching). Distinguishing tracks of nuclear particles from dirt and surface irregularities became much much easier. It seems that that over etching is good, at least for my purpose, counting alpha-looking tracks. The control chip (background) also had more tracks that I was able to recognize before.

46) The second thing I learned by mistake was that examining a wet chips might be desirable. This has to be verified later, perhaps by using a liquid that does not evaporate as quickly as water. I had some dirt on the chip and decided to wash it before reexamination. The chip was not totally dry when I placed it on the microscopic slide. My magnification was 40. The impression I had was that some surface irregularities nearly disappeared when water was there. Will tracks be more recognizable when chips are coated with a thin layer of oil? That is worth exploring, when time becomes available.

47) Here are the results of counting, after 13 hours of etching:
.....
.....
I went to school to count tracks. But, after spending several hours at the microscope I found nothing significantly higher than the background, except in the chip J.

The conclusion from this experiment is that the expected effect was not confirmed (chips A and G showed no tracks above the background). Each of the chips F1, F2, F3 and F4 recorded about two times more tracks on one surface than on the other (and about three times more than in the background) but I do not trust myself on this. Why not? Because these chips were from AlphaTracks. That is the material that became foggy after over-etching, as I wrote earlier. The chips from Landauer were also over-etched but they did not become foggy. I think that AlphaTrack chips are OK for detection of alpha particles but they are not good for me. In the next experiment I will be using fresh Fukovi chips; they were sent to me today from Japan.

The only interesting results are those from the E and J chips where I really see at least ten times more tracks than in the background. The chip E was exposed to the outside wall of the upper tube during the electrolysis and the chip J was exposed to outside wall of the upper tube after the electrolysis. Both chips were from the CR-39 that Oriani used before he switched to Fukovi chips. They came from Italy; each chip is 1 mm thick. When E and J chips were applied to the tube I expected to see tracks on the surface facing the tube only. The other side was to be used to measure the background. But now I know that excessive tracks appear on both sides of E and J. It means I did have a control sample for the background in that material. I am now etching another chip; it will be my control sample tomorrow.

The question to answer is: "How many tracks are there on each surface of E and J and how do these numbers compare to the background tracks. Only then will I start speculating about neutrons (as I did above). Unfortunately my notebook does not say which side of the CR-39 chip was facing the glass. I had a rule about this: "scratched symbols should be on the side opposite to where particles are expected." But did I really follow that rule? I think I did, but am not sure. Tomorrow will be the last day of reporting, I hope.

December 15, 2004
OK, the experiment is over. I did not expect to turn a logbook for an experiment into a diary. But it was my first independently performed cold fusion experiment. I did not find what I was lookin for but an eqaully puzzling phenomenon was confirmed. A “nuclear signature”does come out of the lectrolytic cell but it comed through glass not through the nickel. Here are the results of today's counting.

Chop E: was in contact with the outside wall during the electrolysis (25 hours when the current was 0.30 A). The accessible surface area was about 0.45 square cm. The number of tracks on the side facing the tube was 580. Subtracting the background of 50 tracks (see below) one has the average detection rate of 47 tracks per hour, per square centimeter. The number of tracks on the opposite side (facing air in the room) was 48. This is the same as the background. In other words, my speculation about neutrons was not justified. But the anomalous tracks recorded by the detector E are real.

Chop J: was in contact with the inside wall after the electrolysis (175 hours). The accessible surface area was the same as for the chip E. The number of tracks on the side facing the tube was 169 This is only three times as much as the background. Taking the difference of 169 - 50 = 119 seriously, one finds the average detection rate of 1.5 tracks per hour, per square centimeter. It would be better, in this case, if I used Oriani.s method of subtracting the background. Is the emission of particles (from glass) after the electrolysis as real as during the electrolysis? I do not think so. My approach was too crude; I was looking for a strong effect. But I would probably have less trouble with 1.5 tracks per hour per cm^2 from nickel because the CR-39 chip placed below the cathode was from Landauer. The limit of detectibility would probably be 0.5 tracks per hour per cm^2

Chip Q: was my background for the same CR-39 as chips E and J (1 mm thick from an Italian supplier) and etched for 12 hours, as the other two chips (E and J). The number of tracks counted on one area was 53. The other area had a comparable number of tracks.

The bottom line is that the effect for which I was looking, nuclear particles in the detector placed below the nickel cathode, was not found. I used my best CR-39 (from Landauer) for the detector below the cathode, and for the measurement of the background. Other CR-39 chi were from two other suppliers. They were about as good as my better chips for the detection of alpha particles from ^{241}Am. I checked that. But I was not familiar with the background in chips from other suppliers. The unfamiliar material was used on something that was added in the last moment.

My failure to confirm tracks in the air below the cathode contrast with successes Oriani had after my departure from Minneapolis. Is it possible that my failure is caused by the absence of something that tap water has in Minnesota but not in New Jersey? I know what a skeptic might say. S/he would remind me that, at very low concentrations, alpha radioactive substances are always present in everything. And s/he would speculate that they might migrate in the electrolytic cell concentrating, for example, on the nickel or glass. Then they diffuse and become emitters of what one observes. But negative results, as Oriani likes to say, confirm that this does not happen. A skeptic can be criticized for not being consistent. If only 100% reproducible results should be taken seriously then contamination should also not be taken seriously.

Here are the pictures I took this morning, through the microscope.

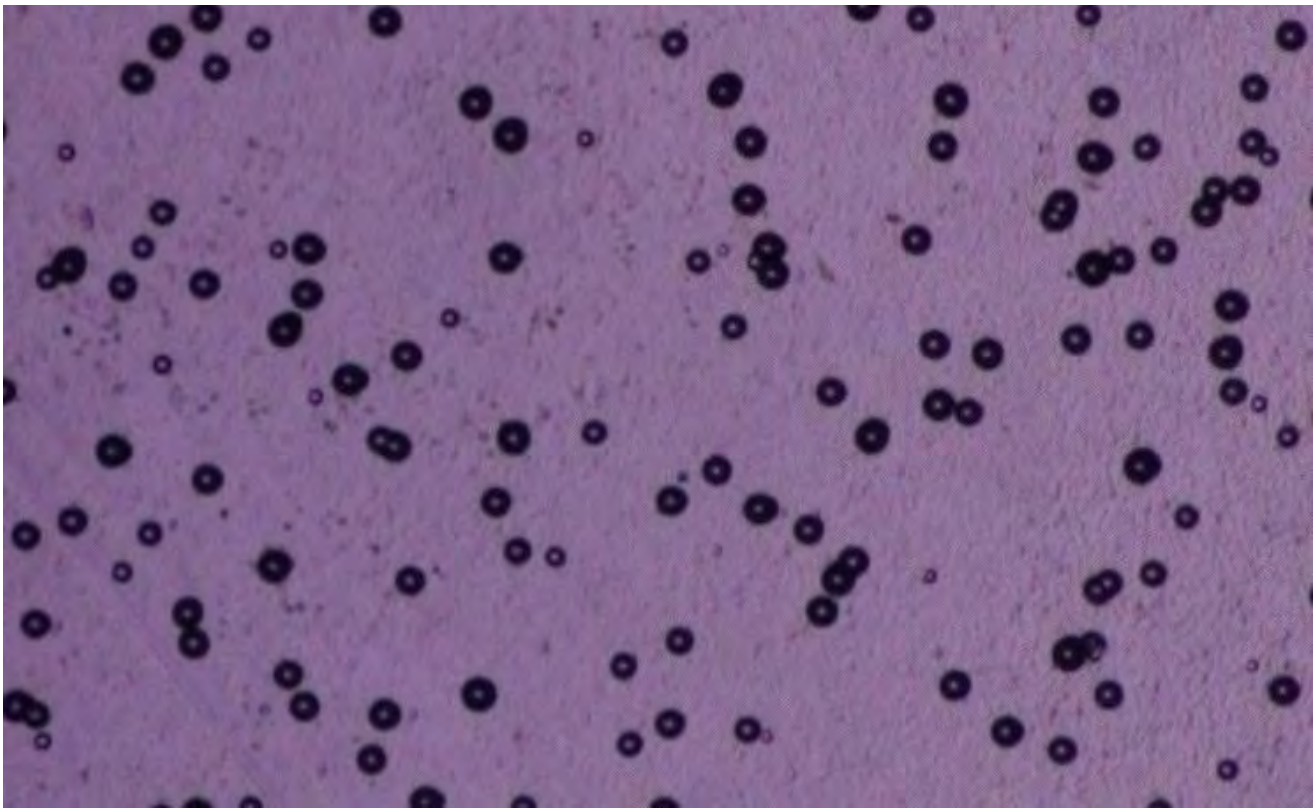

Figure 1 Alpha particles from a ^{241}Am source (in a little corner of chip Q)

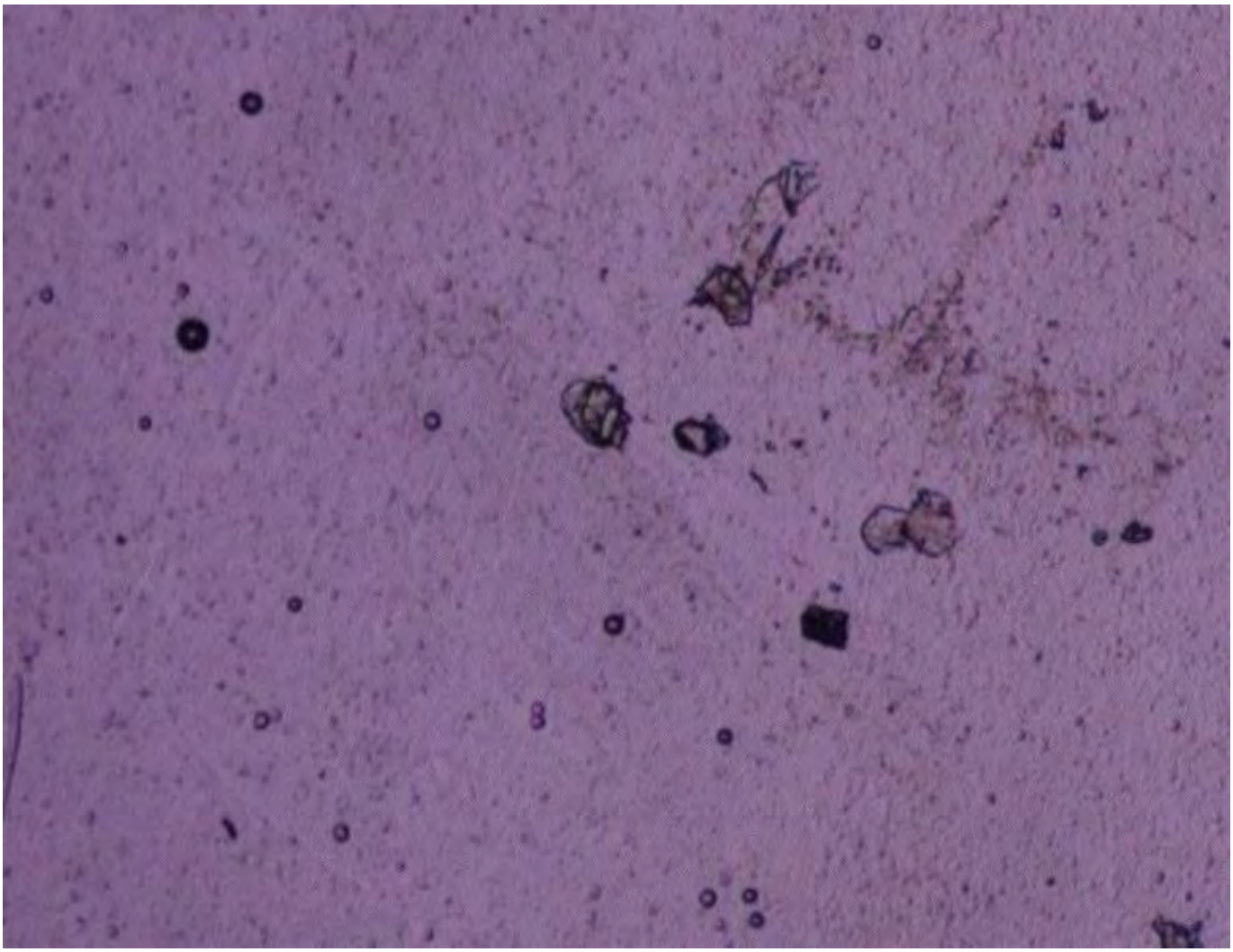

Figure 2 A typical background field (the other side of the chip Q). I suspect that most background tracks are due to cosmic protons, not radon. Detectors were protected from alpha particles in storage (by thin plastic covers).

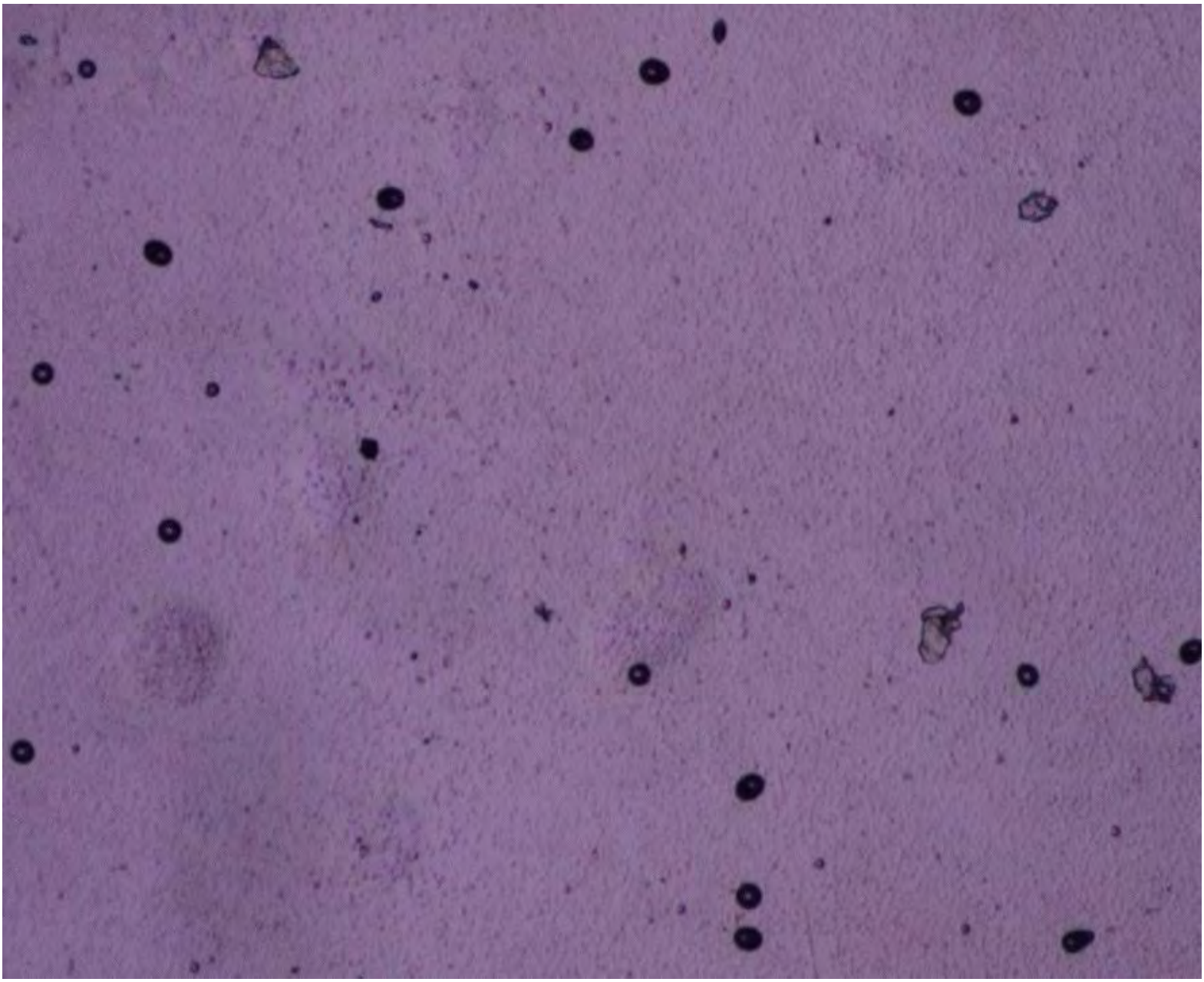

Figure 3 Tracks on the more active side of the chip E

The areas seen under the magnification 40 were 0.91 square millimeters each. But I trimmed the pictures to reduce sizes of files.

December 23, 2004
I am now in the middle of another experiment. It is going to be described in unit #197. But before going there let me say that an attempt to detect tracks in a Cr-39 chip that was applied to the glass after the electrolysis produced the negative result. The chip was in contact with glass for 85 hours. The particles were present during the electrolysis but not after the electrolysis. The experiment described in the unit #197 is going to end in about three or four days. Then many days of etching and examining CR-39. The last experiment described in this unit differed from the experiment we conducted in Minneapolis in two significant ways; that may explain negative results, as far as the CR-39 below the cathode was concerned. the differences were:

a) Oriani allowed concentration of the electrolyte from very low (at the beginning) to several times stronger (when a lot of water was lost). I was keeping the water level constant by adding water very often. My concentration was very weak all the time.

b) My anode was at a distance of about 15 mm from the cathode, his was about 5 mm. Furthermore, I had pulsating current, due to the accumulation of gasses below the platinum disk. In the experiment described in the unit #197 The current was again pulsing because the diameter of the anode was still too large. Oriani said that this should not be tolerated. Therefore I had to turn the current off, remove the anode and reduce its size even more. At the same time the distance between the anode and the cathode was reduced to about 7 mm. The whole operation took only 10 minutes (without removing the electrolyte) and pulsating current was eliminated. But this is part of the

next experiment.

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193) Links to another website

Ludwik Kowalski (December 3, 2004)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Dr. Mitchell Swartz, who I met at the last year's cold fusion conference (ICCF10), also has a site dedicated to cold fusion. This morning he emailed me a message (see below) with several links. I am posting them for your convenience. Please keep in mind that I do not feel competent conducting excess heat experiments. Measuring excess heat, produced at a rate smaller than one watt, is not trivial. Ruling out the non nuclear origin of such heat is very difficult. I am not a chemist. On the other hand, students should be encouraged to explore excess heat phenomena. Tasks of proving, or disproving, controversial claims, made by reputable scientists should be turned into educational projects. What can be a better way to expose students to the excitement of scientific research? Mitchell wrote:

Ludwik:
We have placed numerous links to your site, and would appreciate a link or two to JET Thermal Products. These are the links for JET Thermal Products which has on line teachings of how to reproducibly obtain cold fusion, and has an archive of information supplementing your site.

JET Thermal Products http://world.std.com/~mica/jet.html

http://world.std.com/~mica/jetlogo.html

JET Thermal Products (Introduction including engineering and optimal operating point)
http://world.std.com/~mica/jet.html

JET Thermal Products - Introduction to Cold Fusion http://world.std.com/~mica/jetprdnx.html

JET Thermal Products - Cold Fusion Science (Introduction to the engineering and material science)
http://world.std.com/~mica/jetintro.html

JET Thermal Products - Public Open-House Cold Fusion (ICCF-10) Demonstration at MIT
http://world.std.com/~mica/jeticcf10demo.html

Best wishes.

Dr. Mitchell Swartz

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URLS:

COLD FUSION TIMES
http://world.std.com/~mica/cft.html

JET THERMAL PRODUCTS
http://world.std.com/~mica/jet.html

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194) Comments about theories

Ludwik Kowalski (December 5, 2004)
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This morning I received an interesting message from an Israeli researcher, Boris Khachaturov, who I met at the last cold fusion conference. Referring to the Occam's Razor principle, and showing the URL below, he wrote:

http://en.wikipedia.org/wiki/Occam's_razor

". . . In any case, Occam's principle should be a guide to researchers trying to explain experimental facts. That is what I learned from my teachers. The principle is used, implicitly or explicitly, not only in science but in everyday life as well. I was surprised to discover that cold fusion researchers often ignore simple explanations of experimental facts and invent incredible "theories." Such theories are great brain teasers but they have nothing to do with what is actually observed in laboratories. Such treaties could easily be recognized among presentations at our cold fusion conference. But I am not saying that all theoretical presentations belonged to that category."

Yes, a theory that is simple is more desirable than a theory that is complicated, when both of them explain experimental facts successfully. But I am not sure which "simple explanations" are being ignored. Also, what might be simplier to one person might be more difficult to another. Does the concept of simplicity apply to the number of assumptions made to dervive a theory or does it apply to the mathematical sophistication of the derivation process itself?

In the unit #191, on this website, I am trying to describe my own understanding of the "polyneutron theory" of John Fisher. This work is in progress. The theory is rather simple, as far as mathematics is concerned. But its assumptions are far from being obvious. Do polyneutrons exist and do they have properties assigned to them by Fisher? Only experiments can answer this question. One has to admire those who invent theories explaining experimental facts. But tentative theories, in my opinion, should first be presented to people able to understand them. I did not see too many people able to understand the monopole theory defended at our conference. If I were the author of that theory I would not bring it to a cold fusion conference; I would bring it to a conference on electrodynamics, for example.

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195) Oriani showers.

Ludwik Kowalski (December 3, 2004)
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The unit #188 was focused on Oriani sold effect. The Oriani Liquid and Oriani Gas effects were mentioned in it but not elaborated upon. John Fisher, whose polyneutron theory (to be described the unit #191) provided motivation for placing CR-39 detectors into the vapor phase, and who collaborated with Richard Oriani, sent me the pdf file describing their work. The paper focuses on showers of particles in vapors. Description of that experimental work has been available, for more that a year, from the library at:

html://www.lenr-canr.org

The URL for the file that John Fisher sent me (on 12/1/04) is:

http://blake.montclair.edu/~kowalskil/cf/bigshower.pdf

Enjoy reading this paper, and other important contributions made by these two scientists. In giving me his permission to make the file downloadable from my list Richard Oriani wrote that he is thankful “for making our paper available through another channel, particularly because publication of this paper in physics journals was not possible.“ Publishing a report in this area, even by high caliber scientists, is much more difficult than publishing a report in another area of physical science.

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196) Open letter to the DOE and its team of 18 scientists

Ludwik Kowalski (December 11, 2004)
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1) This unit (a set of questions) was prompted by a recent note entitled "US Review Rekindles Cold Fusion Debate." Written by Geoff Brumfiel, from Nature, it was published on12/2/04.

<http://www.nature.com/news/2004/041129/full/041129-11.html>

The author wrote: "Claims of cold fusion are intriguing, but not convincing. That is the conclusion of an 18-member scientific panel tasked with reviewing research in the area. The findings, which were released on 1 December by the US Department of Energy, rekindle a 15-year-old debate over whether nuclear fusion can occur at room temperature."

I do not know any scientist who claims that useful energy can be produced from fusion of two isolated deuterium nuclei at a room temperature (ignoring Coulomb barrier). That was the major topic of concern 15 years ago. The new DOE panel was expected, as far as I know, to evaluate claims of selected nuclear anomalies, such as generation of helium, or emission of 3 MeV protons, that were not know in 1989.

2) Broomfield wrote that "According to the report, the panel was split approximately evenly on the question of whether cold fusion experiments were actually producing power in the form of heat." Is it not true that in science controversies are resolved through better experiments rather than by counting how many experts are for and against competing claims?

3) Were you, the members of the panel, offered opportunities to visit laboratories of cold fusion researchers in order to participate in decisive experiments, or to personally observe and criticize them?

4) According to the DOE report your conclusions were reached on the basis of one review paper (and interactions with selected cold fusion researchers during a one-day meeting). I would like to know if, in addition, some of you tried to replicate selected experiments in your laboratories. If so, then please share your findings.

5) Did you request anonymity or was it the DOE initiative to remove your names from individual reports? I am probably not the only one interested in your names and affiliations. As you know, scientific publications are usually signed -- for good reason. Who among you were "nine additional scientists chosen by DOE for their expertise in relevant fields"? Who among you are experts on detection of helium, at low concentrations?

6) Four months before the second DOE report was published I addressed you, indirectly, in the form of a Letter to the Editor of Physics Today (published on page 14 in the September 2004 issue). I asked: "Is there any indication that leading cold fusion scientists are incompetent or that their data are fraudulent? Is the research methodology that cold fusion scientists use different from that used in other areas of physical

science? Answers to these questions will help me decide what to think about cold fusion and what to tell students about it." The title of my letter was "Seeking Answers From Cold Fusion Review." Why was the issue of competence and honesty not mentioned in the DOE report? I expected it to be addressed in view of commonly used epithets, such as "bad science," "voodoo science," and worse. A clear statement about qualifications of those whose work was investigated would be extremely useful in the context of existing accusations and attitudes.

7) I agree with you that "claims of cold fusion are intriguing, but not convincing." Experiments must be 100% reproducible to be convincing. But that does not mean they are fraudulent, or that they belong to voodoo science. Electrostatic experiments were also irreproducible before the role of humidity was recognized. That is why more research is needed. The biggest obstacle is negative publicity. You had a chance to decisively remove that obstacle. But you did not take advantage of that opportunity. What should be done now to help the honest and qualified scientists who are currently exploring the intriguing aspects you mentioned in the report? Conditions of their work are highly abnormal; what can be done to improve them significantely?

8) I would be happy to post your replies here; they will be seen by many interested (and often very qualified) readers. Your replies are likely to become important historical documents, no matter how the issues are resolved in the future.

9) Let me finish this open letter by introducing myself. After teaching physics for more than three decades, at Montclair State University in New Jersey, I retired and became an independent researcher. I was trained as an experimental nuclear physicist (Ph.D. 1963, University of Paris, France) and am now happy to be an investigator of LENR phenomena.

P.S. (12/12/04)

10) I believe that your reason for addressing the issue is not different than mine; we want to see the LENR controversy resolved, one way or another. I also think that it is premature to speculate about practical applications; the emphasis should be on scientific aspects of anomalous phenomena, not on benefits they might possibly offer. Technological explorations will follow naturally after anomalous effects are recognized as real; and after normality is established.

11) In the DOE report I read: "Results reported in the review document purported to show that ^{4}He was detected in five out of sixteen cases where electrolytic cells were reported to be producing excess heat. The detected ^{4}He was typically very close to, but reportedly above background levels." Were all sixteen experiments conducted by equally qualified researchers?

In any case, such experimental results are highly significant; total absence of nuclear byproducts (ashes) was one of the most convincing arguments against the suggested nuclear origin of excess heat. That is why I took the 1989 DOE report very seriously. On the basis of your observations, I would say that a tremendous progress was made on the issue of "missing ashes."

12) I am disappointed that additional experiments were not performed by experts among you to clarify the situation, for example to show that the effects are not due to contamination. I would very much prefer to have a delay of one year, if necessary, than a timely report stating, essentially, "on one hand this and on the other that." An old joke about a one-handed lawyer came to my mind when I was reading the second DOE

report. For everything positive in the DOE report there is an immediate negative, and vice versa. How can such report help us to form a valid opinion about what has happened in the LENR field in the last decade? As experts you are in a much better position to address the contradictions than most of us.

13) And here is another reason to be disappointed. The new DOE report states: " To explain these unusual characteristics, the reviewers were presented with a theoretical framework that purported to describe how collective energy from the material lattice couples to a deuteron pair to induce fusion, how the only fusion reaction channel that occurs would be the production of ^{4}He, and how all the energy is coupled back into the material in the form of heat instead of high energy gamma-rays. The reviewers raised serious concerns regarding the assumptions postulated in the proposed theoretical model for the explanation for ^{4}He production."

Generation of helium from deuterium is anomalous because it can not be explained by accepted theoretical models. I agree that our textbook models must be taken very seriously because they have been confirmed by highly reliable experimental data. You are not alone in thinking that attempts to develop a better theoretical model (presumably explaining generation of ^{4}He) are far from being totally satisfactory. But why should this weaken our confidence in the experimental fact itself? Why was the weakness of a new model mentioned in this context? Yes, I know, the field is blind without a theory to guide it. But wouldn't you agree that establishing validity of experimental facts is even more impotrant, at this stage?

14) According to the last paragraph of the DOE report "material science aspects of deuterated metals using modern characterization techniques could be helpful in resolving some of the controversies in the field." That is certainly true. But use of commonly available, and much less expensive, tools should also be encouraged, especially among students. To prove, or disprove, a controversial claim can be an educational project. What can be a better way to expose students to the excitement of scientific research?

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197) Second Oriani experiment in my lab

Ludwik Kowalski (December 23, 2004)
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One can consider this to be a continuation of my unit #192. That was open electronic logbook that turned into a diary. I am going to be much more concise in this description. I am using the same cell but the CR-39 is from Fukuvi. At the end of unit #192 I explained why my previous experiment should not be considered as a replication of what was described in the unit #188.

Description of electrolysis:

a) 23 hours at the nominal current of 100 mA (pulsation due to bubbling)

b) 24 hours at the nominal current of 200 mA (pulsation due to bubbling)

c) Current was cut off for 10 minutes to bring the anode closer to the cathode and to reduce the anode diameter. This was necessary to eliminate pulsation).

d) 33.5 hours without pulsing. The current was growing slowly from 200 mA to 375 mA, as water was decomposed and the concentration of the electrolyte ~doubled).
Then water was added to return to the original concentration.

e) 14.5 hours without pulsing. The current was growing slowly from 375 mA to 510 mA, as water was decomposed and the concentration of the electrolyte ~doubled).
Then water was added to return to the original concentration.

f) 18.5 hours without pulsing. The current was growing from 350 mA to 600 mA, as water was decomposed and the concentration of the electrolyte ~doubled. Then water was added to return to the original concentration.

g) 16 hours without pulsing. The current was growing from 350 mA to 600 mA, as water was decomposed and the concentration of the electrolyte ~doubled. Some water was added after 12 hours to prolong the high concentration run. Then water was added to return to the original concentration.

h) 18 hours without pulsing. The current was growing from 350 mA to 600 mA, as water was decomposed and the concentration of the electrolyte ~doubled. Then water was added to return to the original concentration. **At this point I accumulated 102.5 hours of normal electrolysis (after 47 hours of the electrolysis with the pulsating current).**
A potential problem: I must address the issue of concentrations of the electrolyte. Both Oriani and me start from the same concentration (2.36 grams of Li_2SO_4 salt per 100 cm^3). But my tube (total length above the cathode) is 7 cm while his is about two times longer. I must add the liquid when the level is 3.5 cm above the cathode (to have nearly 1 cm cover above the cathode. In going to that level the volume of my liquid is reduced by a factor of 3. It means my concentration increases by the same factor when water is decomposed. In longer tube the concentration changes by the factor of 4 or 5. This implies that some concentrations used by Oriani were never tried in my cell. In trying to replicate his findings I must run electrolysis at higher concentrations. To triple my minimum concentration, for example, I must add 2*0.29 = 0.58 grams of the salt before filling the tube to the rim.

But I have no scale at home. However my bottle with originally prepared electrolyte is not empty. Instead of adding pure water (as I do when the minimum level is reached) I will add the original electrolyte. This will add 0.19 grams of the Li_2SO_4, salt. (minimum concentration will increase by the factor of 1.65, doing it twice will increase it by the factor of 2.3 and doing this three times will increase it by the factor of 3.0. And one more dose of 019 grams of Li_2SO_4, would make the factor equal to 3.6. That is what I am going to do from now on -- refilling with the electrolyte instead of water. This will not take away racks that have already been accumulated so far in 150 hours. Who knows, perhaps the secret of success is in using high concentrations?

Yes, I know that ideally everything should be as constant as possible in a single experiment. Then one could change one parameter, for example, the current or concentration, and repeat the experiment. And that is what i might start doing when the goal will be to understand and to optimize. But for the time being the goal is to convince myself, and others, that the discoveries made by Oriani are real and worth focusing on. His philosophy is to scan over different currents and concentrations in order to approach 100% reproducibility. I am following this philosophy.

i) Add one dose of 0.19 grams of salt and run the cell for **4 hours** at the nearly constant current (0.55A to 0.65A) to bring the level of the liquid to its minimum before the end of the day. After these 4 hours one of the two CR-39 pieces was removed from the inside of the cell. It was the chip K (Scratched cross was at the bottom; it was not facing the second immersed piece).

Also one pair of CR-39 touching the tube from the outside was removed. The piece facing the tube was G (cross and G are at the top, the side facing the cell was the one that had G and the cross). The other removed piece was L. It was behind the piece G. The cross and the L were near the top, as on the piece G. The side with L on it was in contact with the piece G. A fresh single CR-39 was placed to replace the G+L pair.

j) Add the second dose of 0.19 grams of salt and run the cell **for 12 hours**. During that time the current was changing from 020A to 0.25A in 5 hours and then kept more or less constant near 0.6A for another 7 hours.

k) Add the third dose of 0.19 grams of salt and run the cell **for 12 hours**. During that time the current was around 0.25A for the first 5 hrs and around 0.6A in the remaining 7 hours.

l) Add the fourth (and last) dose of 0.19 grams of salt and run the cell **for 15 hours**. (starting on 12/27/04 at 13:00). During that time the current was around 0.30A for the first 5 hours and around 0.6A in the remaining 10 hours.

m) Add pure water and run the cell **for 12 hours**. During that last electrolysis step the initial current was 0.35A and the final current was 0.6A.

The electrolysis will be stopped after that. Then fresh CR-39 chips will be applied to each side (“was dry” and “was wet”) of the Ni cathode. Perhaps absence of tracks during the electrolysis (I hope they will not be absent) does not mean that no tracks can appear later.

Description of detectors in and around the cell:

The exposed CR-39 area is about the same as in the experiment 192. But the area surrounding the tube is about 50 times larger than my single chip in experiment 192. That single chip caught a lot of tracks (more than ten times above the background) during the electrolysis but nothing after the electrolysis (as described at the end of the unit #192). I am focusing on tracks that Oriani discovered outside the tube. The tube is surrounded by large pieces of CR-39, as illustrated in Figure 1. The piece facing us is 5 cm long and 1 cm wide. Actually it is a set of two pieces, one behind the other. The “cover”, also a set of two pieces, is exposed to the escaping gases during the entire electrolysis. The set of two pieces might be able to detect neutrons, if enough of them are emitted to create recoiling protons. Note the holes drilled in CR-39. They will be used to suspend the pieces from thin nickel wire hooks during the etching (in hot NaOH).

FIGURE 1

I will describe and label these pieces in the next section. Two large CR-39 pieces, not visible in the illustration, were inserted into the electrolyte, above the anode. The level of the electrolyte changed due to decomposition of water. Therefore, only lower parts of the inserted CR-39 pieces are always in the electrolyte. The upper part of the inserted pieces are always exposed to escaping hydrogen and oxygen vapors. Middle sections are sometimes in the electrolyte and sometimes in the vapors. There is also a piece sitting near the top of the tube (without blocking the escaping H_2 and O_2).

The entire outside area of the upper tube (28 cm^2), is exposed to CR-39. This is 62 times larger than the area covered in the last experiment. Likewise, the duration of the electrolysis is nearly three times longer. Therefore, my chance of observing particles emerging from the tube (I observed their tracks in the last experiment) is much higher now than before. And about 20 cm^2 of the exposed CR-39 area inside the tube should reveal particles that Oriani observed inside the electrolyte.

Labeling of CR-39 pieces:

1) Chips A, B and BB will tell me if the outside wall of the tube, where tracks were discovered in my first experiment (see the unit #192), is still active when the this second experiment started.

2) Chip BBB is my background control sample.

3) **Chips C and Pcrossed** were in the air below the cathode during the entire 202.5 hrs of electrolysis. The chip C was looking at the Ni cathode (with its unlabeled side) while the chip Pcrossed was looking at the chip C from below. The labeled side of the chip Pcrossed is looking down toward a wooden cylinder supporting it.

4) **Chips I and J** were a pair making a “cover” above the cell. Chip I was looking down to the rising vapors (with its shinny side); The shiny side of the chip J was also looking down, thought the chip I. The labeled sides of both I and J were looking up. Both of these were used during the entire 202.5 hrs of electrolysis.

5) Chips D and M, both large, stand along the vertical glass wall of the tube. Chip D touches the glass with its shinny side, chip M touched the chip D with its shinny side. Both of these were used during the entire 202.5 hrs of electrolysis.

6) Chips E and N, both large, stand along the vertical glass wall of the tube. Chip E touches the glass with its shinny side, chip N touched the chip E with its shinny side. Both of these were used during the entire 202.5 hrs of electrolysis.

7) Chips H and O, both large, stand along the vertical glass wall of the tube. Chip H touches the glass with its shinny side, chip O touched the chip H with its shinny side. Both of these were used during the entire 202.5 hrs of electrolysis.

8) Chips G and L, both large, stand along the vertical glass wall of the tube. Chip G touches the glass with its shinny side, chip L touched the chip G with its shinny side. These two chips were removed before the step (i) of the electrolysis. In other words, they were collecting tracks only during the first 147.5 hours of electrolysis.

10) Chips F and K were suspended into the cell (crossed labeled down). F remained in the cell during the entire duration of the electrolysis (202.5 hours) while chip F was removed before the step (i), after 147.5 hours. Chip F was replaced by the chip S that remained in the cell for 50 hours (when the concentration of the electrolyte was increased and when larger currents started to be used).

11) Chips P, Q and PP were also used; I will describe their purpose only when it becomes necessary. were also standing along the tube walls.

12) Chips used after the electrolysis will be described later.

Yes, I know that ideally everything should be as constant as possible in a single experiment. Then one could change one parameter, for example, the current or concentration, and repeat the experiment. And that is what I might start doing when the goal will be to understand and to optimize. But for the time being the goal is to convince myself, and others, that the discoveries made by Oriani are real and worth focusing on. His philosophy is to scan over different currents and concentrations in order to approach 100% reproducibility. I am following this reasonable philosophy. What would be the purpose of changing one parameter at a time when outcomes of consecutive experiments are very different, even when controllable parameters are not changed?

Etching, observations, reflections, etc.

1) The figure below shows tracks due to alpha particles from an 241Am source. The chip, etched at the same time as that shown in Figure 3, was dirty. But, as one can see, dirt, scratches and other surface defects can easily be distinguished. The lower left corner is the chip boundary. Ambiguities in tracks counting are minimized when chips are handled carefully, and when they are washed after etching.

FIGURE 2

2) Without ending the electrolysis I removed three large CR-39 chips, K, G and L (after 147.5 hours) and etched them. The area of each chip is about 5 cm2. The chip K, that was suspended into the cell, was at once replaced by a fresh chip S. The average densities (based on random sampling of fields), were between 300 and 700 tracks per cm2 The nominal background density is about 10 tracks per cm2. The distribution of tracks was found to be clearly nonuniform. Clustering of tracks is illustrated in Figure 3. It is a photo of 18 tracks in the field of view whose area is 0.7 square millimeters. The four adjacent fields (of the same size, on the left, right, above and below) had zero tracks. Of the four diagonally nearest fields two had no tracks and two had one track each.

FIGURE 3

3) The electrolysis ended after 202.5 hours and all chips were etched. During the last 50 hours the concentration of the electrolyte was increased and currents up to 0.7A were used (instead of the up to about 0.35A before). The cathode was removed and sandwiched between two CR-39 chips. Likewise, chips were applied to the anode and to the electrolyte removed from the cell. Will these control chips also display particles, after I etch them in several days? I will let you know.

4) Chip S (that replaced the chip K inside the cell) was found to be covered with more particles (also clustered) than the chip K. More specifically, the average track density on two surfaces of the chip K (after 147 hours a accumulation) was about 520 tracks per cm2 (or 3.5 tr/cm^2 per hour). For the chip S, on the other hand, the average track density on two surface (after 147 hours a accumulation) appears to be at least two times higher. That seems to indicate that using more concentrated electrolytes, for example 0.5 M or above, and larger currents, is desirable.

5) And what about the tracks on chips D, E and H that were facing the glass walls of the tube? Each of these chips revealed clustering of tracks, similar to those shown in Figure 2. The same was seen on the chip Q that was used to replace the chip G. Facing this situation I am inclined to anticipate that anomalous tracks are likely to be 100% reproducible in setups similar to that shown in Figure 1. Please write to me <kowalskil@mail.montclair.edu> about your own attempts to replicate Oriani's experiments; I will append your message to this webpage. Let me add, however, that quantitatively, results are not yet consistent. Additional experiments, preferably performed in different laboratories, are needed. The topic is too big for one or two people; your help will be highly appreciated.

6) As you may recall, my first experiment (see the unit #192) failed to confirm presence of nuclear particles in the air outside the cathode. Were I able to detect such particles in this experiment? The 1 cm2 chip, placed below the cathode, did record 28 particles in 202 hours of electrolysis . The method I am using is not appropriate for making decisions about very low recording rates. I do not know why the average recording rate was much higher in the experiment performed at Minneapolis (see the unit #188).

! ! ! P.S. (12/30/2004) ! ! !
Please suspend your judgment about the preliminary results described above. I just discovered that something alpha-radioactive might have been sporadically deposited on some of my CR-39 chips, when they were etched in the hot solution of NaOH. The entire experiment must be repeated, after the source of the contamination is identified and eliminated. I still believe that what I wrote above will be confirmed, at least qualitatively. But how can I be certain about this now, after discovering a lot of tracks on my both blank control samples? More about this later.

7) Tomorrow I will etch several control samples. The goal is to convince myself that blanks chips do not display any tracks after I stop suspending them from the thin telephone wires. I suspect that insulation of that wire released something radioactive. Perhaps it was uranium or thorium in a dye used for color coding. In most cases, but not always, the plastic insulation was was removed. In any case, I will etch the following chips:

Chip A: "blank" that was exposed to the air in the room for two weeks.
Chip B: "blank" that was also exposed for two weeks but in another area.
Chip C: "blank" that was exposed to the air in the room for about 0.5 hours only.
Chip D: was exposed (50 hours) to the plastic cover of the telephone wire.
Chip E: was exposed (50 hours) to the copper of the telephone wire.

Chip F: was exposed (10 seconds) to alpha particles from 241Am (at one end only).

Chip G: was exposed (50 hours) to the electrolyte after the electrolysis.
Chip H: was exposed (90 hours) to the nickel cathode after the electrolysis (dry side)
Chip I: was exposed (90 hours) to the nickel cathode after the electrolysis (wet side)
Chip J: was exposed (90 hours) to the platinum anode after the electrolysis.
Chip K: was exposed (90 hours) to the inside glass wall after the electrolysis.
Chip L: was exposed (90 hours) to the outside glass wall after the electrolysis.
Chip M: was exposed (90 hours) to the black plastic holder of chips used in etching.
Chip N: was exposed (50 hours) to the blue plastic sleeves in the crown of hooks.

Hopefully, the first three chips will show no contamination in the air or in the etching solution. The next two will test my hypothesis that contamination came from the plastic cover and not from the copper wire. (But the hypothesis assumes that contamination was released at 70 degrees C, during the etching, while the plastic cover was exposed to the CR-39 at room temperature. The test might turn out to be nonconclusive.) The chip F is my usual calibration test, to make sure that etching itself was sufficient. It is convenient to have F available to recall the appearance of alpha tracks, when in doubt. The last eight chips will tell me whether or not the cell is ready for the next experiment. Hopefully, these eight chips will show about as many tracks per unit area as chips A and B.

P.S. (1/2/05)
Yes, a lot of tracks were found on the chip D while the chip A had at most 3 tracks per square cmentimeter. I was lucky; the test was conclusive.Finding a source of weak contmination in a system made from many different materials can be a formidable task, unless the source is strong enough for a simple Geiger counter.

The more exciting, and puzzling, are huge numbers of tracks on chips H and I. Why did I not see tracks below the dry nickel cathode during the 202 hours of electrolysis? Why did I see a lot of them in 90 hours immediately after the electrolysis? There are only two explanations of this, as far as I can say: (a) Particles producing tracks were stopped in 6 mm of air (before reaching the CR-39) and (b) the foil started emitting particles after the current was turned off (or shortly before that). The first explanation is in conflict with the experiment i performed with Oriani, and with his subsequent experiments; the distance of 6 mm was chosen to match his distance. The second explanation is “hard to accept” but that is a matter of psychology, not physics. More about all this later.

I just started another accumulation of tracks from the used nickel cathode (another set of chips H and I) and from the glass wall (another set of chips chips K and L). Will I confirm Oriani’s observation that the rate of tracks accumulation

is decreasing in times? Will this decrease be exponential? What will the half-life be? That remains to be seen.

P.S. (1/4/05)
The second accumulations mentioned above ended today and third accumulations started, for both nickel and glass. Oriani sent me his numbers of tracks/cm^2 per hour. They are consistant with the half-life of about 10 days. Will my accumulations confirm this? I looked at information about daugthers of radon and thoron. Nowhere could I see the half-life close to 10 hours.

Here is a trivial problem: "Knowing the half-life of a radioactive isotope calculate how many atoms must be present to emit 100 particles per hour (Activity = 100 per hour)?"

T = 10days = 240 hours ---> lambda = ln2/T = 0.69/240 = 0.029 per hour

A=lambda*N ---> N = A / lambda = 100/0.029 = 34624 atoms.

This is an exceedingly small number of atoms. A piece of dust, I suppose, can be expected to contain that many atoms of any element of the periodic chart. Yes, I know, it is too early to speculate that we are dealing with some kind of induced radioactivity. The only thing we know is that charged nuclear particles are present, even after the electrolysis, and that the average rate of emission, initially on the order of 100 per hour (in all directions) decreases it time. I have no idea what can cause this, except a radioactive substance.

My results (first post-electrolysis analysis, called "experiment #3'):
Chip A: Blank Only two or four tracks per square centimenter.
Chip B: another blank, but with 300 tracks/cm^2. Perhaps the metal box in which it was kept (?) contained something. In any case, this translates to 3.3 tracks/cm^2 per hour. I am still learning; a blank control chip must be handled in the same way as other detector chips, except for the electric current. Next time I will place it into the electrolyte together with the platinum wire, and the scraps of my nickel foil.
Chip C: another blank, clean as A above.
Chip D:: plastic cover over the copper wire. On the average 110 tracks per 0/044 cm^2. This translates into 50 tr/cm^2 per hour. The spiral pankake of wire was placed on the top of a chip for 90 hours. That is very high recording rate.
Chip E:: Copper wire. Some tracks were recorded in 90 hours.
Chip F: 241Am tracks are a large and easy to identify. Etching time was just right.
Chip G: Chip placed into the electrolyte for 50 hrs. About 2 tracks/cm^2 per hour.
Chip H: Chip on top of DRY nickel for 90 hours after the electrolysis 23.1 tr/cm^2 per hour. This is comparable to what Oriani observed. That is my first post-electrolysic exposure; two more will be etched and analyzed in three or four days. But Oriani also saw tracks accumulating diring the electrolysis. Why did I not see them? That is a big puzzle.
Chip I: Chip on top of WET nickel for 90 hours after the electrolysis. 17.7 tr/cm^2 per hour. This is also comparable to what Oriani observed.

Chip J: Chip on top of the anode (spiral platinum pancake) for 90 hours after the electrolysis. 5.4 tr/cm^2 per hour. That was a surprize. My wire was not new; I will ask the electrochemist who gave it to me what was this wire used for before. In any case, I washed this wire and then kept it in the 1 M solution of HCl for 24 hours. Do I have to start wearing gloves while handling the cell components? I will ask Oriani if he looked for particles emitted from the anode.
Chip A:
Chip K: Chip exposed to the inner ("wet") surface of glass for 90 hours after the electrolysis. 18.3 tr/cm^2 per hour. Hmm, nearly the same as from the cathode. Is it a pure coincidence? The back side of that chip (exposed to air) had 0.9 tr/cm^2 per hr.
Chip L: Chip exposed to the outer ("dry") surface of glass for 90 hours after the electrolysis. 15.9 tr/cm^2 per hour. Hmm, nearly the same as from the cathode. Is it also a pure coincidence?
Chip M: Some tracks but not many.
Chip N: Some tracks but not many.

Experiment #2 (tracks collected during the electrolysis) cannot be trusted, as far as emission from the glass walls was concerned. That is because I was not sure which large chips were suspended from the insulated wire during etching. But this should not prevent me from saying that the average recording rates from glass (about 16 tracks/cm^2 per hour) were observed after the electrolysis. In other words, the glass situation is as puzzling as the nickel situation; the average rate of the recording after the electrolysis was higher than during the electrolysis (2.5 tracks/cm^2 per hour). How can it be? I also do not know why my average recording rate, below the cathode and during the electrolysis, was negligibly small in both experiments. This conflicts with what I observed in Minneapolis. It also conflicts with nearly 30 tracks/cm^2 per hour that Oriani recorded in the most recent experiment. The first time I failed to observe tracks below the cathode during the electrolysis was when the distance between the detector and the cathode was 1 mm. The second time I failed to see a lot of tracks was when the distance was 6 mm, the same as in Oriani's experiments.

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199) Nonsense, fraud or very advanced science?

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1) Extracted from <http://www.intalek.com/Products/Products.htm>

Products for sale:

DESCRIPTION: This high quality video details ALL of the amazing phenomena occurring in an area that's 300' in diameter in the beautiful wooded foothills of Santa Cruz California. ..... William Alek, a researcher and explorer, conducted experiments at the spot using the Explorers Kit shown below. He determined that time flows "slower" inside the spot by at least 3 minutes and 40 seconds per 24 day.....Because time flows slower, all of the metrics related to space and time change while walking through the spot. These metrics include volume, mass, frequency, gravity, time, and energy.

Mail Order:

Install the supplied software and attach the cable to your notebook computer, and your ready to explore!

PRICE: Call, prices range from $1000 to $1300

With this kit, you'll observe **RED SHIFTING** or **BLUE SHIFTING** of frequency generated by an Accutron Tuning Fork Watch. This tuning fork oscillates at a fundamental frequency of 360 Hz. . . .

2) Here is something that looks like nonsense, to me.

This is part of a much longer e-mail message received last night. The sender, who is a distant cousin and an architect, takes this text seriously. Is the author of the text a theoretical scientist (knowing much more than I do about modern physics) or is he a bluffer using scientific terminology to impress ignorant readers?

How fractal evolution works...

http://users.aol.com/leonmaurer/fixedtree.gif

... Is an interesting animation, related to ABC theory that describes how emanation of a dual entwined (positive-negative) ray (Superstring?) of Primal Force fractally involves radially (cyclically and vortically) out of the initial "spinergy" (or abstract motion) surrounding the absolute zero (laya) point -- to initially form the "coadunate but not consubstantial" fields of universal consciousness... That continues ("as above so below") in descending phase orders of frequency-energy, to eventually replicate into the seven fold fields of human consciousness. This also indicates why analogy and correspondence is a basic rule for understanding theosophical metaphysics or occult science. For diagrams

showing how this works, see:

http://users.aol.com/uniwldarts/uniworld.artisans.guild/chakrafield.html
http://users.aol.com/leonmaurer/invlutionflddiagnotate.gif

Science today -- while it is gradually approaching an understanding of multidimensional hyperspace fields (Superstring/M-brane theory, quantum field theory, etc.) that are analogous to the ABC fields, and is beginning to comprehend the relationship of the Aether (lower Astral field that, besides being the magnetic model of all forms of life, is also the medium of light) to the physical or metric fields of matter -- has still a long way to go before their wished for "Unified field Theory" (that baffled Einstein for more than 30 years) can become a scientifically proven reality. But, first, they have to solve the "hard problems" of explaining the "experience of consciousness" and "brain-mind binding" that is still baffling all of science -- even after the past 10 years of serious study as a recognized scientific discipline.

This won't occur until scientists realize that consciousness (as awareness and will or intent) is a universal aspect of fundamental reality related to the zero-point itself... That is everywhere (contiguous on the highest level of zero-point energy) and is the central origin of all fields of force... While being the basis of the so called "entanglement" or "action at a distance" between
fundamental quantum particles on any level. With this ultimate understanding they will also be able to synthesize information theory with relativity and quantum theories, along with recent theories of Aether physics, plasma electrodynamics, holographic paradigm, etc., and come up with an entirely new and synthetic scientific (physics, biology, cosmology, physiology, etc.) paradigm.
http://tellworld.com/Astro.Biological.Coenergetics

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My ABC (Astro Biological Coenergetics) theory postulates (predicts) that everything in the universe is fundamentally empowered by electricity and its laws of electrodynamics -- including all the hidden coadunate but not consubstantial hyperspace fields at progressively higher order spectrums (phases) of frequency/energy -- which is the basis of their coenergetic interrelationships and transfer of modulated magnetic wave carried holographic information between zero-point consciousness (awareness/will) and matter.

As a fundamental paradigm, it postulates that all substantial forms of this electromagnetic energy -- from the highest order hyperspace-time (spiritual field) through the six intermediate hyperspace fields, to the lowest order metric space-time continuum (our metric material cosmos of sidereal light) -- are directly involved and evolved out of the infinite "Spinergy" force or angular momentum of a zero-point-instant singularity simultaneously rotating at infinite velocity (Superspin) both clockwise and counterclockwise on at least three perpendicular axes... Thus, accounting for the spherical nature of the space-time continuum, its star forms and rotating galaxies. Also, from a quantum physics point of view, it accounts for both the positive and negative electro-gravity, electro-strong, electro-weak and electromagnetic forces... And, from a cosmological point of view, all particles and anti particles, along with both light and dark matter.

Thus, it predicts that the sun and all the stars are also electrical in nature... As is all the mass-energy composing all physical forms and organic bodies due to the vibrational patterns of the fundamental "superstrings" of primal electrodynamic force (emanating from the spinergy), and their derivative "strings," that determine the properties of the fundamental particles (wavicles). And, is the fundamental force underlying all their physical, chemical, biological, and physiological properties, interrelationships, and configurations. Also, it conforms with all principles of relativity in our sidereal space-time continuum, and considers their correspondence in each ascending phase order of hyperspace-time, modified by their varied time constants that determine their higher order light velocities (which would appear as instantaneous from our point of view).

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200) Scientific methods

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What follows are comments on the so-called “scientific method” in the context of teaching science. They appeared (in recent days) on Phys-L, a discussion list of physics teachers. The names of particpants were replaced by Professor 1, Professor 2, etc.

Professor 1:
. . . Please, let's stop taking things on faith. That phrase refers to believing things in the absence of evidence. That's the opposite of what science is. For a discussion of scientific methods in general, see

http://www.av8n.com/physics/scientific-methods.htm [See appendix below.]

Professor 2 (myself):
It is not possible to "stop taking things on faith" and to help students to master material in any science course. The laboratory discovery approach is extremely important but it is used to learn only a small percentage of what is in a typical textbook. Even scientists do not perform all experiments in their specialties; they often accept discoveries made (and published in refereed journals) by other scientists. How many high school teachers had an opportunity to experiment with scattering of alpha particles, as Rutherford and Geiger did in 1911? How many university teachers had opportunities to perform experiments through which existence of quarks was discovered in 1960s and 1970s? Not too many. We read about such experiments in reputable journals, and in textbooks. Then we accept what was discovered by others. And we teach it. Should we feel guilty? We trust that contents of science textbooks are verified by recognized authorities in relevant fields. Yes, I know, it is a complicated issue.

Professor 3:
P2, you have defined "faith" vary narrowly--as anything that we accept other than that which we have personally experienced. I see that definition as flawed for two reasons,

First, as you have cogently pointed out, we can't directly experience everything, but we can examine the evidence presented, as well as other corroborating evidence, and the consequences of the findings, and come to a pretty reliable conclusion about what is being asserted. Factored into our acceptance or non-acceptance of the claim has to be such things as the methodology of the experiment reported, the nature of the data presented (size of error bars, number of data points, etc.), the reputation of the investigator (for honesty, objectivity, integrity, and skill as an investigator), how it fits in with, or explains other related results, and what it predicts that can be tested and verified, and others that depend on the nature of the experiment and the methodology of the particular discipline.

And second, personal experience is a notoriously poor indicator of reality. We all know about experiments that showed the results the investigator wanted to see. N-rays are the classic example of that, but the Vienese laboratory that was looking at the energy distribution of beta-rays during the 20s and found evidence of definite energies of the electrons emitted in beta-decay, because that was what they were looking for, is another and somewhat more subtle example (see Andrew Brown's biography of Chadwick, "The Neutron and the Bomb," for more on this). And of course, there are all forms of hallucinations, flawed memories, and other non-events that take on a compelling reality to the observer. So to be able to say, "I saw it with my own eyes," is not necessarily as reliable as looking at the record of data taken and examining the experimental set-up, so see if there were any missed systematic errors built into it.

One of the things that makes science more reliable now than in past centuries is the existence of permanent, more or less objective recordings of what happened--chart recorder tapes, photographs, automated data files, etc., instead of just hand-written collections of manually taken observations. this allows others to see exactly the same thing that the original investigator saw, and so provide it with a more or less objective analysis.

One can argue that all of that is fine, but the farther one is from the original, the weaker is the connection. So the textbook or monograph reader pretty much has to accept what book's author says is the truth. And where does that leave the student?

To an extent, that is true, but that chain also provides much reinforcement of the result, since each person who passes the result along, has at least given it enough examination to decide that it is of enough importance and reliability to include, and that it fits into the overall picture the author wants to paint. Of course that doesn't rule out all of the human fallibilities that befall textbook writers, and which can make almost anything is a textbook wrong. Keeping textbooks "honest" is a continuing process, and students need to know this.

Of course the original result can still be wrong, or incomplete, or not sufficiently accurate to account for subsequent results, or any of a number of things that can happen to experimental results over the years. But the big difference between scientific, or evidence-based conclusions and faith-based conclusion is that they are much more subject to change in the face of new evidence.

The faithful say, "I hear what you have said, and it's good enough for me. I believe." The scientist says, "OK, that sounds reasonable, and it fits with the accepted theories (or it explains something that current theories don't), so I'll take it and use it until I see evidence that it wasn't correct, in which case I will change my mind." I think there is a huge difference between these two stances.

Appendix - Scientific Methods (Professor 1 above):

Many textbooks and web-sites describe “the scientific method” in terms most scientists find objectionable. Here is an attempt to do better.

1. There is no such thing as "the" scientific method. Science uses many methods. There will never be a pat answer to the question "what is science". The very notion that there could be a pat answer bespeaks an attachment to rote learning that is incompatible with scientific thinking.

2. One of the goals of science is to make useful predictions.

3. A scientific prediction does not need to be exact to be useful.

4. Sometimes it is possible to make useful predictions, and sometimes not. If you are asked to predict the exact total shown on a particular roll of a pair of fair dice, you will be wrong at least 5/6ths of the time. But if you can get into a situation where the payoff is greater than 6:1, you can make some useful predictions, and you can make money on average.

5. Scientists use words like rule, law, equation, identity, principle, formula, algorithm, etc. almost interchangeably, to describe the process for making predictions (although there are slight variations in connotations).

6. The word “theory” can be used in two radically different ways. The first usage means something like law or rule, only much grander, namely a *system* of rules giving a coherent description and explanation of a topic. The other usage refers to a a mere speculation. Remarkably, both versions are correct, and both have been in use for over 2000 years. It is best to avoid the word entirely when talking to non-scientists, and especially when debating with persons who can’t be trusted, since if you intend one meaning they’ll use the other meaning against you. (It sure would be nice to find a word that expresses the idea of “coherent description and explanation” without risk of misunderstanding.)

7. Mathematical results are validated by formality and rigor. This gives us logical statements of the form “If *A* then *B*”

and suchlike. Physical-science results are sometimes validated by logic, but may also be validated by appeal to experiment. This gives us statements of the form "We observe *A*" and suchlike. Generally science is a complex lattice of facts and rules, combining observations and logic.

8. Predictive rules generally have a limited domain of applicability. To state the rule without stating its limits of validity is improper.

9. From time to time, an established rule may be refined. It may be supplemented by other rules so as to extend the domain of validity. It may be supplemented by exceptions to improve the accuracy. However a rule with too many caveats and exceptions is likely to be not only inconvenient but unreliable. Occam's razor and all that.

10. From time to time, a rule may be supplanted entirely by a simpler and better rule.

11. It is considered very poor form to gripe about the imperfections in an established rule, unless you've got something better to offer.

12. Creating new rules from scratch is exceedingly difficult. There is an infinite number of possible rules, and you will never have enough data to decide which of the contenders is best -- unless there is some sort of additional guidance. Sometimes guidance is taken from intuition and from notions of "simplicity" or "elegance". This is bordering on metaphysics, but it is an important part of science.

13. Scientists, like business executives, government leaders, and everyone else, must often make decisions based on highly incomplete data. The important thing is to be able to change your mind as soon as you get new data that contradicts old hunches. This requires keeping score on each of the rules, keeping track of which are well-supported by existing data, and which are least-well-supported and therefore most subject to revision.

14. An important scientific activity (which applies not just to pure science but also to engineering and even farming, etc.) is designing a series of measurements that will tell you what you need to know, without undue waste. See section 2 for more on this.

15. An important part of scientific thinking is being able to recognize non-scientific thinking. Examples include:
* Elementary logic errors, such as circular reasoning, non sequitur, and many others.
* Selecting the data. (It is not right to select tendentious anecdotes from a mass of data.)
* Other misuses of probability.
* Proof by bold assertion. (It's OK to assert something, so long as you don't pretend to have proved anything thereby.)
* Appeal to authority (as discussed in reference 4).
* *Ad hominem* arguments.
* Improperly weighted voting. (A thousand pieces of weak evidence should not outweigh one piece of strong evidence, as discussed in reference 4.)
* Et cetera.

2 Design of Experiment

Consider the famous *Twelve Coins Puzzle* as discussed in reference 6. Suppose you find a casino that is willing to pay you $350 for identifying the odd coin, but makes you pay $100 for each weighing. If you weigh the right combinations of coins, you can do the job in three weighings, so you make money every time. In contrast, if you follow a sub-optimal strategy that requires four or more weighings, you will lose money on average.

This scenario is reasonably analogous to many real-world situations. Commonly there's a significant price for making a measurement, and you want to maximize the amount of information you get for this price.

I mention this because all too often, people claim that a principle of scientific experimentation is to "change only one variable at a time". It's easy to see that such a claim is hogwash. The Twelve Coins Puzzle suffices as a counterexample. If each weighing differs from the previous weighing by only one coin, you cannot come anywhere close to an optimal solution.

The suggestion to “change only one variable at a time” might nevertheless be good advice in some special situations. That’s because the cost of making a measurement is not always the dominant cost in the overall information-gathering process. For example, imagine a situation where gathering the raw data is very cheap, while just plain *thinking* about it is expensive. Then you might want to follow a strategy, such as changing only one variable at a time, that makes the data easy to interpret, even though you had to do large number of experiments (much larger than theoretically necessary). Consider the contrast:

For young children doing cheap, simple experiments, it might make sense to tell them to change only one thing at a time, because the rate-limiting step is interpreting and understanding the data, and we want to make that step as easy as possible. For skilled scientists (and engineers, farmers, etc.) doing complex, expensive experiments, changing only one variable at a time would be an unnecessary burden, and often a disastrous burden.

Changing only one variable at a time is a crutch, which may partially compensate for the investigator’s lack of skill in interpreting the data. In contrast, for performers with ordinary ability and training, crutches are harmful, not helpful.

3 Correctness and Modesty

As mentioned above, a major *purpose* of scientific methods is to make useful predictions and to avoid mistakes. The known scientific methods are a collection of guidelines that have been found to work reasonably well. One of the most important steps in avoiding mistakes is to always keep in mind that mistakes are possible. This is so important that this whole section is devoted to emphasizing it and re-expressing it in assorted ways. James Randi said you should take care not to fool yourself, keeping in mind that “the easiest person to fool is yourself”.

It is OK to a limited extent to be an advocate for your favorite idea, but you must not get carried away. When you collect data in support of an idea, you must also look just as diligently for data that conflicts with that idea. Then you must weigh all the data fairly, and disclose all the data when you discuss your idea. This is what sets science apart from debating and lawyering, where advocacy is carried to an extreme, and it is considered acceptable to skip or make light of data that tends to support the “opposing side”. Another word for this is modesty. Being aware of your own fallibility is modest. Pretending you are infallible is immodest.

A related form of modesty, which is also crucial for avoiding mistakes, is to not overstate your results. Scientists use certain figures of speech that are designed to avoid overstatement. Among other things, this includes recognizing the distinction between data and the interpretation that you wish to place upon the data. As an illustration, imagine some children go on a field trip to the dairy. Upon their return, they write a childish report that says “cows are brown” -- or, worse, “all cows are brown”. A more modest, scientific approach would be to say “the cows we observed were all predominantly brown”. A statement about the observed cows sticks closely to the data, while a generalization about all cows requires a leap beyond the data.

As mentioned above, practically all scientific results have some limits to their validity, and you must clearly understand and clearly communicate these limits.

4 For Further Reading

a. Richard Feynman, **The Character of Physical Law**
b. Thomas Kuhn, **The Structure of Scientific Revolutions**
c. Richard Feynman, **The Pleasure of Finding Things Out** especially the chapter *Cargo Cult Science*.
d. Valid versus Invalid Arguments: Appeal to Authority etc. ./authority.htm
e. “Truth in Contrast to Knowledge and Belief” ./truth.htm
f. “The Twelve Coins Puzzle” ./twelve-coins.htm

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201) A “nuclear reactor“ in a garage?

Ludwik Kowalski (3/7/05)
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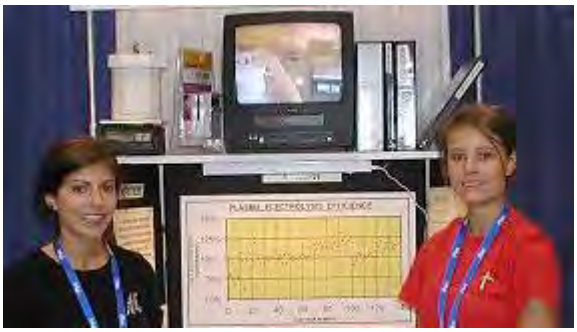
In the unit 170 I wrote: “. . . Another thing that impressed me was that one of the teams that confirmed Naudin’s findings consisted of highly trained electrochemist. On the other hand, I was slightly disappointed that the identity of the US laboratory, in which the confirming calorimetric experiments were conducted, was not specified. The document shows the photo of two researchers from Louisiana, [see below] and their first names (two nice looking young girls, probably students), but not the name of the laboratory, or the name of the project director. Was it a high school project or a projects at a well known university? Such information is important when controversial results are presented. Why was it not provided by Naudin?”



These students, X and Y, prefer to remain anonymous (see below).

Two weeks ago one of these girls, Y, sent me an email message. She wrote: “This project was not done at a laboratory. This was a Science Fair Project that Y and I did last year. The picture on Naudin’s website was actually taken at the 2004 International Science and Engineering Fair in Portland, OR. This was a team project. Y, being a senior, has now graduated, and I am continuing the project this year with the following changes:

1) A much more accurate scale, calibration checked at a local chemical plant lab.
2) Using Potassium carbonate rather than Sodium Hydrogen carbonate.
3) A completely sealed system to condense the steam and calculate the Hydrogen generated.
4) A new reactor (thermos design. mist eliminator, stainless internal used as the anode
5) Digital thermometer calibrated with distilled ice and boiling water
6) Input power readings taken with digital watt meter every 15 seconds.

. . . Last year our best was 147%, with an average of 117%, including our learning curve. This year the average is 143%, with the best being 271%. Being a senior, this is now my last year….but if I were to continue further, I would try the experiment under higher pressure – like 100 psi or so, to look for improvements.

This work was not done at a lab – it was done at my house, in the garage. Most of the items needed to build the power supply were purchased on eBay… Sounds dangerous….two high school girls build a nuclear reactor in their back yard… Finally, should anyone write to you for advice about a cold fusion project, prepare them for the amount of skepticism that they will be subject to with that kind of work. At Internationals, the worst thing we heard was from what we thought would have been the most educated judges…but they were not very accepting. More than once we heard “If it would have worked, you would be dead.

Y
Senior,Catholic High School
New Iberia, Louisiana

It is good to know that the spirit of scientific and technological adventure did not disappear; I am thinking about Marie Curie and Thomas Edison. But I am concerned about safety; the power supply they have (200 volts and up to 10 A) can easily kill a person. Such experiments should be conducted in school laboratories and be supervised by knowledgeable teachers. I sent Y a description of a recent accident with a similar setup in Japan (Mizuno's lab) but she did not comment.

Responding to the above I wrote (on 2/7/05):
1) I was very happy to receive your message; thanks for contacting me. I am also glad that you continue working on the project. But are you taking appropriate precautions in working with a very dangerous electrical power supply, and with hazardous chemicals? Is your work supervised by a trained person? I hope your teacher (or teachers) are there to prevent bad things from happening in the garage. Did you ask for permission to conduct the experiment at school?

2) I am not a chemist. Can you explain to me why are you changing an electrolyte that was delivering positive results?

3) I would not worry about the limited accuracy (0.2%) of the watt-meter when the average excess power is 43%.

4) The big challenge in this field is to convince others, including teachers and competition judges, that the anomaly is real. This is a very difficult task, considering the effects of negative publicity.

5) After retiring last year I decided to find an experiment that would be convincing to all honest skeptics. For that reason I contacted Dr. Richard Oriani (a retired chemist from the University of Minnesota) and we are working together now. The goal is to develop a simple setup that gives 100% reproducible results. Oriani's electrolytic cell is much smaller than yours; it is receiving electric energy at the rate of several watts. That is sufficient to observe nuclear particles; we are not focusing on excess heat, as you are.

6) After visiting Oriani, to work with him for one week. After that I performed ten experiments in my own home. Unfortunately, the goal of 100% reproducibility (ability to get an observable effect from each experiment in any laboratory) has not yet been achieved. But we are not giving up; this work is in progress.

7) I do believe that your excess heat, and excess heat in Naudin's experiments, are real. But that is not the main issue. The issue with all these experiments, as far as I can tell, is absence of evidence that the origin of excess heat is nuclear. One has to perform a complete analysis of all reactions to rule out chemical origin of excess heat. Neither Naudin nor you performed such analysis, as far as I know. Oriani also worked on excess heat experiments but decided (for the time being, I suppose) to focus on nuclear particles.

8) When will your next experiment be performed? I would be happy to send you several detectors of nuclear particles, if you want. They are small plastic chips (very clean polycarbonate) to be placed above the electrolyte, and in the electrolyte. After the experiment you would send the chips back to me and I would analyze them. If we see particles then we will try to repeat the experiment several times. Perhaps your large cell will become a simple setup needed to convince honest skeptics. If so we will try to publish these results in a mainstream journal.

9) Where is X now? Are you still collaborating on this project? What do you plan to do after the graduation?

That is how our correspondence started. Y will apply the chips to the electrolyte and to the electrodes this Sunday. The exposure will be three days. As usual, I want to be sure that nothing is radioactive before the main test. While waiting let me describe her setup. It is Naudin's "cold fusion reactor" with some modifications. Imagine a beaker with two electrodes, for example, both made of tungsten. The current flowing through the electrolyte, between 5 and 10 A, is sustained by a d.c. power supply of about 200 V. Intensive arcing and sparking is taking place in the liquid and the cathode is consumed rapidly, for example, after five to ten minutes. The claim is that the electric energy supplied to the

cell, Ee, is smaller that the energy released in the cell, Et. The difference, Ex=Et-Ee, is called excess energy; it is mostly thermal. The ratio of Et/Ee, is called efficiency.

The average efficiency reported by Y (see above) was 1.47. It means that for every 1,000 J of electric energy the cell releases 1,470 joules of heat. The origin of Ex (470 J) is assumed to be nuclear but no evidence for this is provided. The CR-39 chips will be used by us to explore a possibility that nuclear particles, similar to those discovered by Oriani, are emitted. That could become the needed evidence validating Naudin's claim. Ignoring that aspect let me focus on details of excess heat measurements. The value of Ee is determined by Y as the sum of many P*dT terms, where P are power readings of a watt-meter and dT is the time interval (15 seconds) between consecutive readings. One of Y's experiments lasted 3 minutes and the value of Ee, delivered during that time interval, turned out to be 87,780 J.

The efficiency of this experiment was reported as 1.92. This implies that Et was found to be 1.92*1,470 = 2,822 J. How was Et measured? It turns out that Et consists of several components; I will label them as E1, E2, E3, E4, E5, etc. The E1 is the energy needed to increase the temperature of the electrolyte to the boiling point, E2 is the energy used to evaporate m1 grams of water, and E3 is the energy used to decompose m2 grams of water (bubbles of oxygen and hydrogen escaping from the cell). It is well known that 2,260 J are needed to evaporate each gram of water and that 13,174 J are needed to decompose each gram of that liquid. Thus, in the first approximation:

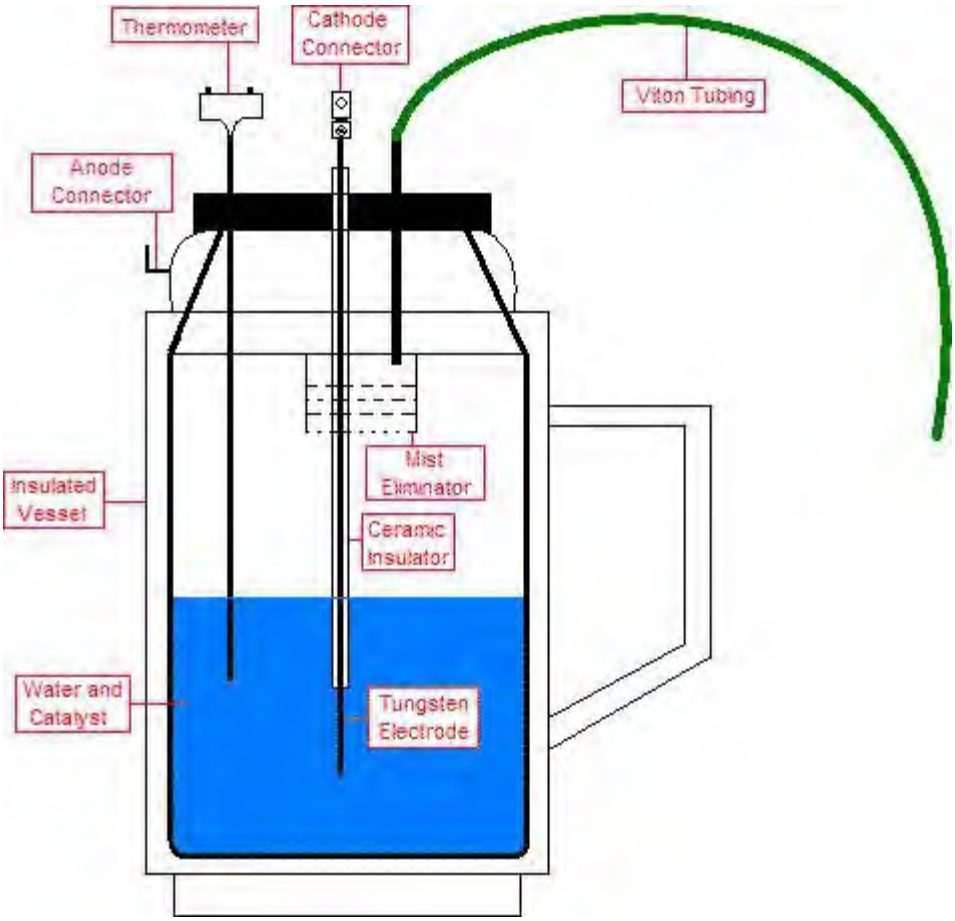
Et = E1+E2+E3 = s*(T2-T1) + 2,260*m1 + 13,174*m2

where T1 is the initial temperature, T2 is the boiling temperature and s is the heat capacity of the calorimetric vessel. The first term, E1, becomes negligible when the electrolyte is preheated before the current is turned on (to make T1=T2). In Y's experiment, however, (T2-T1) was 44 C and the value of s was found to be 116.4 J/C. That mean value of s was determined by Y from eleven preliminary experiments.

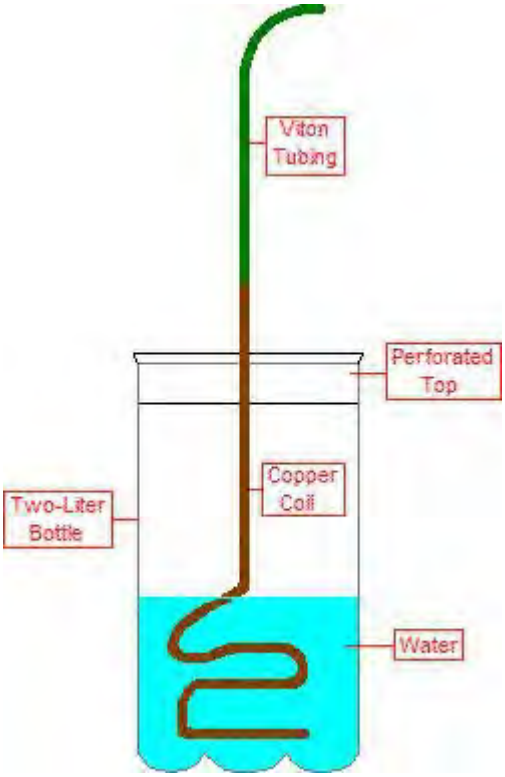
In many Naudin's experiments the vessel was an open beaker. Knowing that m2<<m1 (ignoring E3) Naudin measures the mass of the vessel before and after the electrolysis. The difference, identified as m1, was then used to calculate E2. An interesting innovation, introduced by Y, was to determine m1 and m2 separately. (In Naudin's experiments water vapor, oxygen and hydrogen escape into air and the measured mass difference is actually the sum of m1 and m2). I will return to this subject a little later. Using the above two equations one has:

Ex = (E1+E2+E3+E4) - Ee = [s*(T2-T1) + 2,260*m1 + 13,174*m2 + E4] - Ee

In this equation E4 represents the amount of thermal energy lost by conduction and convection during the electrolysis. It is interesting to note that by ignoring E4 one makes a systematic error of underestimation of Ex. In other words, the values of Ex reported by those who ignore E4 are smaller than the true values. Y's calorimetric vessel is a nearly sealed thermos, as illustrated in Figure 2. In that way the value of E4 is reduced significantly, in comparison with what it would be in an open beaker.



Sealed thermos. The green tube connects to the condenser below.



Condenser; the top is exposed to the atmosphere.

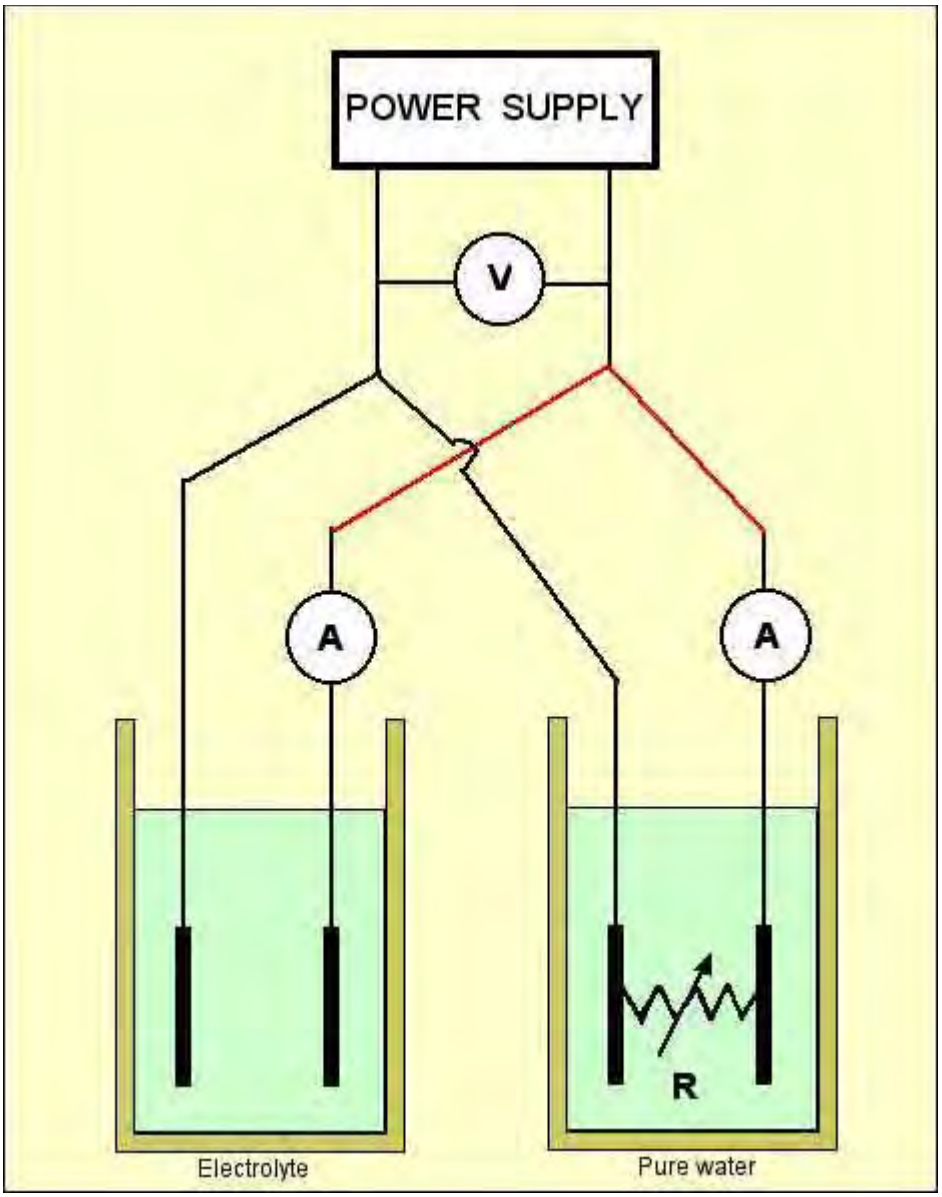
The vapor and gasses escape from the thermos through a flexible tube connected to a second container. That container,

called condenser, is filled with cold water. The exit from the flexible pipe is under cold water. Bubbles of hydrogen and oxygen escaping from the exit are released into the atmosphere. The water vapor, on the other hand, condenses and remains in the container. Both vessels, the thermos and the condenser, are standing of sensitive electronic scales. The change in the mass of the condenser is interpreted as m1 while the change in the mass of the thermos is interpreted as m1+m2. That is how the values of m1 and m2 are measured by Y. In the experiments in which the efficiency was 1.92 the values of m1 turned out to be 35.7 grams while the value of m2 turned out to be 0.6 grams. Showing that m2 is indeed much smaller than m1 has a great pedagogical value; most students are not aware of that fact.

Y also wrote to me about another complication. It has to do with vapors that condense in the thermos, for example, on its walls and near the exit to the flexible pipe. Suppose that 5% of vapor condenses inside the thermos. This has two important consequences: (a) the value of E2 is underestimated by 5% and (b) energy released through condensation remains in the thermos. Y tried to minimize condensation of vapors inside the cell but I am not convinced that she was successful. She wrote: "If you look at the attached picture you'll see a "sink strainer" where the insulator comes thru the thermos top. This is actually two sink strainers glued together with JB Weld epoxy. I got these at Wal-Mart. Between the two strainers I have three layers of 100 mesh stainless steel screen.

Once I put the preheated solution into the thermos and close it up, I let it set for a while for everything to come to an even temperature. This takes about a minute or so for the temp to level out. I ASSUME the mist eliminator is about up to temperature for very little condensation to occur at this point in the vessel ... although I'm sure there's some dripping back into the bottom of the reactor from here."

If I were asked to measure the Ex accurately I would not use a condenser. My setup would consist of two open cells, as illustrated below. The left beaker contains the electrolyte, the right beaker contains the same amount of distilled water and an ohmic rheostat immersed in it. The current in the left cell is ionic while the current in the right cell is electronic. The variable resistance of the rheostate would be chosen to make the two currents equal. To impose the right side current of 10 A at 200 V, for example, the resistance would have to be 20 ohms. To change the current to 5 A, also at 200 V, the resistance would have to be increased to 40 ohms. Note that the applied voltages are essentially identical because cells are connected to the same power supply. It would not be difficult to adjust the reostate manually during an experiment lasting several minutes. One would watch the current of the left ammeter and change the resistance, by as much as necessary, to match it with the current through the right ammeter. In that way the Ee would be identical in two cells.



A schematic diagram of a setup with two cells.

The sizes, of two cells, their masses, and their thermal capacities would be essentially identical. It is well known that a current flowing through an ohmic resistor (in the right cell) generates no excess heat. Therefore, the difference between E2 in the left cell and E2 in the right cell, if any, would be eqaul to Ex. Note that absence of information about E1, E3 and E4 would not prevent me from the determination of Ex. The same idea could be implemented with two cells connected in series. In that case I would use one ammeter and two voltmeters, one for each cell. The reostate would be adjusted to keep two voltages identical (to make sure that Ee are identical).

Let me end this essay by saying that I was highly impressed by what the two high school girls did in a Louisiana garage. It is a pity that "educated judges" were so skeptical when Y and X presented their outstanding work at an international competition. The judges probably thought that the project was pseudoscientific and not worth their attention. I would expect them to focus on what was positive in this explorative work. What is wrong with a student project trying to prove, or disprove, a controversial claim? What can be a better way to expose students to the excitement of scientific research than working on such projects? I am glad that Y decided to contact me and excited about our pending investigation of a "nuclear signature" in her setup. Information about that investigation is going to be appended below.

Appended on 2/18/05:
To concentrate on condensation inside the vessel let me consider a hypothetical example. An open jar, containing an electrolyte, stands on a scale; in that way the total mass can be measured at any time. The electrolyte is brought to the

boiling temperature (by warming it on a hot plate or by passing an electric current through it). The first measurement of P is made at an arbitrary moment after the boilimg started. Consecutive values of P are recorded avery 15 seconds, as in Y's experiment. Suppose that the current was turned off after 5 minutes. The amount of water, m, lost during that time was 50 grams. The value of Ee, calculated from consecutive recordings of P, was 80,000 J. Ignoring the mass the escaping hydrogen and oxygen the value of Et was calculated as 2260*50=113,000 J. The excess heat, and the efficiency, are then calculated as Ex = 113,000 - 80,000 = 33,000 J and ef=113,000/80,000=1.41, respectively.

The jar was not transparent and I assumed, in the above calculation, that no vapor condensed on the walls during the electrolysis. Suppose somebody tells me that 10 grams of vapor condensed and returned to the boiling electrolyte. My task is to recalculate the Ex by taking this into account. The first step is simple; I change the evaporated mass from 50 to 60 grams. The corrected value of Et becomes 2260*60 = 135,600 J. The difference between this value and Ee is 55,600 J (instead of 33,000 J above). This is 22,600 J larger than above. The reason for this is that thermal energy is released in condensation. The latent heat of condensation is the same as latent heat of evaporation. This means the 2260*10=22,600 j of thermal energy was released.

Does it mean that the corrected value of Ex should be 135,600 - 80,000 = 55,600 J? Not necessarily. That answer would be correct only if the thermal energy released through condensation went entirely to the surrounding air. That is one possible extreme. The other extreme is when all the released energy goes back to the electrolyte. In that case the Ex would be 135,600 - 80,000 +22,600 = 78,200 J. Why was the condensation energy added and not subtracted? Because that energy, like Ee, enters the system. In reality, however, only a fraction of 22,660 J enters the system; the remaining part goes into the air. Suppose that the unknown fractions are 50% each. In that case Ex would be 567,300 J. Unfortunately, it is very difficult to determine how the released energy is divided between the outside air and the inside electrolyte. This shows how much uncertainty is introduced into the calculation of Ex when condensations of vapors takes place inside the electrolytic cell. I think that Y recognized this fact and tried to reduce the uncertainties by using a condenser.

Appended on 2/19/05:
It is reassuring to know that by ignoring condensation of vapor inside the electrolytic cell, like ignoring heat losses due to conduction and convection, leads to an underestimation, not to an overestimation, of excess heat. The values reported by Naudin and others are most likely smaller than the true values of excess heat. But what is the origin of that heat? That is still an open question.

In most excess heat research Ex is generated at the rates of watts or fractions of watts. In Naudin-like experiments, on other hand, Ex is generated at the level of hundreds of watts. Tracing chemical reactions taking place in high power cells is likely to be more difficult than in low power cells. Nuclear signatures, by contrast, might be proportional to the cell power. If this is true then discovering nuclear particles in high power cells might be easier.

Appneded on 3/1/05:
Y sent me back the CR-39 chip that were exposed (for three days) to her electrodes and her electrolyte. These electrodes were to be used in our anticipated experiment. The chip exposed to the tungsten cathode had a lot of tracks. I suspect that tungsten rods are welding electrodes containing about 2% of thorium. Such electrodes were widely used in the past, probably to ionize air. The chip that was exposed to tungsten had thousands of tracks, even more than my control chip exposed to an alpha source. Informing Y about this finding I wrote: "Your tungsten has alpha-radioactive material in it; you can probably detect it with a Geiger counter in your school. I suspect it is thorium or uranium. We can not go ahead with the suggested experiment, unless you can use another cathode. Let me know." In the next message I added: "In reading Naudin's descriptions again I see that stainless steel could be used as a cathode. I would be surprised to find a radioactive material in a spoon or a knife made from this alloy. But the new material would have to be checked, if you decide to continue working on our project."

Appended on 3/7/05:
This web page contained true names of students. But I did not want to share this information over the Internet without asking for permission. I am glad I asked. The reply, received this morning, was: "We would prefer to keep our last names out of the reports. You never know these days". That explains why the real names were replaced with X and Y. Additional information about this project will be appended, if Y decides to continue.

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202) Fraudulent claims of Professor Reiner Protsch von Zieten

Ludwik Kowalski (2/22/05)
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A colleague posted an article about fraudulent claims made, over the last 30 years, by a German anthropologist from the University of Hamburg. His claims were taken for granted until very recently. The story was reported on 2/19/05 in the Science News section of the "Guardian Unlimited," . Why am I posting extracts from this article on the website devoted to Cold Fusion? Because that field has often been described as pseudoscientific. The "Voodoo Science" book of Robert Park gives many examples of fraud. That book, and its Russian counterpart (by Krugliakov) give the impression that cold fusion claims are also fraudulent. That is not based on specific cases of "invented data"; the authors simply fail to make a distinction between cold fusion researchers and real con artists.

I think that fraudulent claims are more likely to appear in the area of accepted science than in the area of controversial science. What can a person gain by faking cold fusion data? All claims in that area are nearly automatically rejected. The only effect would be to impress a relatively small group of other CF researchers. On the other hand, absence of access to peer reviewed journals might encourage fraud. Do the editors of professional journals realize that their attitude toward cold fusion can encourage con artists and charlatans?

* * * * * *

It appeared to be one of archaeology's most sensational finds. The skull fragment discovered in a peat bog near Hamburg was more than 36,000 years old - and was the vital missing link between modern humans and Neanderthals.

This, at least, is what Professor Reiner Protsch von Zieten - a distinguished, cigar-smoking German anthropologist - told his scientific colleagues, to global acclaim, after being invited to date the extremely rare skull. However, the professor's 30-year-old academic career has now ended in disgrace after the revelation that he systematically falsified the dates on this and numerous other "stone age" relics..... According to experts, his deceptions may mean an entire tranche of the history of man's development will have to be rewritten. "Anthropology is going to have to completely revise its picture of modern man between 40,000 and 10,000 years ago," said Thomas Terberger, the archaeologist who discovered the hoax. . . .

During their investigation, the university discovered that Prof Protsch, 65, a flamboyant figure with a fondness for gold watches, Porsches and Cuban cigars, was unable to work his own carbon-dating machine. Instead, after returning from Germany to America, where he did his doctorate, and taking up a professorship, he had simply made things up. . . . "Prof Protsch's work appeared to prove that anatomically modern humans and Neanderthals had co-existed, and perhaps even had children together. This now appears to be rubbish." The scandal only came to light when Prof Protsch was caught trying to sell his department's entire chimpanzee skull collection to the United States. . . .

Other details of the professor's life also appeared to crumble under scrutiny. Before he disappeared from the university's campus last year, Prof Protsch told his students he had examined Hitler's and Eva Braun's bones..... Even the professor's aristocratic title, "von Zieten", appears to be bogus. Far from being the descendant of a dashing general in the hussars, the professor was the son of a Nazi MP, Wilhelm Protsch, Der Spiegel magazine revealed last October. The university is investigating how thousands of documents lodged in the anthropology department relating to the Nazis'

gruesome scientific experiments in the 1930s were mysteriously shredded, allegedly under the professor's instructions. . . . Yesterday the professor, who lives in Mainz with his wife Angelina, didn't respond to emails from the Guardian asking him to comment on the affair. But in earlier remarks to Der Spiegel he insisted that he was the victim of an "intrigue."

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203) Proto-science is not pseudo-science

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Irreproducibility, as we know, is typical in the field of cold fusion. I like to compare the situation with irreproducibility that was probably frustrating investigators of electrostatic phenomena before the role of humidity was recognized. Other comparisons are often mentioned, for example, the effect of impurities on performance of early transistors (before it was recognized that 99.99% purity of raw material might not be sufficient.) Failures probably resulted from hidden uncontrollable factors, such as sneezes, or from not washing hands.

Nuclear phenomena triggered but chemical activities remain irreproducible, as far as I know. Unfortunately, those who investigate such phenomena are often treated as pseudo-scientists or even can artists. This is regretable; existing controversies would have been solved more rapidly if "cold fusion" was treated as any other field of study. But let's face it, a field of research is not scientific unless at least something is 100% reproducible, or unless the irreproducibility is understood. Far from being pseudo-scientific the field is active and, as far as I know, its major player are well qualified and honest. Therefore it deserves to be labeled as "proto-science."

The basic idea in the field of CF, as mentioned above, is that "a chemical process can trigger a nuclear process." That controversial idea conflicts with everything we learn from existing textbooks. Yes, excess heat is an important technological issue. But the major controversy is not about the heat, it is about its nuclear origin. Numerous cold fusion claims have been made but the progress is slow because research is not coordinated. If it was up to me I would ask scientists to select one or two irreproducible phenomena and focus on them. But that is not what is happening. Instead of seeing many scientists studying a selected cold fusion phenomenon, such as emission of alpha particles, or large shifts of isotopic ratios, I see that each researcher does something different.

Excellent instruments to study nuclear particles, and isotopic ratios, already exist in numerous laboratories, all over the world. Therefore controversial claims in these areas would quickly be recognized as either valid or not valid if a research-supporting agency, for example, NSF, or CERN, promoted the idea of focusing on one or two claims. How much would it cost to perform ten or twenty reliable measurements of isotopic ratios (using cathodes supplied by CF scientists) in several independent laboratories? Probably not longer than one month, including preparations and testing of ion sources. Likewise, emission of charged particles, reported by many investigators (Jones, Lipson, Karabut, Oriani, etc.), could be quickly confirmed or not confirmed by scientists from several laboratories. To study Oriani effects (see item #192), for example, a single Si detector, or a dE-E setup of two such detectors (in a sealed container with a thin window), would be placed above the electrolyte, in the electrolyte and below the cathode. Showers of particles leaving tracks in CR-39 detectors should be easily observable by electronic means. Track detectors show cummulative effects, electronic detectors should allow observations of particles at the time of emission.

This approach, to solving a controversy, would be more natural than counting how many appointed panel members "voted" for or against various claims. The recent DOE panel report makes no reference to experiments. I still do not know why the names of panel experts were removed from their individual repotrs (see item #196).

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204) An Israeli Connection

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Israeli industrial researchers participated in both cold fusion conferences that I attended: ICCF10 and ICCF11. They did not represent well know research centers, like Weisman Institute or Technion, they came from a small private company called Energetics Technolpgies. Here is how the company activities were categorized in August 2003 by I. Dardik et. al. (ICCF10):

"Energetics Technologies (ET) was recently established to investigate possibilities for generating energy from Low Energy Nuclear Reactions (LENR) using a new approach of wave excitation. The new approach involves use of so-called "waves-waving-waves" or Superwaves for driving the processes that generate LENR. Four experimental approaches are being pursued: electrolysis (EC), glow-discharge (GD), gas loading in catalyst cells (CC) and high-pressure high-temperature cell (HPTC) with ultrasonic wave excitation."

And here is how their work was described in the January 10 issue of the New Energy Times <http://www.newenergytimes.com/news/8.htm>

Two groups who are relatively new to the ICCF conference series gained a significant amount of attention from the cold fusion veterans. The first was the American-Israeli team of Energetics Technologies. The Energetics Technologies Ltd. laboratory is located in Israel and is a wholly owned subsidiary of Energetics Technologies LLC in the United States. . . . [The company] presented data from its three best electrolytic cold fusion experiments at the conference:

- Run #56 gave 80% excess heat over a duration of 300 hours with a total excess energy of 3.1 Megajoules. [generating heat at the mean rate of 29 W]
- Run #64a gave 2500% excess heat over a duration of 17 hours with a total excess energy of 1.1 Megajoles. [generating heat at the mean rate of 17 W]
- Run #64b gave 1500% excess heat over a duration of 80 hours with a total excess energy of 4.6 Megajoules. [generating heat at the mean rate of 17 W]

[At the ICCF11] Ehud Greenspan of Energetics Technologies provided the following four points to summarize other key aspects of their claims:

'1. Excess energy generated was more than 25 times higher than the input energy. The next highest excess energy reported in ICCF-11 was obtained by Roger Stringham.

2. The remarkable excess-energy to input-energy ratio reported by Energetics Technologies has been obtained at relatively high power levels of several tens of watts and lasted for tens of hours.

3. It is instructive to compare the excess-energy obtained by Energetics Technologies to that of Mizuno quoted by Jed Rothwell as a remarkable achievement -- 84 MJ from 100 g of Pd. These numbers correspond to a "specific excess-energy" of 0.84 MJ per gram of Pd. The specific excess-energy obtained in the Energetics Technologies experiments reported at the ICCF-11 was ~20 MJ/g, i.e., more than 20 times higher.

4. The highest "specific-power" obtained in Energetics Technologies experiments -- ~70 watts per gram of Pd, is higher

than the average specific-power commercial fission reactors are operating at between 20 to 50 watts per gram of uranium.'

. . . Energetics Technologies and Mitsubishi may be the two most well-funded groups performing cold fusion research in the world. The angel investor behind the company is Sidney Kimmel, . . . The greatest challenge they face is reproducibility. Certainly, their claims of excess heat and energy appear to be among the highest ever seen in cold fusion experiments. The Holy Grail of clean nuclear power has not been obtained, however, because they have not seen high reproducibility with their work. If they can find a way to readily reproduce 2500% excess power, commercialization for electrical power generation is not far behind. But unlocking the secrets of nature to get to that point will not be an easy task..... "

Conference presentations of Israeli scientists -- they can be downloaded from the library at <http://www.lenr-canr.html> -- are very impressive. The most spectacular successes reported by them (see above) were obtained in electrolytic cells whose electrodes were prepared in close cooperation with a team of Italian scientists from the Frascatti Research Center. The current flowing through an electrolytic cells, or through a glow discharge chamber, was a "superwave," a mixture of several low frequency components. Browsing the Internet I found the following description of superwaves (by Arik El-Boher at a Frescati seminar on April 22, 2004):

"Energetic Technologies (ET) was recently established in Omer (Israel) to investigate intensification of Low Energy Nuclear Reactions (LENR) using excitation by SuperWaves©. SuperWaves is defined as a low frequency carrying wave with several successive stages of amplitude and frequency modulation – waves waving within waves. All stages of modulation are interrelated non-linearly. The inventor of the SuperWaves – Dr. Irving Dardik – has experimentally proven that these very special wave patterns are efficient in accelerating and intensifying different processes in metallurgy, biochemistry, health and other fields."

At the same Frescati seminar, entitled "Nuclear reactions through superwave excitation," El-Boher reported that "a significant amount of excess heat was measured in the first glow discharge experiment. The power generated during the experiment was up to 3.9 times the input power. When driven with SuperWaves the excess heat was higher than when driven with DC. A significant amount of excess heat was also measured after the shutdown of the cell, lasting for approximately 10 hours following the cell shutdown." Will the reported effectiveness of superwaves be confirmed in other laboratories? This remains to be seen.

The primary interest of the Israeli company, as far as know, is excess heat; I am not aware of their attempts to identify the origin of that heat. The essence of the cold fusion controversy, on the other hand, is not excess heat, it is its nuclear origin. I mentioned this recently to somebody who reported generation of excess heat at rates exceeding 100 watts. The answer was typical: "Since you can't have more than 100% efficiency - it must be nuclear." No, it does not have to be nuclear; it might be something else. Let me say, however, that emission of nuclear particles from a glow discharge chamber has been reported much earlier, as described in the unit #13.

Post Scriptum:
If the excess heat is nuclear then its generation can be described as an exothermic nuclear reaction:

$$A + B = C + Q$$

where A and B are masses of reacting nuclei and C, the product of the interaction, is usually the sum of masses of two or more nuclei. The Q stands for the amount of energy generated. In the very first nuclear reaction observed (Cockroft and Walton, 1931) A was the accelerated ion of hydrogen ^{1}H and B was a stationary lithium ^{7}Li.
Fusion of these two nuclei produced ^{8}Be which immediately decayed, producing two alpha particles, ^{4}He. In the so-called hot fusion A and B are rapidly moving hydrogen ions (in an ionized deuterium gas) while C stands for either ($^{3}He + {}^{1}n$) or ($^{3}H + {}^{1}p$). These two outcomes occur with about the same probability. The third possible outcome, production of ^{4}He, is extremely rare. Positively charged atomic nuclei, A and B, always repel each other. For that reason nuclear fusion of A and B is generally believed to be extremely rare at low temperatures. That is the essence of the controversy about cold fusion. The theory predicts that cold fusion of atomic nuclei is practically impossible while many

experimental scientists were able to detect it by observing nuclear particles: neutrons (^{1}n), tritium (^{3}He) and alpha particles (^{4}He).

If devices built by Israeli researchers generate nuclear energy then nuclear particles, or other reaction byproducts, should also be present. If it was up to me I would look for such "signatures" of nuclear reactions. The rate ot which nuclear byproducts are produced should be directly proportional to the rate of production of excess heat. Establishing this fact, in a highly reproducible setup, would finally resolve the cold fusion controversy. As far as I know the major issue on which Israelis are focusing is irreproducibility. But this should not prevent them from trying to demonstrate presence or absence of nuclear byproducts. How will cold fusion impasse be resolved? Will it be resolved by scientists or will it be resolved by engineers? Scientists are primarily interested in solving mysteries, engineers are primarily interested in building useful gadgets, such as airplanes and power plants. A 100% reliable gadget would stimulate scientific research, a 100% reproducible scientific setup would lead to technological innovations. Healthy competition between scientists and engineers is desirable.

Post-post Scriptum:
Basic laws of flying "heavier than air machines" were not known when first airplanes were built. But many laws of electricity were already known when first electrical power plants were built. The field of cold fusion, however, is still waiting for its Wylbur Wright, for its Michael Faraday and for its James Maxwell. Why does it take so long to resolve the controversy? I tried to answer this question in the unit #203.

Addeendum (3/16/05)
Interesting internet items about Energetics Technologies:

<http://www.zpenergy.com/modules.php?name=News&file=article&sid=1143>

Excess Heat in Electrolysis Experiments at Energetics Technologies
I. Dardik, T. Zilov, H. Branover, A. El-Boher, E. Greenspan, B. Khachatorov, V. Krakov, S. Lesin and M. Tsirlin;
Energetics Technologies P.O.Box 3026 Omer Industrial Park Omer, Israel

Using the electrolytic cells described in our ICCF-10 paper driven with Dardik's modified SuperWaves, significant amounts of excess heat were obtained in a number of experiments. The most successful of these experiments generated excess heat a couple of times: (1) Approximately 5 hours into the first loading of deuterium into the Pd cathode – giving an average power gain of ~2500% during 17 hours. The average current density was 7 mA/cm2. (2) The same foil was deloaded after the excess heat generation stopped for no apparent reason and than loaded again. After 16 hours of loading excess heat was generated again at an average level of ~1500% for 80 hours. The average current density was 8.4 mA/cm2. At the end of the two experiments the tritium concentration in the electrolyte was 270% of its pre-experiment level. The total amount of excess energy generated is approximately 1.1 MJ and 3.5 MJ in, respectively, the first and second experiments. This amount of excess energy corresponds to, respectively, ~4.8 KeV or ~15.3 KeV per Pd atom. The corresponding average specific power is 71 or 48 W per gram Pd. For comparison, the average specific power in commercial nuclear fission reactors is between 20 to 40 W per gram uranium.

The Pd cathode is a 50 μm thick foil that is 7 mm wide and 60 mm in effective length. It has been pre-treated by Dr. Vittorio Violante of ENEA in Frascati, Italy. The anodes are two 0.1 ? 20 ? 60 mm Pt foils. The electrolyte is 0.1M solution of LiOD in D2O. After the experiments the cathode was investigated using a number of probing techniques, including AES, SEM-EDS, TEM and SIMS. Significant amount of low Z contaminants were found on its surface, extending to a depth of dozens of Angstrom. Their origin appears to be the lubricant used for rolling the foil in the pre-treatment process. Their presence prohibited detecting nuclear reaction products with acceptable certainty on and near the surface. No transmutation products were found at deeper layers. However, no measurement of He inventory was attempted.

The Super Waves Principle
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The results presented at ICCF10 of Glow Discharge experiments performed by Energetic Technologies (ET) researchers in Israel were described as “dramatic”. I expect that no less will be said of the Electrochemical Cell results to be presented at this conference. It is the purpose of this communication to describe the technical features of the ET results that distinguish them from prior cold fusion results, and the governing principle and theory that guide these new experiments. This information is offered to assist others to make progress in developing new energy technology.

Unique to the ET experiments is the use of a new understanding of wave modulation. Rather than dc, dc + ac, dc pulsed or bi-level perturbation, the waveforms being subjected (successfully) to test by ET employ waves fractally nested in a specific non-linear manner designed to stimulate intrinsic oscillatory processes across a wide range of scales. I call these SuperWaves©. When properly implemented SuperWaves have been demonstrated to influence strongly processes in the realms of physics, physical chemistry, metallurgy and also in physiology.

Early validations of the SuperWaves principle were obtained in physiological studies and from theoretical and applied studies of the melting, stirring and strengthening of metals. The current presentation will focus on the organizing principle of SuperWaves in the D/Pd both as a means to improve the rate and extent of (D) loading, and to provide the stimulus needed to produce cold fusion heat (and nuclear?) effects.

P.S.
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205) Another negative encounter with DOE

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The 9th issue of New Energy Times <http://www.newenergytimes.com> describes a deplorable episode; a scientific propsal submitted by a recognized scientist was rejected by the DOE without being sent to reviewers. Here is how this bureaucratic decision.was described by the affected scientist, Dr. Melvin Miles:

On January 24, 2005 I submitted a White Paper Proposal to Dr. James Decker of DoE. Basically, I proposed experiments to optimize the cold fusion excess power effects by going to higher temperatures. For materials, I proposed using Pd-B alloys prepared by NRL and codeposition materials prepared by the methods of Drs. Stan Szpak and Pam Mosier-Boss.

My proposal was forwarded to Jim Horwitz of DoE (Basic Energy Sciences) who telephoned me on February 17 with his feedback that was mostly negative. Some of his comments are as follows to the best of my memory.

1. Proposals for the optimization of cold fusion nuclear effects cannot be considered because the 18 DoE panel members concluded that such nuclear effects do not exist.

2. Electrochemical cells have been studied to death, for example, by McKubre at SRI. Proposals of further electrochemical studies will likely not be funded by DoE.

3. Any proposed new experiments need an acceptable theory to justify such further studies.

4. More peer-reviewed journal publications are needed before this field can be considered for funding.

Because of these points, Jim Horwitz concluded that he could not justify sending my proposal out for review.

Based on this experience, I think it is unlikely that DoE will fund any research on cold fusion. If anyone has a more positive encounter with DoE please let me know. - Melvin Miles

According to Steven Krivit, the editor of New Energy Times, “Miles is a published author of 200 papers, 70 of them in the cold fusion field. A physical chemist, he has been recognized for his excellence in science by a 1966 NATO Postdoctoral Research Fellowship Award, and the following awards from his 24-year tenure with the China Lake Naval Weapons Center: Sigma Xi Award for the Best Scientific Paper in 1985 and 1988, William B. McLean Award in 1987, Fellow Award in 1989.”

According to : his areas of research expertise are: “Thermal batteries, lithium batteries, fuel cells, cold fusion, corrosion, electrochronic materials, thermodynamics, chemical kinetics, and electrochemical super-capacitors.” I am not surprised that a scientist with this background, working in a US military laboratory (see my item #51) was among the first who tried to validate to the claim made in 1989 by Pons and Fleischmann. And his negative results used against the claim. But Miles recognized experimental difficulties and continued his research. About one year later he reported positive results. That was the beginning of a long period of systematic investigations; many of them are described in papers downloadable from . I recall his report on finding 4He (byproducts?) in the cathode used to generate excess heat. He also tried to find 3He but the result was negative. It is not fare that an honest scientist, motivated by a desire to resolve the cold fusion controversy, one way or another, is not taken seriously by our DOE.

Jed Rothwell, whose full paper can be downloaded from

http://www.lenr-canr.org/News.htm (click on the “DoE lies again”)

made several interesting comments about this strange episode. I think they are worth thinking about. Jed wrote:

[Miles] letter imposes impossible Catch-22 conditions. Let us consider each statement:

1. “Proposals for the optimization of cold fusion nuclear effects cannot be considered because the 18 DoE panel members concluded that such nuclear effects do not exist.”

First, Horwitz is incorrect. The Review says that about a third of the panel members agreed that nuclear effects were detected. In addition, most panel members concluded that the claims for nuclear energy being the source of measured energy were not convincing. This is a far cry from concluding that “nuclear reactions did not exist”.**Second,** the Review recommends that proposals for experiments be considered. How could such proposals be considered if the effect does not even exist? Such an attitude condemns all proposals to rejection.

2. “Electrochemical cells have been studied to death, for example, by McKubre at SRI. Proposals of further electrochemical studies will likely not be funded by DoE.” Studies of heat production have been made by dozens of laboratories with good success and such studies are presently underway in many countries. The reality of anomalous heat production is no longer an issue. The challenge now is to improve reproducibility and to increase the amount of energy produced. In addition, the source of this unexpected energy needs to be discovered, whether it be nuclear or from some other unexpected process.

Miles is one of the few scientists who have had good success and who understands the nature of the novel process so that worthwhile results can be expected. If his work cannot be supported by the DoE, no proposed work can be expected to meet the required standards.

3. “Any proposed new experiments need an acceptable theory to justify such further studies.”

This statement turns the scientific method on its head. Cold fusion is based on experiment, not theory.

Theory is never needed to justify or prove experimental results. There is no theory to explain other recently discovered phenomena such as high temperature superconductivity (HTSC), but no one rejects the reality of HTSC on that basis. Furthermore, this imposes another Catch-22: until additional experiments reveal the nature of the reaction, theorists will not have enough data upon which to base their theories.

4. “More peer-reviewed journal publications are needed before this field can be considered for funding.”

This is the most obvious and absurd Catch-22 of all. How will researchers publish peer-reviewed papers unless they first receive funding to perform experiments? Researchers must be funded first, then perform experiments, write papers, and submit the papers to reviewed journals. Without funding none of this can happen. Inadequate funding in the past has been one of the handicaps that has prevented enough measurements from being made to answer the normal questions posed by peer reviewers. Nevertheless, as can be seen in the list of references, much of Prof. Miles’ work has been peer reviewed, yet even this does not appear to be sufficient for the DoE.

I agree with Rothwell; the arguments presented by Horwitz make no sense. A propsal submitted by a recognized authority in the field of physical chemistry should have been given the courtesy of being reviewed by other scientists. Something is basically wrong when a bureaucrat, even a highly knowledgeable one, can reject a proposal made by a qualified scientist. This, as observed by Krivit, is not consistent with a recent statement of Jim Decker, principal deputy director of the Department of Energy 's Office of Science: "We make decisions on funding research proposals on the basis of peer review and relevance." Which policy is in effect, that described by Decker or that described by Horwitz?

Something is basically wrong when judgements about validity of experimental claims are not based on empirical evidence. I already wrote about this in ”The Open Letter to DOE scientists” (see unit #196). But nobody replied; probably because members of the DOE panel do not know about my letter. Several weeks ago a slighly modified version of unit #196 was submitted as a letter to the editor of Physics Today. Will that letter trigger a discussion of moral responsibilities of those who coordinate scientific development? I hope so. But I still do not know if my letter will be published in Physics Today.

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206) A new Russian report on nuclear alchemy

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1) Two years ago I mentioned that work performed in a Russian laboratory, Lutch, had a strong influence on my attitude toward cold fusion. The publication that impressed me, by Karabut, was translated from Russian in unit #13. Irina Savvatimova, also from Lutch, worked with Karabut, as reflected in their 1992 publication. I had a chance of meeting her at two cold fusion conferences. Two days ago she informed me that her ICCF11 report can be downloaded over the Internet. The URL is:

http://rapidshare.de/files/853392/Savvatimova_article_ICCF11.doc.html

Note that the "download" button, on the screen that opens, is at the bottom. Be aware that the file is in the MS Word format and that its length is 14 Mb.

2) The ion bombardment apparatus used by Irina is similar to that described in unit #13; it is the deuterium glow discharge chamber. The applied voltages were between 300 V and 850 V; the currents were between 10 mA and 20 mA. But the main tool was a scanning electron microscope. It was used to examine a titanium cathode before and after the ionic bombardment (in experiments in which excess heat was also measured). The microscope was equipped with a setup, called EDS, (elemental dispersion spectroscopy?) that analyses X-rays emitted from spots selected by the electron microscope beam. Another instrument, called TIMS (thermo-ionization mass spectrometer), was also used in the reported study.

3) The purpose was to demonstrate nuclear alchemy -- formation of new elements. That area of research, known as nuclear transmutations, has been investigated by several cold fusion researchers. (see my units #85, #104 and #105). Traditional alchemy is known to be impossible and transmutations, if confirmed in reproducible experiments, will convince us about reality of unexpected nuclear processes. The instruments used in Moscow are probably available in many US laboratories; I have no idea why others are not trying to either validate or contradict experimental data reported by Savvatimova and Gavritenkov. Counting how many DOE panel members believed (and not believed) in validity of experimental results is not a correct way to deal with claims made by experimental scientists. Don't we have experts able to perform experiments described by Russian researchers?

4) I suppose that it is a matter of poor translation to refer to "byproducts of transmutation" as "impurity elements." The terms "new elements" and "trace elements" would also be better for a situation in which authors are convinced that new elements were not introduced through a contamination. The English word impurity implies contamination, at least to me. With this in mind let me quote from the paper: "The exceeding content of the impurity elements on the irradiated titanium surface were found in 20 cases from 47 analyzed places. The amount of the impurity elements in the comparison with an initial material was more from 10 up to 1000 times and from the tenth parts of percent up to percents. The integral content of the impurity elements in an initial sample did not exceed $7*10^{-3}$ %. "

The initial overall concentration of iron in titanium, for example, was 0.002% but the local concentration in the selected 6 sites, after the exposure to deuterium ions, was twenty times higher (0.04%, according to Table 3). The situation for Al is even more dramatic: -- 0.003% before the exposure to ions versus 0.33% after the exposure, in 9 selected sites. Possibilities of non-nuclear origin of such findings are discussed and rejected on the basis careful examination. I recall an earlier publication in which a highly unusual isotopic composition of iron, found in the palladium cathode, was

reported. Irina was a coauthor of that paper (see item #13). Unfortunately, the isotopic composition of iron produced in titanium was not reported; that would be a very convincing argument in favor of its nuclear origin. Some speculations about nuclear reactions, that might be responsible for generation of new elements, are offered. The last paragraph consist of five conclusions:

"1. The following elements O, F, S, Al, Na, Mg, Fe, and Ni in amount of 0.3-0.5 % up to ten percents were found in Ti foil after deuterium glow discharge experiments with excess heat effect.

2. The appearance of new elements within structural defects on the surface (in the micro explosions places, micro craters, local melting zones, phases inclusions on the tracks and others formations) could be the result of unequilibrium processes such as a micro arc with overvoltage with some accelerated effect leading to fusion and fission reactions in the excited crystal lattice of cathode surface.

3. The appearance of most elements obeys the law of energy conservation.

4. Excess heat effect in Ti foil could be explained as a result of elements transmutation in the cathode under excitation of crystal lattice in the deuterium low energy glow discharge.

5. It is necessary to have more statistical results with sequential analysis for explanation and understanding of mechanism."

I do not know why the potential difference under which the reported results were obtained was not specified more accurately; a person desiring to replicate the experiment would not be happy with the 300 to 850 volts range. And I was not happy that isotopic ratios were not reported. I expected to see them because of the following introductory statement: "Investigation of the isotopic composition was carried out by the thermo-ionization mass spectrometry method (TIMS)." The only place in which isotopic masses are shown is Table 4. But that table does not list iron. Three isotopes of Sr are listed but isotopic ratios are not specified. What is interesting, however is absence of the isotope ^{87}Sr. In natural Sr that isotope is 12.5 times more abundant than ^{84}Sr, actually observed by authors. I suspect a typing error in Table 4; otherwise the observation would be discussed as highly significant. What can be a better indication of nuclear origin than a highly abnormal isotopic ratio? Another possibility is that the ^{87}Sr peak was covered by the tail of the eleven-times-higher ^{88}Sr peak. I am not familiar TIMS; I do not know what its resolution should be in different mass regions.

Normally the most abundant isotope of calcium is ^{40}Ca (97%). But that isotope is not listed in Table 4. Another typing error? The same question can be asked about the absence of ^{160}Gd. I will assume that trivial clerical errors were made in composing Table 4. What else can it be? Absence of information on isotopic composition of iron is likely to be disturbing to anybody who read unit #13, and papers listed in it as references. According to Lutch scientists one half of iron produced in a palladium cathode consisted of ^{57}Fe. That is over 20 times more than in common iron. Is the situation in titanium the same as in palladium or is it very different? I would expect Savvatimova to focus on that question. But she did not address it. I am puzzled by this.

Unusually high contamination at selected spots on the surface of a titanium cathode, in comparison with its nominal bulk composition, can possibly be due to factors that have nothing to do with nuclear reactions. All kind of impurities are likely to appear on surfaces when foils are manufactured. Lutch researchers are certainly aware of this; that is why they compare observations of two surfaces of the same foil. The surface bombarded by ions, has more "new elements" than the surface that was not bombarded. But the factor of twenty (0.002% versus 0.04% for iron), if my understanding is correct, does not refer to two surfaces. Arguments of that kind would become totally irrelevant if the isotopic composition on "new iron" were shown to be very different from that of ordinary iron. Savvratimova knows this very well; she was a coauthor of several publications in which highly unusual compositions of new elements, including iron, were reported for a palladium cathode. That is why I am puzzled.

Appended on 3/24/05:
This unit was listed as "work in progress" for eight days because I was waiting to for Irina's clarifications. And I am

glad that I waited; what she wrote to me (see below) is extremely important. In my opinion, the paper should be revised before it is made available over the Internet.

Irina's clarifications:
Your questions are very deep. Not all questions have simple answers.
1. The change of the potential difference has no significant influence on the results. The glow discharge in our chamber is stable for any potential difference between 300 and 800 V.

2. The change in chemical composition with initial structure was compared in the Table 4 . CPS is count per second during the mass-spectrometry analysis. Such significant numbers are not error. Table 4 does not content trivial clerical errors:

a) Ca40 is an isotope, which we observed in large quantity during the analysis of the initial sample and after after experiment. It is a reason of the excluding this isotope from table 4. [To avoid confusion I would not exclude Ca-40 from the table]

b) Sr 87 has the mass as Rb 87 (85, 87). It is not right to include in table for the comparison. Much mass 87 in the tungsten cathode by mass-spectrometry method was noted also before. [I am not sure I understand this point.].

c) The decreasing Fe 56 up to 100 times was observed after experiment. Fe57 had not significant quantity before experiment, and Fe57 had not change in the comparing with initial sample after experiment.

However:
** The peak of the mass 55, after the bombardment, was 40 times larger than before the bombardment. [In my opinion focusing on a single peak is less convincing than focusing on isotopic ratios].

** Mass 55 is Mn55 or Fe55 (electron capture). Mn was not detected by EDS method. Fe was revealed by EDS method up to 4±0.2% (I think that this is highly significant. I will look at this fact again more carefully. It may be an indication of a mechanism for the Fe56 - Fe55 transformation.

3. You are right, showing isotopic ratios would be a more convincing evidence of nuclear transmutations. I did not include the ratios to save place (I tried to put all results in 12 pages). I do have isotopic ratios but they were not included in the paper; I will add them soon. Thank you very much for your interest and questions.

[Jed Rothwell told me that the paper of Savvatimova and Gavritenkov will soon be downloadable from the library at <www.lenr-canr.org> I hope it will reflect information on isotopic ratios.]

Adenedum (6/24/05):
The updated version of the report has been in that library since last may. Table 5 reports data on ratios of Ca isotopes. The claim is that Ca is one of the transmutation products generated during the experiment in the titanium target and that its isotopic composition is significantly different from that found in common calcium (96.9% of Ca-40, 0.65% of Ca-42, 0.135% of Ca-43 and 2.0% of Ca-44). It would be preferable if percentages of individual isotopes found in the Ti target were reparted in the same way as for common calcium. Unfortunately, what is reported are relative abundances.

The table shows that the relative abundance Ca-40 with respect to Ca-44 was 31 (insted of 48.4, as in common calcium), the relative abundance Ca-40 with respect to Ca-42 was 125 (insted of 161), the relative abundance Ca-44 with respect to Ca-43 was 49 (insted of 14.8), and the relative abundance Ca-44 with respect to Ca-42 was 4 (insted of 3.3). I do not know why absolute percentages were not given; that would be more useful. Estimated experimental errors, associated with these abnormal relative abundances, would also be useful. I assume that more information will be available in a future publication. Changes in isotopic composition of all trace elements are worth studying. As I wrote before, the term "trace element" is better than the term "impurity" (used in this paper). Impurity implies contamination.

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207) Cases of Scientific Controversies

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I belong to Phys-L, the Internet discussion list for physics teachers. Last night a teacher from California posted a message with the following URL in it:

<http://www.newscientist.com/channel/space/mg18524911.600>

It is a link to an article that appeared in the current issue of **New Scientist Magazine** (dated as 3/19/05). The title is: "13 things that do not make sense." Describing
the article the teacher wrote: "The review has been one of a handful of American publications that has defended continued investigation into cold fusion - in contrast to other media which has ridiculed or ignored the issue. Now the New Scientist lists
cold fusion as one of a dozen 'observations that simply defy explanation . . . exceptions that could rewrite all the rules'. " The "13 things" are examples of protoscience; they are not examples of pseudoscience. Let me summarize these examples, and insert some comments. The last case is cold fusion.

Case #1 (Placebo effects):
This is about Benedetti's data on how " 'mind can affect body's biochemistry. ..... The relationship between expectation and therapeutic outcome is a wonderful model to understand mind-body interaction.' Researchers now need to identify when and where placebo works. There may be diseases in which it has no effect. There may be a common mechanism in different illnesses. As yet, we just don't know." The placebo effect, according to the article, is a well known experimental fact. That fact, however, has not yet been explained. So what? Absence of an explanation does not take away from its use in medicine.

Case #2 (The horizon problem):
This is a cosmological paradox. How can the microwave radiation fill the visible universe, whose size, from one visible horizon to another is 28 billion light years? This well recognized (?) fact is in conflict with a confirmed theory according to which nothing can travel faster than light. Attempts to solve the paradox lead to other paradoxes, according to a Cambridge astronomer Martin Rees. The conclusion is that "In scientific terms, the uniform temperature of the background radiation remains an anomaly."

Case #3 (Ultra-energetic cosmic rays):
"For more than a decade, physicists in Japan have been seeing cosmic rays that should not exist." Their energies exceed the theoretically established limit (known as the Greisen-Zatsepin-Kuzmin limit). How will this apparent contradiction will be solved in the future? Will experimental facts will be invalidated or will something wrong be found in the theory? The essence of science is to recognize contradictions and to find ways of solving them. An ongoing experiment designed to shed light on the well known problem is described in the article.

Case #4 (Homeopathy):
"Homeopaths prepare their remedies by dissolving things like charcoal, deadly nightshade or spider venom in ethanol, and then diluting this "mother tincture" in water again and again. No matter what the level of dilution, homeopaths claim, the original remedy leaves some kind of imprint on the water molecules. Thus, however dilute the solution becomes, it is still imbued with the properties of the remedy.

You can understand why Ennis remains skeptical.“ Me too. But I would not refuse an invitation to witness a properly conducted demonstration. According to the article, “No homeopathic remedy has ever been shown to work in a large randomized placebo-controlled clinical trial.”

Case #5 (Dark Matter):
Astronomers know that many galaxies spin faster than can possibly be explained in terms of our best gravitational theory. This paradox was recognized by Vera Rubin in 1970. “The best response from physicists was to suggest there is more stuff out there than we can see. The trouble was, nobody could explain what this ‘dark matter’ was.
And they still can't. ..... Astronomical observations suggest that dark matter must make up about 90 per cent of the mass in the universe, yet we are astonishingly ignorant what that 90 per cent is.” That is one possibility. The other is that our gravitational theory should be modified to deal with extremely large distances.

Case #6 (Life on Mars):
This is a case where an experiment performed on Mars (Viking Lander, 1976) indicated presence of organic molecules. The discovery made by Gilbert Levin was not recognized as a proof. “Almost all the mission scientists erred on the side of caution and declared Viking's discovery a false positive. But was it? The arguments continue to rage, but results from NASA's latest rovers show that the surface of Mars was almost certainly wet in the past and therefore hospitable to life. And there is plenty more evidence where that came from, Levin says. ‘Every mission to Mars has produced evidence supporting my conclusion. None has contradicted it’. " But not contradicting is not the same thing as confirming. The original experiment was not replicated during later missions.

Case #7 (polyneutrons):
At the last cold fusion conference (ICCF11) I learned about a suggested theory of cold fusion. That theory of John Fisher was described in item #191. The description, however, does not reflect recent modifications. Several days ago John wrote to me about the new version of the theory; it will be described in an upcoming conference in Italy. A new attempt to identify a polyneutron is apparently in progress in France.

Case #8 (Trajectories of two space probes):
Observed trajectories of two space probes (Pioneer 10 and Pioneer 11, launched in 1972 and 1973, respectively) did not coincide with what was anticipated. How can this be explained? “Nobody knows. Some possible explanations have already been ruled out, including software errors, the solar wind or a fuel leak. If the cause is some gravitational effect, it is not one we know anything about. In fact, physicists are so completely at a loss that some have resorted to linking this mystery with other inexplicable phenomena.” Another indication that our knowledge of laws of gravity is limited?

Case #9 (Dark energy):
The article claims that dark energy (a property of empty space) the most contradictory problem in physics. “In 1998, astronomers discovered that the universe is expanding at ever faster speeds. It's an effect still searching for a cause - until then, everyone thought the universe's expansion was slowing down after the big bang. Theorists are still floundering around, looking for a sensible explanation, ..."

Case #10 (Planet X):
Distribution of icy rocks, in our own planetary system changes suddenly beyond the planet Pluto. This has been interpreted as an indication that a planet, as massive as Earth, was attracted all the rocks around. But no such planet has ever been observed. This controversy is likely to be resolved in 2015 when the New Horizons probe (to be launched by NASA in 2006 will reach Pluto.

Case #11 (Extraterrestrials trying to contact us?):
An astronomer, Jerry Ehman, once detected a very unusual signal with a radio telescope. “And 28 years later no one knows what created the signal. ‘I am still waiting for a definitive explanation that makes sense,’ Ehman says.” Why is he waiting for it? Who is he expecting to provide it? Rare effect are very difficult to study because they are rare.

Case #12 (light from distant quasars):
Light emitted 12 billion years ago, and analyzed by spectroscopists on earth was found to contain peaks due to

absorption in atoms scattered in the universe. But locations of peaks seems to be shifted with respect to where they are on earth. That is what was discovered in 1997 by an Australian astronomer, John Web. But French astronomers, headed by Patrick Petitjean, failed to confirm this observation. The team is now conducting a new experiment to validate the data. What can be a better illustration of how controversies should always be resolved in science? But progress is not easy. “The more we look at these new data, the more difficulties we see’ wrote another team member, Michael Murphy.

Case #13 (cold fusion):
“After 16 years, it's back. In fact, cold fusion never really went away. Over a 10-year period from 1989, US navy labs ran more than 200 experiments to investigate whether nuclear reactions generating more energy than they consume . . . With controllable cold fusion, many of the world's energy problems would melt away: no wonder the US Department of Energy is interested. In December, after a lengthy review of the evidence, it said it was open to receiving proposals for new cold fusion experiments.
That's quite a turnaround. The DoE's first report on the subject, published 15 years ago, concluded that the original cold fusion results . . . were impossible to reproduce, and thus probably false. .....The snag is that fusion at room temperature is deemed impossible by every accepted scientific theory.”

Validation of experimental facts should be based on better experiments. Science has been highly successful because it is characterized by unity between theoretical models and experimental data. The principle, as somebody wrote, is that theories guide but experiments decide. In science, unlike in mathematics, validity of theoretical models is based on experiments. Unfortunately, as illustrated in unit # 206, the DoE does not seem to be open to support research in the area of cold fusion.

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208) An interview with Martin Fleischmann

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For some reason I was not aware (or forgot) that Haiko Lietz, a science reporter from Germany, interviewd Fleischmann at the last cold fusion conference (September 2004). The original version of this interview, in German, has been posted at:

< http://www.heise.de/tp/r4/artikel/19/19257/1.html>

while the English translation has been posted at Steve Krivit's New Energy Times website:

http://newenergytimes.com/Conversations/FleischmannByLietz.htm

This interview is interesting and worth posting on my website as well. Haiko has no objection. I think that what Fleischmann did not say is even more interesting than what he did say when the question was "*What mistake have you made in your life?"*

I would expect him to say that the agreement to have the 1989 press release was a big mistake. Either he or Pons depending whose decision counted (in the eyes of university administration), should have resisted the premature release of information about cold fusion research. But that event was not even mentioned in the interview. I would also expect him to say something about the broken agreement to work together with Steven Jones and his team. Jones' lab, in Provo, was only 20 miles away from Salt Lake City. As far as I know, the agreement was to investigate the unexplained phenomena a little longer, and then publish the results in a peer-reviewed journal.

Did Fleischmann already talked about these things in another interview? Did he write an article about political manipulations at the University of Utah? If he did then I would very much like to refer to material with which I am not familiar. Please write to me at <kowalskil@mail.montclair.edu>; I would be happy to poste additional information. Please refer to my unit #131 (Jones' history paper) and #132 (extracts from Fleischmann sociology paper). Cold fusion would not be excluded from mainstram science if it were introduced to the world in the usual way. Allowing the press release to go on prematurely was a mistake of Fleischmann and Pons.

* * * * *

"Why is everybody waiting for America when it comes to research?"

A conversation with Martin Fleischmann, the discoverer of cold fusion

by Haiko Lietz

Professor Martin Fleischmann became world-famous when on March 23, 1989 he reported, along with his colleague

Prof. Dr. Stanley Pons, the discovery of cold nuclear fusion in a simple tabletop electrolysis experiment at room temperature. The Nobel prize seemed in reach for them. But since some laboratories failed to reproduce the results, scientific and public opinion changed against their favour. Cold fusion has long been a synonym for an error in science. In this interview, made at the 11th International Conference on Cold Fusion before the 2004 Department of Energy Review result became public, Fleischmann speaks about how he deals with criticism, the role of quantum theory, and the demise of a purely consuming society.

Martin Fleischmann was born on March 29, 1927 in Carlsbad, Czechoslovakia. Because he was adopted by a Jewish family he and his family were forced to flee from the German soldiers to England. At London's Imperial College and Southampton University he ascended to become a world-renowned electrochemist. His last publication <http://lenr-canr.org/acrobat/SzpakSthermalbeh.pdf> with US Navy scientists was one of the basic papers considered by the Department of Energy reviewers. Fleischmann lives with his wife in Wiltshire, England.

You could long be "kicking it in the Caribbean" – to use a famous quote from the Pulp Fiction movie – but you're here presenting your thoughts on quantum mechanics. Can't you let loose?

If you work in this field and you have a negative result, you can walk away from it. It doesn't hold. But if you get a positive result, then of course it gets a hold of you. But I'm going to give up. My wife says, you keep on saying that you're going to give up. But I really haven't done anything new since 1995. I've made certain number of observations which are still valid, and I would have to reinterpret them. But in fact I have not reinterpreted them. I've just given the same old interpretation as I've always given to those observations.

But if you plan to give up maybe you should wait until the end of the year because the U.S. Department of Energy will be putting out another report.

I don't know. I think that they will be putting out another report. But it is very difficult to see how a subject like this can attract government funding. This is difficult, because you have to demonstrate some sort of requirement. You have to say is there a military requirement? Or is there a civilian requirement? I think there is… there are requirements. But our society does not respond in that way anymore.

The German Ministry of Research wants to wait for the Department of Energy report before it acts in any way.

(Laughs) That is so stupid! Why is everybody waiting for America when it comes to research?

You said "cold fusion" is a horrible term. Why?

This was a term pushed on us by other people. But what we do is not fusion in the conventional sense – the fusion process as recognised by hot fusion. And the fact that it was given that name polarized people's opinions. They said, "Well, it has to be like hot fusion. But it isn't like hot fusion. There are no nuclear signatures. So it can't be fusion. So you're all wrong." they said. That's the way it happened. If it had been called something else it might still have happened, but it is less likely to have happened.

Here is the "sixty-four-dollar" question: Why is heat and helium-4 produced in electrolysis experiments involving heavy water and Palladium cathodes?

I'm afraid it is because two deuterons fuse together to form helium-4 and heat. That is actually true. But that still isn't the conventional nuclear signature of hot fusion. In hot fusion it would have to produce tritium and a proton or helium-3 and a neutron or helium-4 and a gamma ray. This is if you had two particles hitting each other at enormous energy.

Did those people that assumed that the result should be as in hot fusion fail to consider the different environment of your experiment?

Yes, it's completely crazy! I think they will come to realize that.

Why did you initially start your experiments?

This whole subject was driven by the need to find demonstration of the quantum electro dynamic paradigm. We've had the classical paradigm of Newtonian mechanics. We've had Planck's quantum mechanical paradigm. I think most people who work in physical science realize the limitations of the quantum mechanical paradigm and that it has to be replaced by the quantum electro dynamic paradigm, nevertheless the introduction of that paradigm is strongly resisted. And if you introduce the quantum electro dynamic paradigm you see that what is called cold fusion might be possible.

How could nuclear fusion at room temperature be explained using quantum electrodynamics?

Well, if you think about quantum electro dynamics you realize that you get a large assembly, a large collection of atoms and molecules behaving as a single quantum system. So then you say, if I build a small amount of energy per atom into this large assembly of atoms and molecules then I will have a very large energy. And that large energy of course translates in the end to observable nuclear effects.

So physics actually drove your research?

I had worked since the 1960s on quantum electro dynamics in conventional chemistry. I had realized that all those systems had to be modelled in terms of quantum electro dynamics. And then I said, well, what is the most extreme question we can pose in quantum electro dynamics? And that is, can we get a nuclear effect by chemical means? I thought it would happen, but you might not be able to observe it. What happened is that it is possible and you can observe it.

That's what drove your cold fusion research?

That was the underlying question. I thought it had to be true, but I thought that we would not be able to observe it.

There is the next generation after yours, you could say, that is now continuing what you had started… one hope*... and what is for you the most interesting work being advanced by this generation in the field?*

Well, the most interesting work that has been done, in my opinion, is the work which was done in Frascati, which is not mentioned by the current people coming from Frascati. That is the work which was started by the late Prof. Preparata, his colleague Emilio Del Guidice, Antonella De Ninno, and Antonio Fratolillo on the effect of electric fields in creating more extreme conditions in the lattice, leading to the generation of excess heat and the generation of helium-4. To me that is the most significant result which has been reported in the last two years.

What does Stanley Pons actually do?

I don't know what has happened to him. He's sort of disappeared. I am not in contact with him although I would be prepared to be in contact with him. But he obviously doesn't want to be in contact with me. As far as I'm concerned he has disappeared, which is very sad.

Which cold fusion related events in your life do you like to remember?

None! (laughs) No, I don't really. This has been a terrible experience.

Come on, you must have at least met some nice people?

Of course there are people I appreciate meeting, but on the whole, when you get into this sort of situation, when you become a non-person, you become extruded from the society – but that's alright, that people ignore me. To hell with reputation! In science there's only one thing which matters. And that is the experiments. People periodically forget that you can say, "well, something might be possible, it might not be possible, now let's put it to the test."

But your critics also did experiments.

That was terrible! There were three studies which did us a great deal of harm. One was the study in the California Institute of Technology. Another one was the one in MIT. And the third one was in Harwell. MIT simply changed a graph. They changed the baseline. And actually if you look at Nathan Lewis' results (CalTech) rather carefully you will come to the conclusion that he observed excess heat! The only study which was honestly reported was the study in Harwell. That was actually honestly reported. They actually observed the generation of excess heat. They did not look at their data with sufficient care, but if you look at their data with sufficient care, you will see they observed the generation of excess heat – as I pointed out to them. I wrote to the head of the laboratory and said you observed excess heat – look at your results!

What mistake have you made in your life?

Oh, I've made plenty of mistakes.

Can you name one?

It's difficult for me because when I make a mistake I acknowledge it. I say I made a mistake. It's no big deal. I made a mistake. You put it right. So, I don't really remember. In truth, I don't make many mistakes.

You said you are pessimistic about the future.

I'm pessimistic about science.

The future of science or the future in general?

No, not necessarily… In the short term I'm not pessimistic about the future, but I think that we have to acknowledge that our society has become orientated towards consumption rather than production. And a society that becomes orientated towards consumption abandons scientific investigation. There are plenty of historical precedents of this phenomenon. And in the end, what has happened in the past is that societies which abandon the pursuit of science die. Our society will not necessarily die, but it will become unimportant.

If cold fusion should be developed to an energy source I'd like to thank you and leave the last words to you.

Well, I hope and pray that Germany will contribute to this science. It has a long scientific tradition, which could contribute a great deal to advance this subject. And I hope that the topic will be reconsidered and that especially the young people will enter the field and produce useful results. That's my hope.

Addendum (3/27/05):
Steve Krivit read my comment in the preface (about regretting the 1989 press conference) and sent me his own interpretation. He wrote:

1. From all my conversations with Martin, he seems to not want to publicly criticize the University of

Utah. It appears to me that the University of Utah administrators twisted his and Stan's arms. In my conversation with him during this interview, the clear impression I was left with was that at the time, he and Stan had the opportunity to co-operate with the University's wishes or they could find employment elsewhere.

2. He would not say anything about Jones publicly. Privately, he will express great bitterness and rage towards Jones. He's British, remember.

3. You may post this excerpt from my book if you think it is helpful.

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The Infamous Press Conference

Fleischmann and Pons had little choice in matters pertaining to the initial publicity and the infamous press conference. The University of Utah's patent- and grant-seeking interests took precedence over scientific protocol. Fleischmann and Pons told the university that they would need many more months to complete a formal paper on the subject before making any announcement to the scientific community, but administrators wanted to announce the discovery before any paper had been published.

Fleischmann reflected on this stressful period of his life in an April 2004 letter: "I was not at all in favour of the high publicity route adopted by the University of Utah and wanted to delay consideration of publication until September 1990." But the university made it clear to him that he "had to appear supportive of their position." Fleischmann ran up against a wall with the university administration and attempted to use his prestigious connections to halt the press conference:

I cast around for other means to put a spoke into the university's objectives. I tried to get hold of Lord Porter, the president of the Royal Society, to ask him to contact Mrs. Thatcher, to ask her to get hold of George Bush (senior) to block the proceedings. I failed in my manoeuverings!

Eventually, the two electrochemists agreed with the university administrators to submit an abbreviated paper called a "Preliminary Note."

The University of Utah administration received word that the Fleischmann and Pons paper, "Electrochemically Induced Nuclear Fusion of Deuterium," had been formally accepted for publication on March 22, 1989, in the Journal of Electroanalytical Chemistry. Arrangements for a press conference to announce this news to the world were made sometime between March 20 and March 22. The press conference, not surprisingly, was a hastily and poorly planned affair.

The rushed announcement has been attributed to several factors. First and foremost was the universitys objective to secure first place in the cold fusion race and to corner the market on the cold fusion intellectual property. A few miles away, at nearby Brigham Young University, physicist Steve Jones was working on another type of cold fusion experiment. It is now known that Jones'cold fusion was markedly different from Fleischmann and Pons': Jones' work showed no signs of being an energy-producing device.

But this distinction was poorly understood by university administrators. On hearing rumors that Jones may be poised to announce "cold fusion," the University of Utah moved to secure its place at the patent office by publicly announcing its "prior claim."

The press conference was a disappointment to many scientists who were eager to learn the details of the experiment. The press release announcing the March 23 conference, edited by university administrators, carefully limited the scientific details. Furthermore, and unfortunately, as author Charles Beaudette wrote, there were other communication problems:

"The Preliminary Note that was accepted for publication the previous day was not made available for distribution [at the press conference]. The omission constituted a breach of protocol, as did their failure to brief their colleagues in the chemistry and physics departments beforehand."

It wasn't until 2? weeks later, on April 10, that the paper was published in the Journal of Electroanalytical Chemistry. Still, the preliminary note was devoid of many important details and highly inadequate as a guide for other scientists to replicate the experiment. It was clearly a hasty attempt on the university's part to establish a foothold for its patent objectives and, perhaps, its fame.

The university believed it had its hands on the most valuable patent in modern history, and for this reason it also prohibited Fleischmann and Pons from personally disclosing key details to their fellow scientists. Dr. Chase Peterson, the university president at the time, was forthright about the university's interest, as shown in written testimony to the U.S. Congress:

"Upon the advice of our patent counsel, it is not possible for the University of Utah to share research results with other laboratories, particularly national laboratories, until the information has been incorporated into a patent application and the application is on file in the patent office."

This secrecy generated ill will not only among skeptics from around the world but also among academic peers at the University of Utah. Distrust, anger, and even rage mounted almost immediately when other scientists attempted to learn the essential details of the experiment so that they could, in earnest, prove or disprove the experiment, a normal part of the scientific process. But this was nearly an impossible task, considering the legal restrictions.

"Fleischmann looks back with sadness on these times. 'I really didn't want to do it this way. I did not want to do this project this way,' he said in an interview in 2003."

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209) Australian connection

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This is to another ICCF11 paper. I consider it to be an "Australian connection" because three out of five authors, including the first author, are from Australia. The title of the paper is "**Low Energy Nuclear Reactions resulting as picometer interactions with similarity to K-shell electron capture;"** the authors are H. Hora, G.H. Miley, X.Z. Li, J.C. Kelly, and F. Osman. George Miley is from Fusion Studies Laboratory at the University of Illinois, USA, and X. Li is from Tsinghua University, China. Miley is a recognized authority in both hot fusion and cold fusion; I read many of his papers. Let me begin by quoting an observation made in the Introduction; it resonates with what I wrote in unit #203"

"Since every cold-fusionist likes to get his own credit, nearly nobody is taking the work of colleagues serious or carefully reproduces the other's work. It is really the responsibility and duty of funding agencies like DOE or others to financially attract most carefully selected teams to reproduce the one or the other serious result as the very first step. Only after this clarifying progress on experiments, one may talk about any theory or model."

In writing about excess heat experiments I often wrote that nuclear origin of that heat must be demonstrated. Many researchers reported excess heat (generated at the rates of below and above one watt) but failed to demonstrate its nuclear origin. here is how this issue is addressed by Hora et. al:

". . . The generation of heat was indeed in the focus of interest. Experiments were performed in gaseous atmosphere at different pressures and temperatures placing Pd wires in deuterium gas but observing also effects if not deuterium but light hydrogen is loaded into the palladium. Long time repeated experiments with Pd wires in hydrogen atmosphere showed . . .heat was . . . produced [at the rate of] 3.6 kW/cm3 heat or 13 keV per palladium atom. Such energy cannot be produced by chemical processes."

In other words, the issue of the "non-chemical origin" can be ruled out when heat is generated at a sufficiently high rate, and during a sufficiently long time. In the described experiment heat was generated for 43 hours after the glowing discharge was terminated. But the 13 keV per reaction is still lower than a typical energy of several MeV known to be generated in many nuclear reactions. Addressing this issue the authors wrote:

"Since any heat generating process will not be due to every average Pd atom but to specific ones only, reactions with the well known MeV can be concluded as expected from nuclear reactions If such nuclear reactions occur - even without emission of alphas, betas, neutrons or not resulting in radioactive reaction products - the MeV recoil of daughter nuclei should produce x-rays in the few keV range and the MeV daughter nuclei should appear as traces in CR39 foils. Both has been detected."

I am not going to focus on essential ideas developed in this theoretical paper. The main point is that "hot fusion" was also very controversial when it was discovered in early 1930s. A conflict with an existing theory (this was before the tunneling effect was recognized) should not be a basis for rejection of highly reproducible experimental facts. Let me quote an interesting quotes from the Introduction.

The problem may be that the phenomena [of cold fusion] were brought forward to physics by non-physicists. In this situation it may be permitted to recapitulate what happened in similar cases before. When Becquerel discovered 1896 that the pitchblende from St. Joachimstal in Bohemia and other minerals containing uranium are emitting certain

radiation blackening photographic plates, a wide range of people were speculating about this phenomenon. There were even papers explaining that some ghosts are involved. Ernest Rutherford after his undergraduate studies in New Zealand produced splendid results with his Ph.D. in Cambridge where he before 1900 contributed to Marconi's detection of electromagnetic radiation by discovering very ingeniously the "radiomagnetic detectors". After becoming a professor at the McGill University in Montreal, he discovered that pitchblende emitted helium as demonstrated spectroscopically and found that another emission were energetic electrons which were just recognized at this time. With this discovery of alpha- beta- and gamma-radiation he became the founder of nuclear physics but his faculty was going to dismiss him because he was working in a field related to ghosts. He was saved in last minute by the offer of a professorship in England.

The discovery of fission is a well known case of unexpected phenomenon. I do remember what O. Hahn and F. Strassmann wrote in their famous 1939 paper. It was something like this: "We know that our discovery makes no sense in terms of what is theoretically expected. But we are analytical chemists and we have no doubt that barium is produced when uranium is bombarded by neutrons." L. Meitner was no longer in Germany when this paper was published. But it was she who were the first to say, in a paper published in England, that uranium might be splitting into two large fragments and that the process generates more than 100 MeV of energy per event. The term fission, that she introduced to name this mechanism, was already used by biologists to describe division of cells.

"The more skeptical are physicists when chemists or others are claiming anomalies in physics. Such a problem was between the chemist Otto Hahn and the radiation physicist Lise Meitner, a most prominent team e.g. with the discovery of the new element protactinium in 1918 where they – against the rules – did not receive the Nobel prize. Physicists expected the production of heavier nuclei when bombarding uranium with neutrons into which direction Meitner was looking when she left Berlin mid 1938 under unfavorable circumstances to Stockholm. Meitner meeting Hahn November 1938 still "objected to the most recent findings" of Hahn who then again with his world best techniques of chemical micro-analysis confirmed that elements of middle weight were produced, proving that the neutrons were splitting the uranium nucleus. These results were reproduced very quickly in comparably easy experiments and the enormous consequences are known."

Possibilities of practical applications of fission, via a chain reaction, were recognized as soon as reality of the new process nuclear was confirmed. The authors of this ICCF11 paper claim that their experiments were 100% reproducible. Numerous references to their publications are shown.

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210) Investigations of Oriani solid effect: Work in progress

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1) Introduction:
My attempts to get involved in cold fusion research were described in unit #189. The first attempt was to work with Harold Fox. We demonstrated that the anticipated transmutation of a radioactive thorium material into a non radioactive material did not take place. The second attempt was a short cooperation with Steven Jones. Only one experiment was conducted by me and it could be interpreted as a confirmation of findings reported by Jones and his team. The third attempt was a short cooperation with Dennis Letts and his team. A very strong anomalous effect was discovered but, unfortunately, the experiment was not repeated. Both attempts were described at the last cold fusion conference (ICCF11, 2004). Our paper presented at that conference can be downloaded from the library at <www.lenr-canr.com>.

The first three attempts had two things in common: (a) I was only marginally involved, and, (b) I had no opportunities to repeat experiments. The fourth attempt was, and still is, very different. It is a study of Oriani effects described in unit #188. I have a replica of Oriani's cell at home and often use it, since november 2004, to perform experiments. Preliminary results, described in units #192 and #197, reflected my learning. Descriptions are too long and boring; it was naive to think that people would be interested in so many irrelevant details. But I am not sorry for being actively involved in a study which may turn out to be very significant.

Oriani vapor and liquid effects have been described in 2003, at ICCF10 (International Conference of Cold Fusion). Papers of Oriani and Fisher, presented at that conference can be downloaded from the library at <www.lenr-canr.org>. The Oriani-solid effect, which we are now studying, has been published in 2004, at ICCF11. That effect consists of particles detected in air below the 0.12-mm-thick nickel cathode (at the bottom of the cell). The cathode is thick enough to stop natural alpha particles coming from the electrolyte. Oriani thinks that alpha-like particles must somehow be produced in nickel, or in air below it. Studying particles in air, outside the electrolytic cell, can be conducted not only with track detectors (used by Oriani), but with surface barrier detectors as well, as in Jones' experiments.

The goal of our research was to turn Oriani's vertical cell (described in unit #188) into a simple setup for student-oriented experiments. Track detectors are ideal for that purpose because they are extremely sensitive, relatively inexpensive, and simple to understand. That is why our work, so far, was done exclusively with track detectors (CR-39 chips). Surface barrier detectors will probably be used after finding conditions under which observations are nearly 100% reproducible. Information about energies of particles, and about times of their emission, would help us to understand what is going on.

2) From Oriani's recent e-mail messages

a) According to what I wrote in unit #188, the CR-39 chip (area 0.6 cm^2) placed 1.5 cm below the cathode, recorded 1818 tracks of alpha-like particles in 72 hours during the electrolysis. This however, as Oriani discovered after my departure, was due to a contamination. Not surprisingly, I failed to replicate these results in two experiments at home. But Richard's recent results are remarkable; he continues observing anomalous alpha-like particles below the cathode. In a recent message (3/21/05) he sent me a table showing that such particles were observed in 9 out of 9 experiments. The recording rates, expressed in tracks/cm^2/hr, fluctuated from one experiment to another with the mean being 1.70 and standard deviation being 0.71. Nine control experiments, with detectors placed far away from the electrolytic cell,

showed fluctuating backgrounds. The mean background, also in tracks/cm^2/hr, and standard deviation, turned out to be 0,27 and 0.083, respectively. That is a very impressive result; the difference between the two means -- 1.7 and 0.27 -- seems to be real, as far as statistical fluctuations are concerned. But that, as always, has nothing to do with possible systematic errors.

In another message (3/26/05) Oriani described two additional control experiments. In these experiments detectors were placed below the cathode with no current flowing through the cell. A small amount of ThO_2 powder was added to the electrolyte (in a specially constructed cell and in another room) to test a theoretical prediction of John Fisher. I am mentioning this fact because recording rates, first with zero current (no electrolysis) and then with the current of 180 mA, are interesting. The zero current results, in two different cells, were not significantly higher than the mean background in the air, far away from the cell. Presence of an alpha radioactive substance in the electrolyte has no significant effect on the background below the cell. This is not surprising; the cathode thickness of 0.12 mm is larger than the range of alpha particles. The 180 mA results also turned out to be not significantly different from what was measured, with the same current, without thorium.

3) Description of my recent findings:
In trying to replicate Oriani's recent results I used the same concentration of the electrolyte (23 grams of Li_2SO_4 per liter of distilled water) and the same current (180 mA) as he. I conducted five new experiments, two controls and three tests. The first control (chip area 1.2 cm^2) was a zero-current experiment lasting three days. The second control was a 0.60 cm^2 chip situated in air, one meter away from the cell. The tests were two-days-long treatments. The results are shown in the table below. The 3th column shows the chip areas, the 4th column shows the numbers of preexisting (old) tracks, the 5th column shows the numbers of new tracks and the last column shows the average track recording rates.

Table 1 (Ludwik's data)

	exposure hrs	chip area cm^2	# of old tracks	# of new tracks	tracks/cm^2/hr
Control 1	72	1.2	23	59	0.70
Control 2	72	0.6	6	19	0.44
Test 1	48	1.2	20	65	1.13
Test 2	46	1.2	23	39	0.41
Test 3	47	1.2	24	50	0.87

The mean from my three tests, 0.80 tr/cm^2/hr, is not significantly different from the zero-current Control 1. It is clear that my attempt to observe alpha-like particles below the cathode, and due to the electric current, was not successful. The table below can be used to compare my raw data with his. It consists of five typical results; they were extracted from tables describing nine tests and nine controls.

Table 2 (Typical samples from Richard's data)

	exposure hrs	chip area cm^2	# of old tracks	# of new tracks	tracks/cm^2/hr
Control 1	47.5	1.0	3	19	0.33
Control 2	92.6	0.87	10	41	0.37
Test 1	23.5	0.6	2	20	1.4
Test 2	46.5	0.8	5	82	2.2

Test 3	47.8	0.72	4	62	1.8

One thing is immediately obvious. My old track densities, about 20 tr/cm^2, are much higher than old track densities reported by Richard. But my counting geometry (chips were ~1 mm from the cathode) was different from his (chips were ~6 mm from the cathode). Furthermore, my etching conditions (6.25 N, 75 degrees C and 6 hrs) were different from his (6.5 N, 80 C and 2 hrs). And on top of this I counted all tracks while Richard counted very dark tracks only. Taking all this under consideration, I think that my results neither contradict nor confirm Oriani's new results. They are not as good as his. I should conduct experiments with chips that also have smaller numbers of preexisting tracks.

4) Discussion:
If I were to take the results of Table 1 seriously (I am not) I would say that the effect has nothing to do with electric current. In that context the first 72-hours-long experiment would be a test, not a control. The only control would be a chip in the air, away from the cell. By taking this attitude I would say that the four chips below the cathode (mean 0.78 tr/cm^2/hr and standard deviation 0.30) record something that can not be attributed to background (0.44 tr/cm^2/hr). But how can one draw such conclusion from a single set of data? Like Oriani, I should have many controls and many tests. The effect that is only one standard deviation stronger than the background cannot be taken seriously.

The issue of irreproducibility has been discussed by Edmund Storms; his papers can be downloaded from the library at <www.lenr-canr.org>. Storms keeps emphasizing that cold fusion phenomena depend on NAE (Nuclear Active Environment), a mysterious ingredient that has not yet been identified. And he is convinced that the situation will improve rapidly after the NAE is discovered. If he is correct then cold fusion has no precedence in nuclear science. Other phenomena, such as artificial radioactivity, heat generated by radium, neutrons and fission, did become reproducible shortly after they were discovered. On the other hand, one might speculate that the cathode we used in November had more NAE than cathodes we used in March. As an experimentalist I will accept the NAE after it becomes something specific; for the time being it seems to be nothing else but a philosophical idea. For the time being I will expect our cathodes, (made from 99.99% pure nickel) to be interchangeable.

As I indicated above, the discrepancy between our new results might be due to differences in etching and differences in deciding what to count and what not to count. I counted all tracks, large and small, while Oriani counted very dark tracks only. Suppose that tracks that are small, after the first etching, become large after the second etching. In that case old tracks can easily be counted as new tracks. We are currently discussing ways to make our future experiments more compatible and less subjective. Hopefully both of us will arrive to more or less identical conclusion. After studying three different effects (nuclear particles in the liquid, in the vapor and in the air below the cathode), Richard now thinks that we should focus on the third effect because it seems to be the most reproducible.

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211) Fatal confusion about cold fusion

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Charles Beaudette, the author of a well known book about Cold Fusion, made interesting comments on the second DOE report. His paper can be downloaded from the Library at <html://www.lenr-canr.org>. Let me focus on some ideas discussed in that paper. His observation that the "Office of Science did not have a choice in this matter [selecting the cold fusion panel] given the pariah status of the field" is questionable. The DOE had many choices, selecting a panel whose task was to express opinions was only one of them. A better choice, as I wrote in unit # 196, would be to select two teams to perform critical cold fusion experiments. I will have more to say about this later. Claims based only on opinions are not as convincing as claims based on reliable experimental data.

I agree with Baoudette that "there is a considerable level of confusion as to how the field of cold-fusion research began, and how its several parts now relate to one another." He is making this observation on the basis of what the DOE referee wrote in their individual reports. Some anonymous referees seem to be unaware that two different claims were made, sixteen years age, under the same label. The first was the discovery of the unexplained excess heat (by Fleischmann and Pons) and the second was the discovery of unexplained neutrons (by Jones and his team). The label cold fusion, introduced by Jones, was a natural consequence of his earlier investigations of muonic molecules, and of highly compressed matter.

But imposing the same label on research devoted to excess heat was totally inappropriate. In a recent interview (see item # 208) Fleischmann said that the term cold fusion "was pushed on us by other people." This, however, was not at all obvious sixteen years ago. Fleischmann and Pons themselves were promoting the idea that excess heat is accompanied by emission of neutrons. Here is how this tragic confusion, now a piece of history, is described by Beaudette: " By pretending that they were experimental physicists during a few weeks prior to the announcement, the two chemists made mistakes in their attempt to measure neutron radiation. They erred badly collecting data, and their errors were quickly discovered and emphasized." The resulting consequences are well known. Even today, as illustrated by Beaudette, the claim of excess heat is often confused with the never-made claim that excess heat is due to thermonuclear reactions. The main issues are:

a) Is it true that the amount of thermal energy, released in some electrolytic experiments, exceeds the amount of electric energy received during the electrolysis?

b) Can the excess heat, if confirmed, be explained by chemical reactions taking place in materials from which electrolytic cells are constructed?

And additional non-chemical issue has to do with reality of nuclear particles, such as neutrons, protons, tritons and alphas, claimed to be emitted from solid metals loaded with hydrogen ions. The first issue has to do with calorimetry, the second has to do with quantitative chemical analysis, and with chemical thermodynamics. I see no reason why the DOE could not find a group of experts able to answer the above two questions in its existing laboratories. That would be much more useful than creating a panel whose task was to express opinion. Another panel, composed of physicists would be assigned a task of either confirming or refuting emission of nuclear particles from hydrogenated metals.

Expertise and equipment for addressing the above three issues were, and still are, available to the DOE. I agree with Baudette that it was a mistake to initiate a peer-review work of the cold fusion field by panelists who "were unfamiliar with its technical development, leading scientists, significant experiments, and principal papers." I also agree with his

criticism of approaches used by some panelists. Criticizing experimental findings on the basis of conflicts with the accepted "canon of nuclear science" is certainly not consistent with our scientific methodology.

To end this unit let me address two other topics raised in Baudette's paper: the issue of the "burden of proof' and the issue of "reproducibility." I disagree with him that those who criticise nuclear interpretation of excess heat, and who suspect that the heat might be due to something else, should "have a duty to elucidate possible [chemical or] storage mechanisms as disturbing artifacts." Their duty is to mention such artifacts, the burden of showing that the artifacts are not responsible for what is observed is on those who claim that the effect is highly unusual, in terms of our current paradigm.

Addressing the second issue Baudette writes: "Concern for experimental reproducibility (repeatability) sometimes overshadows experimental results in cold-fusion research, both among its practitioners and its critics." That is certainly true. Franco Scaramuzzi, an esteemed hot-fusion physicist who practiced cold-fusion research for fourteen years, wrote: "a) I agree that reproducibility is a 'must' in experimental research; b) however, a new field, at it beginning, is often characterized by a lack of reproducibility , and it is the task of the scientists operating in the field to understand what is going on, in order to pursue reproducibility." I agree with this. In my opinion, however, a field in which reproducibility (of experiments conducted by qualified researchers) is poor is not yet scientific. Protoscience should not be confused with science.

Unlike pseudoscience, the term protoscience is not pejorative. Many alchemists and astrologists were protoscientists before chemistry and astronomy became scientific fields. Cold fusion research will become scientific when at least one of its numerous discoveries is recognized as highly reproducible. I like how Beaudette refuted claims, made in 1989, that Fleischmann and Pons were offering only pseudoscience. He refers to Lagmuir's description of pseudoscience and contrasts it with what was actually stated, or not stated, in the published reports. Beaudette also reminds us that "Langmuir says nothing about reproducibility." His paper ends with an extensive list of references.

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212) Answering four questions

Ludwik Kowalski (15/4/05)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

This morning I received a set of survey questions, from a friend in Europe. Here are my answers:

- WHAT IS COLD FUSION (LENR, CANR, CMNS)?

It is certainly not pseudoscience, as many think. It is protoscience claiming that nuclear activities result from some chemical processes, such as those taking place in electrolysis, or when hydrogen ions diffuse through some solids. Kinetic energies of such ions, and dominant products of reactions, are believed to be very different from those observed in the so-called “hot fusion.” The term hot fusion usually refers to thermonuclear reactions, and to reactions in which ions are electrically accelerated before colliding.

- HOW DOES IT WORK?

I do not know.

- WHAT CHANCES DOES IT HAVE TO BE SCALED UP TO A TECHNOLOGY?

Impossible to predict at this time.

- WHAT HAVE WE TO DO IN ORDER TO ATTAIN THIS?

Experimental results, performed by qualified researchers, must become 100% reproducible anywhere and any time. Protoscience will then become part of mainstream science. Also see items #210 and # 211 at my cold fusion website:

http://blake.montclair.edu/~kowalskil/cf/

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This website contains other cold fusion items.
Click to see the list of links

213) More about Energetics Technologies

Ludwik Kowalski (20/4/05)
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Yesterday I visited Energetics Technology (ET), a small private company in Israel (subsidiary of an American company) conducting cold fusion research (previously described in unit # 204). The company is also interested in magneto-hydrodynamics processing in metallurgy, a field with which I am not familiar.

Two things impressed me the most. The first was the high caliber of technical expertise and equipment assembled at ET in the last three years to deal with several aspects of cold fusion. The second was the difficulty under which industrial research is conducted. I am familiar with scientific research at universities and in national laboratories; the area of industrial research is new to me. Academic researchers usually have tenure; scientists conducting industrial research here work under a contract that is expected to be renewed each year. This, I suppose, has to do with the high risk of investing in a controversial technology.

The company which is focused on the use of a special wave form (SuperWave) proposed by one of the company's owner, Dr. Dardik, is headed by Shaul Lesin, an expert on magneto-hydrodynamics. The second in command is Arik El-Boher (see photos below), conducting research in cold fusion. He is focusing on glow discharge cells. Arik has six such cells working in parallel. The geometry of each cell is cylindrical -- the anode (molybdenum or tungsten wire) in the center and the cathode (pure or alloyed PVD coatings on stainless steel cylinder) approximately one centimeter away.

KEEP READING IF LOADING OF PICTURES IS SLOW


. . .


SHAUL AND ARIK

At one point I said that my choice would be to have a cathode in the center (to increase the electric field near it). Arik indicated that such geometry was tried but the results were not better than with the presently used cells. He believes that the

area of the cathode must be maximized to enhance excess heat. Arik hopes to obtain better results from the ongoing experiments in which thin layers, and sets of layers, are deposited on cathodes.

I also had a chance to talk with other four researchers: Tanya Zilov, Boris Khachturov, Vitali Krakov and Mark Tsirlin. All of them were educated in the former Soviet Union. Tanya is an electrochemist, Boris is a nuclear physicist, Vitali is a physical chemist while Mark is a material scientist. I can not imagine a better set of specialists hired to investigate practical aspects of cold fusion. Each of them spent about an hour with me, explaining their setups and talking about results. The pictures of these scientists are shown below.

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TANIA, BORIS, VITALI AND MARK

Tanya has a set of nine electrolytic cells (heavy water and LiOD) working in parallel. Each cell is equipped with a commercial recombiner (turning oxygen and hydrogen into D_2O) and with a set of temperature sensors. Loading of deuterium ions into cathodes is controlled by measuring cathode resistances. Rare episodes of very high efficiencies (excess heat of up to 2500% of electric energy supplied) at low reproducibility rate, described in unit # 204, were obtained with electrolytic systems driven by superwaves. Like many other cold fusion researchers, Tanya does not know why large excess heat is not observed more frequently. A reproducible setup generating excess heat at the efficiency of more than 100% or 200% would probably lead to a commercial success. Recently in one of their ultrasound electrolytic cell experiments (after analyzing the experimental data), they achieved an efficiency as high as 1200%. These results will be presented in the coming workshop May 2005 on cold fusion in Siena, Italy.

At present she and Boris are trying to irradiate cathodes of electrolytic cells with ultrasounds. The idea is to expose palladium cathodes to cavitational bubbles. Preliminary results from this approach are very encouraging. Excess heat generated at the level of several watts, she said, is 100% reproducible, but only when the efficiency is low. More specifically, the 100% reproducibility was recorded for electrolytic cells in which the cathode is Pt and the anode is Pd. They call this arrangement reversed because Pd and Pt are most often used as a cathode and anode, respectively. The highest efficiency has been14.5%. In my opinion this is a monumental achievement. But Tanya is not satisfied; the common efficiencies of ~10% she said, are too low to be practically useful. In my opinion, focusing on what is 100% reproducible would be very useful at this stage. A Russian saying "tishe jedesh dalshe budiesh" (those who advance slowly will get there sooner) seems to be applicable in this case.

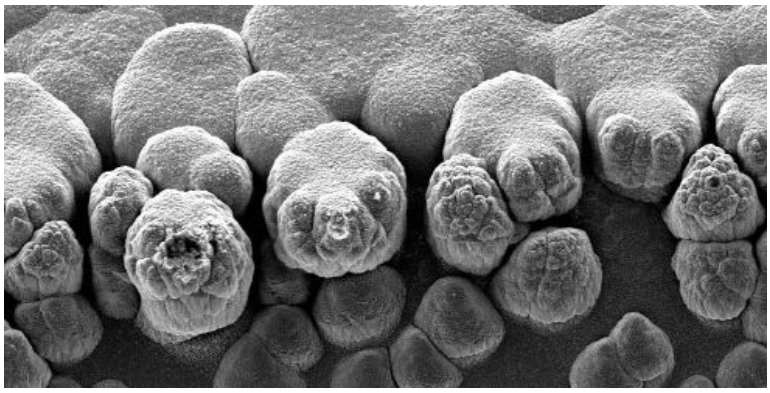
The error of measuring excess heat, in Tania's and Boris' experiments, is close to 1% -- this is good enough to conduct systematic investigations at the level of ten percent efficiency. That is what I would do in such a situation. Taking advantage of 100% reproducibility I would systematically investigate effects of various parameters, such as concentration, temperature, pressure, current density, etc. I would not worry about low efficiencies. Then I would publish the results. Yes, I know that people directing industrial research might be reluctant to share essential information; academic research, with which I am familiar, is different from research focused on commercial success. My goal would be to identify the mystery of the NAE (nuclear active environment) that, according to Storms, makes generation of excess heat possible. In other words I would take advantage of the achieved experimental reproducibility and try to turn protoscience into science. That seems to be the most urgent issue in the area of cold fusion.

Tanya said that highly reproducible Pd/D loading results were obtained by using superwaves, rather than commonly used dc. Furthermore, the highest excess heat has been generated using superwaves with very low current density. My first reaction to this was to think about the power factor (cosine phi due to phase shift between the current and the voltage) at different

frequency components. I asked Arik about this. He said that they do not worry about power factors because sampling rates in measuring voltages and currents are at the level of 50KHz. This is well above the highest frequency components (up to 1 kilohertzs) of their superwaves. Furthermore, calibration experiments with dc and with superwaves were in very good agreement (less than 0.5%), using 1 MHz Yokagawa power meter.

Vitali specializes in catalytic materials and in gas loading. If I understood him correctly, he was not able to reproduce the effects discovered by Case and by Paterson. These effects were described at the 10th cold fusion conference (ICCF10). He works with compressed palladium powders in dielectric and conducting materials. Mark specializes in the analysis of materials. Using sophisticated analytical instruments, available at Ben Gurion University and at other Israeli Universities centers, he performs both elemental and isotopic analysis. No evidence of alchemy (production of new elements) or abnormal isotopic ratios was confirmed by his investigations, even in cathodes that generated spectacular amounts of excess heat.

He explained to me why results of elemental analysis are so difficult to interpret. For example, relative squares of the X-ray spectroscopic peaks (EDS method) strongly depend on local topography of the surface bombarded by electron beam as well as on sizes and phase composition of analyzed area with rather small impurities content. Therefore the obtained results fluctuate widely as one examines different spots even on the same surface. This complicates comparisons of impurities concentrations for different samples. That factor is usually not mentioned by those who report on nuclear transmutations. But Mark is fascinated by structural changes on palladium surfaces resulting from such processing. Many of his SEM pictures have artistic values, as illustrated below.



_ . _ . _ . _ COLD FUSION IN HEBREW _ . _ . _ . _ A SCANNING ELECTRON MICROSCOPE PHOTO

Appended on 1/13/2008

Steve Krivit, a scientific reporter, and the editor of New Energy Times, published an interview with interesting information about Energetic Technology. It is item #7 in the just-published issue #26 of his online journal.

http://newenergytimes.com/news/2008/NET26.htm

The title of the item is “Interview with Irving Dardik and Alison Godfrey of Energetics Technologies LLC.” The journal website is:

http://newenergytimes.com/news/news.htm

This website contains other cold fusion items.
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214) That is not cold fusion!

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Department of Mathematical Sciences
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1) The following reprint was posted on Phys-L, a discussion list for physics teachers:

Observation of nuclear fusion driven by a pyroelectric crystal
B. Naranjo, J.K. Gimzewski & S. Putterman
NATURE _434_ page 1115 (28 APRIL 2005)

While progress in fusion research continues with magnetic and inertial confinement, alternative approaches such as Coulomb explosions of deuterium clusters and ultrafast laser plasma interactions also provide insight into basic processes and technological applications. However, attempts to produce fusion in a room temperature solid-state setting, including cold fusion and bubble fusion, have met with deep skepticism. Here we report that gently heating a pyroelectric crystal in a deuterated atmosphere can generate fusion under desktop conditions. The electrostatic field of the crystal is used to generate and accelerate a deuteron beam (>100 keV and 4 nA), which, upon striking a deuterated target, produces a neutron flux over 400 times the background level. The presence of neutrons from the reaction D + D ==> 3He (820 keV) + n (2.45 MeV) within the target is confirmed by pulse shape analysis and proton recoil spectroscopy. As further evidence for this fusion reaction, we use a novel time-of-flight technique to demonstrate the delayed coincidence between the outgoing a-particle and the neutron. Although the reported fusion is not useful in the power-producing sense, we anticipate that the system will find application as a simple palm-sized neutron generator.

See also:
http://www.nature.com/nature/journal/v434/n7037/abs/nature03575.html
http://www.nature.com/nature/journal/v434/n7037/edsumm/e050428-06.html

2) Yesterday, replying to the above, I wrote: “The most interesting is generation of high potential differences by small differences in temperatures. I was familiar with piezoelectric, but not with pyroelectric, crystals.” Then I discovered that Google points to many references on that subject. One paper (see the URL below) prompted me to write the following private Email message:

“I enjoyed reading your description of the new neutron source at:

http://www.nature.com/nature/journal/v434/n7037/full/4341077a.html

Can I have your permission to post it on my website? That website:

http://blake.montclair.edu/~kowalskil/cf/

is devoted to cold fusion but I am sure that people interested in that controversial subject will also be interested in what you wrote. As you probably know, several years ago CF researchers were discussing high electric fields (due to + and - charges across different highly localized surface defects) as a possible cause of cold fusion.”

Here is the reply from Dr. Mike Saltmarsh:

"The copyright to my article belongs to Nature, so I'd guess that it is their permission not mine that you need. Of course you need no permission to post a reference to the article on your website. However as you are presumably are aware, the pyroelectric generator idea has absolutely nothing in common with the cold fusion furore. There is no new physics involved, indeed fusion has been produced by beam-target interactions for decades. It's easy, but regrettably useless for energy production.

Parenthetically, I was involved in the original cold fusion fiasco, and had overall charge of the effort to investigate the phenomenon at ORNL. I was absolutely stunned to find that so many apparently competent researchers seemed to suspend their critical faculties when presented with an exciting result. **Do not underestimate the power of self delusion! Irreproducibility is still a key indicator of a lack of understanding.**"

Responding to the above, and hoping to enrich this item, I asked Mike to elaborate on the power of self-delusion. He wrote: "I don't think that I will amplify what I said before. To do justice to the topic I'd need to collect some references and do a decent job for you, and I'm not willing to put in the time right now. If you're interested, you might like to look at the history of "polywater", which caused a furore about 20(?) years ago. Or read Steven J Gould's article on the measurements of the speed of light as a function of time (I think it's in his book The Mismeasure of Man). Good luck with your website. You can quote whatever I've emailed to you." Too bad; input from a retired national laboratory scientist, who was in the center of a recent controversy about cold fusion via ultrasounds, would be interesting.

The controversy is not over; researchers from Energetics Technologies (see item #213) did say that ultrasonic cavitation was an important factor in their excess heat experiments. The first topic on the agenda of the upcoming cold fusion workshop at MIT (May 21, 2005) is "Acoustic-induced Cold Fusion Experiments." Russ George, who I met at ICCF11, has a website <http://www.d2fusion.com/> at which he writes that tiny-heavy-water bubbles, collapsing on metallic surfaces, cause cold fusion. Responding to the search phrase <nuclear "bubble-fusion"> Google delivered 771 references. Yes, I know that sonofusion might turn out to be hot fusion, and that it has nothing to do with the title of this unit. But the connection is obvious: from tiny bubbles to ultrasounds that produce them; from ultrasounds to piezoelectric generators, and from piezoelectricity to pyroelectricity.

This website contains other cold fusion items.
Click to see the list of links

215) Technology -- solar versus cold fusion

Ludwik Kowalski (5/3/05)
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After spending a day at Energetics Technologies (see item #213) I had a chance of visiting Jacob Blaustein Institute of Desert Research (Israel). It is located at the Sede Boker campus of Ben-Gurion University. This visit made me think about differences between applications-oriented research in the field of solar energy and in the field of cold fusion. In the former case research is based on science, in the later case it is based on protoscience. Not a single cold fusion application has been commercially successful, so far, while progress in the area of solar energy has been enormous. Will practical applications of cold fusion start to appear after the field becomes scientific? I hope so. But for the time being emphasis must be on science, not on technology. I am particularly impressed by recently published roadmaps for implementations of photovoltaic (PV) technology in different countries, as described in

<http://www.epia.org/05Publications/OtherRoadmaps.htm>

They indicate that solar batteries are likely to become dominant suppliers of all our electric needs, including industry, before the end of this century. The American roadmap, entitled “Our Solar Future,” is downloadable as:

<http://www.seia.org/media/pdfs/pvroadmap.pdf>

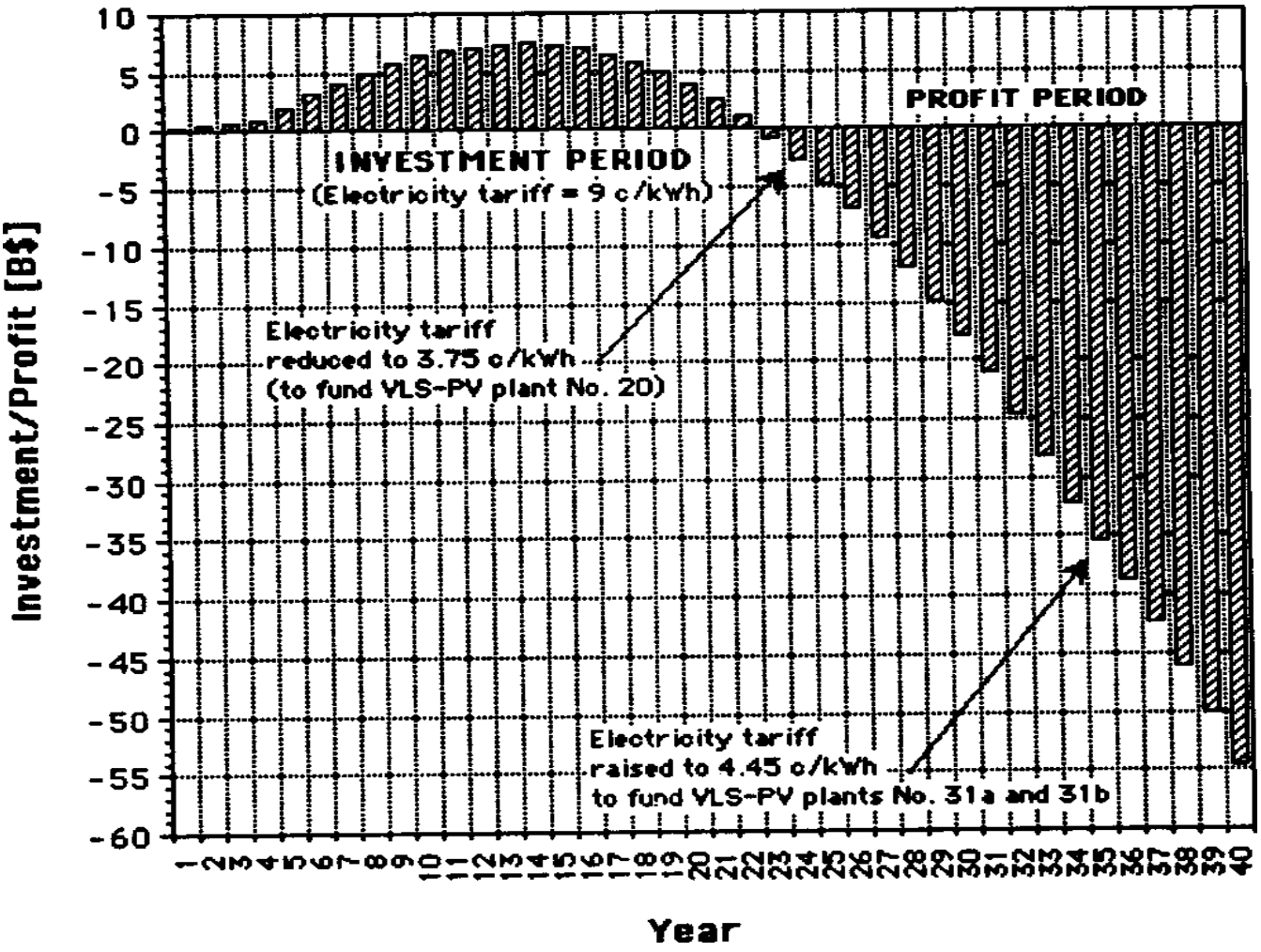

David (pointing to a mirror) and myself below the concentrator.

According to Dr. David Fainman, the director of the Israeli solar research center at Sede Boger Campus, the problems of implementation

are no longer technological; they are primarily financial. He gave me the preprint of a paper to be presented at the International Conference on Solar Concentrators for Generation of Electricity or Hydrogen. That invited paper describes a proposed financial model for a country like Israel. I am sure that this paper will be available over the Internet after the conference (May 1-5, Scotsdale, AZ, USA).

Solar concentrators, by the way, are computer-controlled reflectors that focus solar radiation. A photovoltaic panel, receiving concentrated radiation, is a set of solar batteries. I wish I had time to ask technical questions after the brief description of the financial model. One of my questions would be about the need of cooling. To keep the cells cool one must constantly remove a lot of heat. This would probably not be necessary if inexpensive mirrors were replaced by very expensive batteries of cells.

According to the model used by Fainman, the CPV (concentrated photovoltaic) technology will pay off the expenses, and will generate profits, after 23 years. This is illustrated in the figure below.



Credit line during the first 40 years. (Negative values show profits in billions of $ per year)

The figure, describing the forecast for a country like Israel, is based on a set of assumptions. Some of these assumptions are:

a) Subsidy-free financing, at an interest rate of 5%, “via an open credit line that is repaid with interest entirely from revenues that accrue from a steadily increasing number of [very large] power plants.”

b) It will take four years to build a facility to make large solar power plants.

c) “Starting from Year 5, one 1000 MW plant is erected per year.” The first erected plant will start to generate revenue in Year 6, the second will start to generate revenue in Year 7, etc.

d) To start off, electricity is sold at approximately the prevailing market tariff, until the credit line has been paid off.” All the revenue is used to pay for the the operation and maintenance, for general and administration expenses, and for the credit line with interest.

e) The cell efficiency will be 32% and “plants will have 30-years lifetimes without undergoing degradation.” In reality degradation is

expected but the initial efficiency of cells is expected to increase gradually in the next 35 years.

I was not aware that 32% efficiency is already possible today, and that 50% efficiency is expected in 35 years. But I will assume that Faiman, and his coauthors, are not science fiction writers. They probably have good reason to believe that modeling assumptions are realistic. The Internet is full of information about photovoltaic cells and I should learn about new things. Here are some links:

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http://www.eia.doe.gov/cneaf/solar.renewables/renewable.energy.annual/backgrnd/chap11i.htm
http://www.fsec.ucf.edu/pvt/pvbasics/index.htm
http://www.sandia.gov/pv/
http://www.fsec.ucf.edu/pvt/
http://64.243.182.248/includes/pv%20tutorial.pdf
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The main point is that solar electricity is already a practical reality on a small scale, and that it is likely to become a dominant source of electricity in many countries. That is good; fossil fuels will soon be too expensive to compete with solar energy. Dreams about abundant, and pollution-free (?), energy seem to be turning into reality. Yes, scientific and practically-oriented research, in the area of solar energy, were initially subsidized (and are still subsidized) by governments and private agencies. But subsidies would not materialize if experimental results were irreproducible, as they still are in the areas of cold fusion.

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216) Too good to be true?

Ludwik Kowalski (5/5/05)
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Montclair State University, Upper Montclair, NJ, 07043

One of the most important achievements of the 19th century physics was the discovery that a well defined amount of mechanical work is needed to generate one unit of heat -- not less and not more. Joule demonstrated this in a well known paddle wheel experiment. Water was forced to flow through small holes in the paddles of a rotating wheel and the amount of heat was measured calorimetrically. But, at a recent conference, a Russian scientist, A.I. Koldamasov (1), described an exception from this rule. He claims to have a device in which the heat to work ratio is twenty times larger than what would be allowed according to Joule's equivalence rule.

His device, named reactor, is similar to Joule's paddle wheel. But water, free of ions (resistivity 10^{11} ohm*m), is forced to flow through holes (diameters 1-2 mm) drilled in a plate made from the dielectric material (rather than from a metal). After reviewing basic concepts of thermonuclear physics Koldamasov writes: "On the basis of the above theoretical ideas, a nuclear reactor producing energy of nuclear fusion reactions was designed and tested (see Fig. 1). The reactor operation is stable and well controllable. It gives 20 units of useful power per unit of supplied power. Furthermore, it is possible to obtain power both in the form of heat and directly electric energy, omitting steam water cycle. According to the consumer's needs, the electric energy can be of both direct and alternating current. [Russian patent reference is given.] . . .

The working fluid is fed by a gear pump under the pressure of 5 to 7 MPa; the channel is 25-30 cm long, and the orifice diameter is 1-2 mm. By changing the electric motor rotation rate, we change the frequency of flow pulsation's and reach the resonance frequency of the orifice, which causes intensive cavitation. Under the action of this cavitation, the plate material emits electrons from the input edge, which are carried away by the flow, and at the output edge a high-density positive charge is formed (see Fig. 6). The charge is annular, its density is practically uniform, and it constitutes a medium temperature plasma ~10000 K with the density ~10^4 J/cm^3. If heavy water possessing the same dielectric properties as the working fluid is added to the outflowing medium, then nuclear fusion reactions arise in the zone of influence of the charge."

What kind instruments were used to demonstrate occurrence of nuclear reactions? I was not able to find the answer to this question in the Koldamasov's paper. Here is how this paper ends: "The number of deuterium atoms collisions, and hence the magnitude of the released energy, depends on heavy water concentration in the working fluid. In our experiments we used a mixture with the ratio 1:100 (1 part of heavy water per 100 parts of light water). The continuous operation time was up to 100 hours. The experiments are entirely reproducible." Who is Koldamasov? Where were his experiments performed? Who are his coworkers? How much of "excess energy" was generated in 100 hours? Not knowing how to answer such questions I turned to the Internet.

The first item that came up, after typing "Koldamasov" indicates that reproducible results were first reported by the author seven years ago. Here is the extract from what was delivered by Google: "Six authors who stated 100% reproducibility of the effects detected are Bazhutov, Kanarev, Karabut, Savvatimova, Notoya, and Koldamasov." And here is another earlier reference (2). On Hideo KozimaÕs website:

http://web.pdx.edu/~pdx00210/News/CFRLEngNews/CFRLEN35.htm

I see a list of papers read at the 9th Russian Conference on Cold Nuclear Transmutation of Chemical Elements (September/October, 2001). It contains references (3) and (4). The session at which these papers were presented was chaired by A. B. Karabut, the author of what I described in item 13. The device is said to generate twenty times more electric energy that is needed to run its motor. Why is it not manufactured and used all over? I suspect that the 100% reproducibility claim was found to be highly exaggerated. Somebody probably tried and failed to produce a commercially successful gadget.

References:

1) A.I. Koldamasov at "Russian Conference on Cold Fusion and Ball Lightning," Sochi, Russia, 2002.

2) A. I. Koldamasov. Nuclear Fusion in Electrical Charge Field. Fundamental problems of natural science and engineering. Volume 1. St.-Petersburg, 2000. 167 pages."

3) Koldamasov A.I. "Principles of Work of New Type Nuclear Reactor."

4) Baranov D.S. "Investigation of the Radiation Effects in the Koldamasov Cell."

ADDENDUM:

While searching on the Internet I found a useful compilation about "who is who" in Russian cold fusion research. The authors are I.V. Goryachev, and Y.N. Bazhutov; the title is " Organization, current status and main results of Russian research in cold fusion and transmutation of chemical elements." It shows that the author of the last reference, a Ph.D. physicist, is from the Research Institute of High Temperatures, Russian Academy of Sciences in Moscow. His specialty is listed as experimental " investigations of the methods of initiating nuclear reactions in deuterated dielectric liquid under the conditions of cavitation." The name Koldamasov does not appear in the downloaded document: <GoryachevIorganizati.pdf>

Appended on 5/22/05:

1) On 5/13/05, after finding the sentence quoted below (at the website of CNW -- Group Canada NewsWire), I sent an e-mail message to Dr. Yang, the Chairman & Founder of the Hy-En Group of companies:

Dear Dr. Hyunik Yang:
Browsing the Internet (before going to the MIT cold fusion colloquium next Friday) I found this description of your commercial activities:

". . . The three main applications derived from "Cold Fusion" are massive production of low cost hydrogen and heat energy for commercial and industrial applications steam or hot water and electric power..... "

Please provide a reference (or references) about the method used to obtain electric power via cold fusion. Thanks in advance,
Ludwik Kowalski

2) The above message was not answered. During the MIT colloquium I heard that Koldamasov died and that his invention is going to be used by a company whose name is iESiUSA (Innovative Energy Syatems Inc.). Materials on the company's website<WWW.iesiusa.com> do not refer to clod fusion. But they are worth quoting.

a) "Innovative Energy Solution Inc. is a leader in developing reliable next generation, clean energy technologies. iESi owns several patents related to its proprietary hydrogen generation, heat generating and waste heat recovery technologies."

b) "The new clean energy plant will enable Norwood Foundry to generate six times (12 MW) more electricity than it consumes (2 MW) at its foundry located in Nisku, Alberta, Canada." That is indeed extraordinary - consuming

electric energy at the rate of 2 MW and generating it at the rate of 12 MW, presumably at the same time. This would be a giant perpetual motion machine. Recall that Koldamasov's device had the output/input ratio of twenty, not six. But even a factor of two would be extraordinary.

c) "The revenue to be generated through the joint venture project is expected to exceed $6 million annually. Under the joint venture, iESi is responsible for the implementation of its revolutionary clean energy technologies, while Norwood [the old existing foundry] will finance the project.....The plant is slated to be fully operational by the third quarter of 2005..... iESi owns several patents related to its proprietary hydrogen generation, heat generating and waste heat recovery technologies. Headquartered in Las Vegas, Nevada, iESi also has offices in Canada, Europe and South Korea." I hope that my retirement savings are not invested in iESi.

d) Innovative Energy Solutions Inc. (iESi) today announced that it raised $3 million in its first round of financing. The investor group consists of more than 300 individuals from Canada, Europe and the United States. iESi will use the investment to accelerate product development, push market penetration and establish manufacturing facilities in Alabama. 'The success of our initial funding is encouraging to iESi as we focus on executing our business endeavors and growing the company. The first round closed with an oversubscription of 200,000 shares, which demonstrates a substantial interest in our energy generating technologies and what they can do for a fuel-based economy,; said Ron Foster, chairman, iESi. iESi will open its second round of financing on July 29, 2004, by offering 500,000 shares at $4 apiece. Established in 2003**,** iESi's mission is to offer clean, viable solutions to traditional energy sources, and reduce the world's dependence on oil from the Middle East. iESi is organized into three broad divisions to maximize return on its intellectual capital and intellectual property."

Too good to be true? Yes, I think so. But the company website provides many claims that are worth discussing with students. Physics teachers should take advantage of this. Something is not right in this business of promoting nonexistent technologies. In unit #224 I referred to a colloquium presentation of Robert Rines, the patent counselor at MIT. At one point he asked a question; "what harm can possibly result from granting a patent whose validity cannot be established at sufficiently high level of certainty?" He was referring to patents in the area of cold fusion. Nobody, he said, will be stupid enough to invest in things that are still uncertain. I now tend to disagree. A granted patent gives some kind of legitimacy to unjustified commercial claims. Patents can be viewed as instruments of protection of citizens. Yes, I know that such instruments are not very effective. But they are better than nothing. Citizens should also be economically protected by other legal means.

Addendum (5/25/05):
Here is the content of a document, dated as 5/23/05, that I found on the Internet. It provides background information about promoters of cold fusion technology.

EDMONTON, Alberta, May 23 /PRNewswire/ -- Innovative Energy Solutions, Inc. (iESi) today announced the arrival of the Company's co-founder and Chief Technical Officer, Hyunik Yang, Ph.D. and Dr. Nahm Cho in Sherwood Park, Alberta, just southeast of Edmonton. The recent relocation of Dr. Yang and Dr. Cho should expedite the efforts that are already well underway on the Direct Electrical Power, Heat and Hydrogen Generation units and allow the Company's team of scientists to advance the impressive work they accomplished for iESi while in South Korea.

"Much of iESi's proprietary intellectual property is based on the research in the field of quantum energy generation authored by Dr. Yang," said Patrick Cochrane, CEO, iESi. "Drs. Yang and Cho and our team of scientists are continually conducting research to enhance iESi's technological offerings and we look forward to their continued presence in Edmonton."

"Dr. Cho and I are pleased to finally be here in Canada. We're eager about working closely together with our scientific and management teams to implement such a worthy offering that will allow the world to go from dependence on energy providers to energy self-sufficiency," said Dr. Hyunik Yang, Chief Technical Officer, iESi.

As concern grows for the world's natural resources, energy efficiency is gaining worldwide attention. iESi is poised to be the leader in the development of innovative energy solutions through its safe and patented plasma processes which

included Direct Electrical Power Generation, low-cost Hydrogen Generation and low-cost Heat Generation, all three of which were developed by Dr. Yang.

In his career, Dr. Yang has also held positions as professor at Hanyang University and as senior research engineer at Hyundai Electronics. Dr. Yang has designed several new inventions in the field of quantum energy and cold fusion. Dr. Yang received his Engineering B.S. from Hanyang University in South Korea, and completed his Engineering M.S., Ph.D. and post-doctoral work at Columbia University in New York. He is a member of the American Society of Mechanical Engineers, Society of Automobile Engineers, Korean Society of Mechanical Engineers, Russian Academy of Natural Science, Korean CAD/CAM Society and the Korean Society of Machine Tool Engineers. Since 1997, Dr. Yang has been listed in the Who's Who in the World, and in the Who's Who in Science and Engineering since 1998.

Dr. Cho also held positions as professor at Hanyang University. Dr. Cho is the author of four patents in South Korea and is an expert in the field of Nuclear Transmutation and Fusion. Dr. Cho received his Precision Mechanical Engineering B.S. and M.S. from Hanyang University in South Korea, and completed his Ph.D at the Tokyo Institute of Technology. He is a member of the Japan Society of Mechanical Engineers, Japan Society of Precision Engineers, Korean Society of Mechanical Engineers, Korean Society of Precision Mechanical Engineers, Korean Society of Manufacturing Engineers and the Micro Biochip Center.

About iESi
Innovative Energy Solution Inc. (iESi) is a leader in developing next generation clean energy technologies. iESi owns several patents related to hydrogen generating technology, heat generating technology and waste heat recovery. The Company's Corporate Offices are in Las Vegas and iESi also has offices in Canada, the United Kingdom and Europe. For more information please visit: **http://www.iesiusa.com**

Go to item #226 (for continuation of this unfolding story).

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217) An Ukrainian Connection

Ludwik Kowalski (5/7/05)
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Research of Adamenko and his associates was described in unit #168. At the Informal Workshop organized by the University of Bonn (IZKS Center, February 6-7, 2004) that work was described as follows:

THE KIEV EXPERIMENTS ON LOW ENERGY NUCLEOSYNTHESIS

THE ABSTRACTS OF INVITED PAPERS

The physicists of the Kiev "Proton-21" laboratory of low energy nuclear physics (Dr. S. Adamenko and coworkers) claim evidence (through 5000 experiments, carried out since 1999) for an enormous number of "low energy nucleosynthesis" in a little ball of copper invested from all sides by 0.3 MeV electrons. The elemental and isotopic analysis of the reaction products done in Kiev was confirmed in laboratories of other countries (USA, Russia).

Starting from a 99.99 % pure ball of copper (0.01 % impurities) a large quantity of elements like ?, ?, La, Ce, Pt, Pb was produced. The total number of new nuclei is comparable with the total number of initial copper nuclei. The process has further amazing features: (a) The radioactivity of the resulting material is not different from the background activity and (b) transuranic nuclei are produced, including superheavy ones. The aim of the workshop is to present for the first time the Kiev results and discuss their significance, meaning and possible applications (e.g. energy producing, eliminating of radioactive wastes).

It is from: http://www.proton21.com.ua/articles/BonnAbstracts.pdf. Let me add that the website of the laboratory headed by Adamenko is: <www.proton21.com.ua/index_en.html>; the list of their publications can be seen at <http://www.proton21.com.ua/articles_en.html>. What follows is a more recent description of that work, as popularized in the online newspaper: Ukraine Now <www.ukrnow.com>. I am quoting the entire article because I do not know for how long it will remain at <www.ukrnow.com/content/view/4300/2/>. That description will probably remain important, even if the extraordinary claims (nuclear alchemy without radioactivity, etc.) turn out to be a false alarm. I would like to thank D.L. for informing me about the article shown below.

Taming a Neutron Star

Ukraine Now, Friday, April 22 2005

Ukrainian scientists claim they have tamed thermonuclear fusion.

It appears that Ukrainian scientists have made a discovery which could be as significant as the production of fire or the invention of the wheel by our ancestors. If their theory is confirmed, humanity will get a source of practically free energy which could be obtained in any quantity out of anything. This energy would be ecologically clean and result in small power plants.

The situation looks like something from a fantasy novel about an inventor of a perpetual motion machine in which a lonely enthusiast who does not care for recognized theories and authorities builds an engine of eternal action or a time

machine in a barn, or creates a life-giving potion. In real life we treat such characters skeptically. I have met dozens of them and disappointment has always been the result of such meetings. However, Kyiv scientists from Proton-21 have demonstrated results that leave one speechless.

This is a team of highly experienced experts and the equipment used in their laboratory exceeds the technical capabilities of many scientific research institutes. Bombarding a target with a relatively weak beam of electrons, they get results similar to what goes on inside neutron stars. The target collapses in on itself (shrinks), followed by an explosion and the brightest flash, nuclear transmutation (the occurrence of a great number of elements which the target lacked before) and a stream of various radiation. The energy of the explosion exceeds the initial energy of the election beam by a million times. One need only harness it, and while it appears that is not an easy task, it could be realized technologically.

I have no doubt that readers will cluck: "Thousands of physicists all over the world have been engaged in such experiments for the last 50 years, moreover, they are working with huge synchrotrons like the one at Serpuchov or the one at CERN. Countless billions of dollars have been invested in thermonuclear energy and now amateur "experts" in Kyiv have beat them all? Sorry ,but Icannot believe that !"For such educated readers we will repeat that synchrotrons have nothing to do with this discovery. Attempts to produce thermonuclear energy have been underway at Tokamak installations and laser compression systems in Russia at the Kurchatov Institute and in Saratov , as well as in the U.S. at Livermore and Los Alamos, and at a laser center in Kyoto ,Japan .

I went to a meeting with project chief Stanislav Adamenko with feeling similar sarcasm. I had taken along a stack of critical reviews of cold nuclear fusion because this project highly resembled that to me from the beginning. I cannot say that what Adamenko and his colleagues told me completely won me over, but his research has reached such a level that despite my skeptical attitude it would be negligent of me not to tell the public about the project. If their results are confirmed, if Proton-21 will achieve at least part of what they claim, the world will obtain a colossal source of cheap and clean energy. If the idea becomes a soap bubble, then we will forget it.

However, the photos of samples exploded from inside look very convincing. And the database with results of analyses based on almost 10,000 of successful experiments forces one to take them seriously.

Nuclear 'Lighter'

Here is what Adamenko says. As the scientist's speech is full of arcane terms and data which are clear only to nuclear experts, I will try to simplify his story.

Adamenko: First we have the goal of developing efficient and safe technology for using nuclear waste. We want to construct a sort of "lighter" able to force radioactive atoms to "burn down" and turn into "ashes." The first successful experiment was made on Feb. 24, 2000 , on non-radioactive targets. In a couple of months we found stable, non-disintegrating atoms of super heavy chemical substances among other particles in the screen that captures the target's fission products. Finally, in our installation which is located in a laboratory at the Institute of Nuclear Research of the National Academy of Science of Ukraine , we conducted successful experiments neutralizing radioactivity in 2002. We managed to exceed the boundaries of the initial topic.

So, why has humanity failed to tame thermonuclear reactions for almost all 60 years? Scientists are not able to find conditions to conduct a stable process. The whole world thinks that the main thing in the process is a race for more powerful ways to impact targets, and such an approach inevitably leads to a dead-end – targets exploded before a large-scale thermonuclear process managed to start. After many searches we succeeded in finding such a tool and we managed to start a reaction. In brief the idea looks like this – we punch the surface of small ball-like target with a powerful, but very short beam of electrons. The surface layer transfers the blow deeper inside, to more deeply located layers, and they in turn transfer the impact deeper. As the target has the form of a ball, the level of compression increases when the shock wave deepens into the substance. It looks like a tsunami – its wave is not too visible at the deep sections of ocean, but it rapidly increases in shallow places, rushing onto the beach with huge waves.

The shock wave in the ball behaves in the same way – starting from some layer its pressure becomes so intense that it

presses atoms of substance despite their mutual repulsion. This pressure is a million times higher than anything existing or created on Earth. The nuclear "pot" of a substance with incredible density is formed in the center and all electrons are taken away from their orbits encircling the nucleus, while the very nuclei lose their initial characteristics and mess with each other. The nuclear mix flies away and forms new atoms – many of them were absent in the initial target material. The main idea in this method is to create conditions for such an electronic blow that all surface layers of the target will start continuously shrinking inside. We dubbed it "The method of punching the compression of a substance."

For those who have a deeper knowledge of nuclear physics I will provide data and calculations. Scientists from all over the world are trying to conduct controlled thermonuclear reaction experiments with mixes of deuterium-tritium (D-T). In theory, in order to ignite it and to give way to energy which exceeds the energy spent for "ignition," one should create conditions described long ago known as the "Lawson criterion." According to this criterion, first the temperature ***T*** of plasma should not be less than 7-10 keV (70-100 million degrees Kelvin). And second, the density of plasma **n** (the number of ions in a cubic centimeter) and the time of its detention under specified temperature TAU should satisfy the following ratio:

$\mathbf{n}*TAU>10^{14}$.

The creation of such density of plasma at a specified temperature, and moreover its detention for a necessary period of time, is a task which has been an unreachable dream for physicists all around the world. According to the results published by Adamenko and his colleagues, they managed to reach the following conditions inside the core of the target:

$\boldsymbol{T} >>$ 40 keV (upper estimate of 100-200 keV);

$\mathbf{n} >> 10^{27}$ nucleus /cm^3 (upper estimate exceeds $10^{30} – 10^{33}$ nucleus/cm^3);

$\mathbf{TAU} > 10^{-8}$ sec.

So, in some of the 10,000 experiments the Lawson criterion is surely exceeded:

$\mathbf{n*TAU} >> 10^{19}$

Such density of a substance is characteristic of the nuclei of neutron stars and white dwarves. This extreme state was obtained under Earth conditions for the first time and is the result of the artificially invoked collapse of the target as described above.

It is not difficult to estimate the energy emitted during the collapse and the following explosion. We put 100-200 joules in a beam of electrons to create the initial ionization. For comparison – in order to heat half a glass of water 1 degree we need 400 joules. We take a quarter of this energy – this is an insignificant value. But the emission of the target lasts for 10^{-8} sec. So what do we obtain after the collapse? First we obtain isotopes of elements of the entire Mendeleev's periodic table from hydrogen to super heavy elements. It is possible to calculate for each nuclei how much energy such a transformation took. And it happens that to channel nuclear transformations only 10-30 megajoules are necessary! That means that the ratio of input and output energy differs by 5-6 times!

There are other ways of getting energy. If we take only the energy of a light flash, it will be almost equal to the energy necessary to launch the process. The ion component gives 1,000% and this is a low estimate.

First There Was Copper, Then Mandeleev's Entire Periodic Table

Aleksander Kochno, the director general of the Proton-21 laboratory, and another employee, Vladimir Vysotsky, a professor of physics and mathematics at the Kyiv Shevchenko University , join our conversation.

Vladimir Vysotsky: They demonstrated amazing photos of exploded targets, and it could be clearly seen that the explosions happened in their center, in the depth of the substance. I was particularly interested in a photo of a strange hemisphere imprinted on the screen with a ball clearly cut out from the center. The scientists explained that inside this ball the reaction of the nuclear collapse had happened and the explosion had taken this hemisphere out of the target and smashed it into the screen. Other tables show the results of spectrographic analyses – how many various elements were registered during explosions, for example from a copper target with 99.99% purity.

Alexander Kochno: At first we had tested methods of inertial thermonuclear synthesis which are close to classical methods. But we tested in our own way. When we received results which did not fit forecasts, we started analyzing them. And we realized that we faced entirely different physical mechanisms. We started creating theoretical models, optimizing installation and came to what we see now through iterations.

We opened door to that process and a stream of absolutely new information flew out. We simply failed to analyze everything and had to rethink these things.

-So are there any preliminary conclusions available?

V.V.: The first is that these experiments were followed by short-term, but unbelievably powerful emissions – X-rays, gamma rays, light rays, etc. The second is the formation of new elements – both the entire spectrum of Mendeleev's periodic table and such super heavy elements that nobody had ever suspected their existence. These are transuranium and rare isotopes. There is an example – the most distributed isotope of iron is Fe-56. Its proportion of the total weight of iron amounts to approximately 92%. And there is a rare isotope called Fe-57. There is a small amount of it – almost 2.2%. This is the so-called "Mossbauer isotope" which is used in nuclear physics and metrology, and its market value is $10,000 dollars per gram because it is very difficult to split it from ordinary iron. So, during our experiments we got Fe-57 in higher quantities than Fe-56. Even in this single case one could start commercially feasible production.

As for the super heavy nuclei, it is known that until now the only elements with nucleic charges up to 116 were synthesized in the best laboratories of the world. Moreover, there were reports at recent physics conferences – one famous laboratory registered the formation of 4 nuclei with a charge of 116 and rough atomic weight of almost 280. A second famous laboratory synthesized 5 nuclei during a half-year period. We can synthesize transuranium elements in trillions of nuclei and the maximum rough atomic weight we have registered is almost 4,000 units. We suspect that nuclei with a rough atomic weight up to 100,000 were formed during our experiments. It is curious, but the possibility of the existence of such nuclei was forecast 30 years ago by academician Migdal.

Adamenko: The most amazing thing is that these super heavy nuclei are either stable or very long-lasting ones. By the way, you can note that the greatest dream for all thermonuclear laboratories in the world is to provide synthesis on the basis of nuclei lightest in weight – tritium and deuterium. The system of synthesis we have developed goes on with almost similar efficiency with any nucleus (including those classified as the iron group, which is impossible in principle in classical thermonuclear synthesis).

V.V.: Third are the streams of charged particles. These are ions with great energy, electrons, protons, deuterons, fast-moving plasma. There is an abnormally high number of protons. During each explosion, so to say, a small galaxy, a new world, is born. We get interesting confirmation of this fact. The distribution of chemical elements in the universe is known – what percentage of its weight belongs to hydrogen, helium, lithium and so on. There is a curve which was built with corresponding data. And the distribution of elements which happens as a result of our experiments nicely correlates with it, no matter what we put into the "oven" – on average the "ash" has the same content. This has been checked by many experiments. As for the energy of the particles, they have from 100 keV to 5-10 MeV for protons and deuterons. Besides that, we register great numbers of particles with energy levels of up to 1 GeV.

The fourth moment is applied deactivation. As a result of mini explosions the substance under radiation gets rid of such phenomenon as radioactivity. After collapse has occurred due to the impact on the sample, the substance loses its initial content. At a certain moment a "nuclear mix" forms, where atoms (in their general meaning) are located in the center of the core, while there are no electrons around them. And the same thing refers not only to light but also to any heavy nucleus. An almost uniform electron-proton-neutron mix is formed. This concoction serves as material for the

immediate formation of a new nucleus in another "package" and this new rebirth must be maximally stable. Unstable nuclei are simply not viable and they split immediately. And in case the substance was radioactive initially, then after the explosion the newly formed substance has no radioactivity at all!

So, this method may be used to utilize various wastes, and not only radioactive ones – any poisons, even the worst ones, could be transformed into safe substances and energy may be obtained as well if scientists will succeed in making them targets for nuclear collapse.

The fifth thing is external manifestations and testing effects. In our experiments we register huge quantities of various abnormalities. For instance, the material of the reactor walls absolutely changes its structure and pores are formed. Experts in nuclear reactor testing say that when comparing our substance with material which had worked in a reactor for 20 years, our substance showed more changes in structure.

Here one can develop theories and technologies for each paragraph. The field for further research and applications is boundless.

Soccer Ball Sized Reactor

- Here is a pragmatic question – how could the effects you have discovered be used in practice?

V.V.: The easiest way is to use them to deactivate radioactive waste. Chernobyl has shown that this is a critical question. In November I was in Las Vegas attending a conference dedicated to the prospects for nuclear industrial waste. The conference was held by IAEA. Each country reported what it could do in the sphere of processed nuclear fuel elimination. Nothing new was offered – waste is still buried. Americans and Japanese are trying to develop huge proton accelerators for getting neutrons which they will use to emit at waste in hopes that as a result they will get a less radioactive substance. The first experimental installation will be built in the middle of the century. Now hundreds of million dollars are already being spent annually. Russia ,China and other countries chose the same way. Even if they are successful they will not have the ultimate utilization of fuel. Neutrons processing some types of radioactive waste will stimulate the formation of other types of waste. That means the market for our installation is wide open. However, we have many problems and we need corresponding solutions.

-Is it realistic to get electricity with the help of your installation? How can your explosion be harnessed to generate electricity?

V.V.: We may choose different paths. Each observed process – emissions, streams of particles and external effects may be potentially used for this. One should calculate which option will be the optimal one. The primitive way is to transform energy from the collapse into heat, then create vapor with heat and use it to rotate the turbines of a generator. Maybe it would be more useful to employ magnetic-hydrodynamic generators. They look like ordinary generators which produce current at the moment when conductors cross a magnetic field. Everything works in principle and can be calculated. But we have not yet come to a solution for providing our own laboratory with its own light. Now we are conducting experiments in order to understand the essence of the processes. And the main thing is to install different targets each time, to reach a vacuum, to measure all parameters, to shoot, measure everything again, to disassemble the installation, to replace the target and so on.

For efficient production of electricity the system must be automatic – it should make not one shot an hour, but, say, one or ten per second. The automatic supply of a target rod, the receipt of energy – all these are serious technological problems which should be solved first.

An approximate calculation shows that when using targets with different dimensions at 10 shots per second with a performance factor of 10%, one may hope to get 100 megajoules per second. The capacity of such an installation will practically replace a 100 MW power unit! But we have no limits, say, for increasing its capacity by 10 times and that would equal the standard capacity of a reactor at Chernobyl . Anything could be used as fuel – copper, silica and even water. And there will be no radioactivity either before, or after!

-And what do you think the approximate dimensions of such a reactor would be?

A.K.: The machine itself may be relatively small. The reactor will not be larger than table. So, in the future there will be no need to construct huge power stations. They would be compact and be easily delivered to any necessary place. However, I would like to say again – there are many technical problems and their solution will take several years.

-And what about the safety of your small bomb? Could it be used by terrorists, for instance?

V.V.: These processes are small and they could not be used to produce a great explosion. Moreover, the explosion is always directed inward.

-But as a result you obtain an outside explosion! And could this mini nuclear explosion trigger a reaction which may result in the collapse of the surrounding world?

V.V.: No! The explosion is always limited by the target which the beam is aimed at, and the entire process is going on only inside the target. Only nuclear fission products come outside and they could not trigger repetitive explosions.

A.K.: And in order to conduct a relatively serious explosion one should launch the process at a target which has dimensions of at least 1 centimeter. According to our estimates, that is not possible. One could not create a pocket-sized atomic bomb based on this effect.

The Black Hole as an Experimental Artifact

A.K.: Now we are talking about things which make ordinary physicists speechless and the existence of super heavy elements an axiom for us.

And we had a lot of such cases – after our experiments we gave our samples to other organizations and asked them to check them with their equipment. And we did not tell them what these samples were. They brought us reports and in a couple of months they admitted that they had no idea how such results might be registered, for instance during spectrographic examinations with lines which are impossible for these elements. "This is not according to textbooks!" they said. They overhauled equipment, tuned it, looked for malfunctions and obtained the same results. We told them how we got these samples and they replied: "So, why didn't you tell us!" We explained that we did it to keep the experiments pure.

But we have some things which could not be explained, for instance, "black holes."

-???

A.K.: They do not "light up" under any light.

Adamenko: There are various methods of to analyze substances. One is radiation by a beam of ions and registering reflected signals. We tore secondary ions away from bombarded substances and analyzed them. And after an explosion we saw how local sections (20-50 microns) appeared on a screen. They reflected nothing no matter how hard you bombarded them with ions. Not only did these smashed out particles of screen refuse to fly, but even primary ions did not return. These are regions which absorb absolutely everything, in any range of weight! Just imagine, you shoot a handgun and the bullets simply disappear. They are not reflected and they do not pass through. And moreover, the process of absorption has no visible manifestations. This could not happen according to the laws of classical physics. We gave this effect a temporary name – "black holes." We suppose these places contain the nucleus of some super heavy element which has "not grown yet" till its reaches its limits. A so -called "dump pit ."We spent more time bombarding these sections (up to half an hour), and only then did these "black holes" start to return some part of the ions. Probably, saturation had occurred. These are interesting subjects of study.

There is another assumption connected with these sections and it refers to cosmology. It is known that there is a great quantity of "dark" matter in the universe, and it cannot be detected by ordinary tools. It is possible that the "black

holes" we observed are part of this matter. It may be made of the nuclei of super heavy elements.

Ukraine Patents 'Eternal Engine'

-Is your method registered? Have you received any patents for it? What are the results of experiments published in scientific magazines?

A.K.: The patent is already given in Ukraine , and our claim is in different stages of registration in Europe, the U.S. and Asia . The international application PCT/UA2003/000015 for a patent was accepted on May 19, 2003 , with priority dated 14.08.02. We reported our findings at approximately 15 international conferences and published several articles in serious scientific magazines. Did they shout "hurray" or not? They did not shout, but they did not condemn our findings either. Now they are keeping silent, trying to overthrow our data. But the very fact that two theoretical articles totaling 60 pages were published in leading physics journals Foundation of Physics Letters and Foundation of Physics speaks for itself.

-Several years ago scientific circles widely discussed the theory of so-called "cold fusion." And coverage ranged well beyond scientific circles. However, supporters of the theory have failed to provide confirmation yet. I should note that your work has many similarities with this theory.

V.V.: What is general to both cases is that here one can observe processes which are going on under relatively low energy when in general they should not exist, as well as the formation of new elements. There is a difference in approaches and theories which describe these processes. We managed to create an installation which gives real results which can be reproduced with 100% certainty, while the theory of cold fusion cannot do that. The increase in energy emitted during the reaction is at the level of statistical mistakes in experiments. Yet we have increases of many thousands of percent.

-How well are your results documented?

A.K.: We have conducted almost 10,000 experiments and they were carefully documented. All targets were weighed and measured before and after bombardment. All conditions were registered – target content and screen material with an accuracy up to 4-5 digits after the decimal point. We conducted chemical characteristics of electron beams, and spectral and other analyses. We have made more than 20,000 analyses already. All targets are stored safely in their own numbered slots. They may be taken out, measured once more and we may find parameters which we had not thoroughly examined during the first analysis. Each day we receive up to 10 new samples and we simply have no time for proper analysis, leaving them for the time being. All data are entered into a database which is adjusted to conduct various correlations and calculations. We may take a necessary sample, cut a piece away from it and pass it to somebody for a control check. We have no situations in which we conducted an experiment, measured and obtained results, and the sample disappeared and we could not confirm what we had found. That could not happen. We welcome scientists from all over the world – let them come, analyze and discuss. We are ready.

Stanislav Adamenko, the science head of the Proton-21 laboratory

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218) My dilemma -- to do or not to do it?

Ludwik Kowalski (5/6/05)
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This morning I receive a message from a stranger, Walter. He wrote:

> "Oriani counted very dark tracks only" It is important to know
> whether "very dark" tracks were counted in a double-blind
> manner. Otherwise the experimenter can unconsciously --
> and innocently! -- bias the results towards what was expected.
> I.e., "very dark" might actually have to be darker for the control
> experiments. Best if the counting can be done automatically,
> e.g., by image-processing software.

I asked for the advice about the image-processing software but Walter said that it was only a general comment and that he never used such software. But his comment is highly appropriate for a situation in which the signal to noise ratio is as small as described in unit #210. The only way to decide if the signal is real (that is to decide if alpha-like particles are really emitted from the cathode) is to use photos, as described by Oriani at ICCF10. Walter's message reminded me that I wanted to use this method; here is my reply to him:

> After the first etching I will take numerous photos of the entire
> surface. This will identify "old" tracks not by their sizes or darkness
> but by their positions (with respect to recognizable scratched lines).
>The same will be done after the second etching. "New" tracks will
> thus be identified unambiguously by comparing the photos "before"
> with the photos "after."

Should I start this work right now or should I wait? I am hesitating because I know how much work is involved in photographing ten surfaces. At the minimum magnification of my microscope each photo covers the area of only 1.3 mm by 0.9 mm. Each of my CR-39 chips has the area of about 1 cm2. That implies about ~100 pictures per surface. With ten surfaces (5 for control chips and 5 for treatments) I must study about 1000 pictures. That is not trivial. Assuming I use the same exposure times (24 hours), and the same counting geometry (chips are ~5 mm away from the cathode) as Richard, I should expect, on the average, 33.6 alpha tracks per surface. This translates into about one track per 30 photos.

The purpose was to produce a simple method by which students and teachers can convince themselves about reality of nuclear particles due to a chemical process. To be useful the experiment should be feasible in three to four hours. Suppose that the rate of emission is ten times larger than what was reported by Oriani. In that case the expected number of tracks would 336 per surface. No photography, and no detailed statistical analysis, would be necessary to see that the effect is real. At the minimum magnification of my microscope the field of view has the diameter of about 5 mm. This would allow to count tracks at the rate of several minutes per surface. But now Oriani is in a situation in which photography is a must, as far as track detectors are concerned. This is not for students and teachers. In a week or two Oriani, and a nuclear physicist from the University of Minnesota, will start exploring the phenomenon with a

silicon detector. I should wait for their preliminary results before deciding what to do next. The silicon detector, if the background can be lowered, as in Jones' laboratory, seems to be (?) more appropriate than CR-39 for very low emission rates. But lowering the background might not be trivial; it calls for a set of good veto detectors.

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219) Post-workshop comments

Ludwik Kowalski (5/10/05)
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There was an interesting workshop at Stevens Institute of Technology last week. The title was “The Applied Science Problem;” I am grateful to a neighbor, Dr. Mary Ann Hellrigel, for inviting me. It was a gathering of about forty scientists, historians of science and students. Most contributions were very interesting and stimulating. Cold fusion was not mentioned but I think that most participants would be interested in it. The essay below was e-mailed to them after the workshop.

Protoscience, pseudoscience, etc.

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After hearing about complex relations (linear and nonlinear) between science, applied science and technology, I would like to comment on pseudoscience and protoscience. In the linear model protoscience precedes science; it may lead to science or to pseudoscience. Someone mentioned that at least 95% of scientific publications describe research based on previous investigations. But each identifiable thread has a beginning, a seminal publication or a patent. It describes a discovery that was made, in one way or another, without an obvious connection with other studies. In science it may be a discovery of a new phenomenon, such as the piezoelectric effect; in technology it may be a gadget, or a new material. Subsequent accumulation of knowledge follows in a brick-by-brick fashion.

How are seminal contributions made? What do they have in common with other kinds of intellectual creativity, such as music, visual arts or poetry? That is a big topic and I do not want to deal with it. I am mentioning “seminal contributions” to define protoscience. A seminal contribution that has not yet been validated belongs to the realm of protoscience. The ways of validating scientific claims are based on additional explorations. The process of validation may be fast or slow. A topic becomes scientific when claims are confirmed by several recognized scientists. The process is gradual and no sharp boundary exists between protoscience and science.

But a boundary between science and pseudoscience is usually well defined. Pseudoscience, such as astrology, differs from science, such as observational astronomy, by the way in which claims are validated. Pseudoscientists only pretend to be scientists, sometimes to benefit from it materially, sometimes to satisfy their psychological needs. A large number of examples can be found in a book entitled Voodoo Science (1). Scientific error, says Robert Park, "has a way of evolving ... from self-delusion to fraud. I use the term voodoo science to cover them all: pathological science, junk science, pseudoscience, and fraudulent science. In pathological science, scientists fool themselves. Junk science refers to scientists who use their expertise to befuddle and mislead others . . . Pseudoscience has the trappings of science without any evidence. Fraudulent science is, well, fraud--old-fashioned lying.”

The author, however, was not able to recognize “cold fusion” as protoscience. I would like to focus on this point

becaus the cold fusion episode, in my opinion, will be remembered as an interesting aberration in the sociology of science. This will happen regardless of how the scientific controversy, begun in 1989, will be resolved. The major proponents of cold fusion were recognized authorities in their disciplines. Some of them, now in their sixties and seventies, have been conducting research in the field for more than 15 years. As far as I know (from reading their papers and from attending conferences) their ways of validating claims are not different from those in various areas of recognized science. How was it possible, our descendants will ask, that such people were treated in the same way as pseudoscientists? How was it possible that their publications were often rejected by editors of mainstream journals and that financial support was not available to them as easily as in other areas of "pure science?"

I have been studying cold fusion for more than two years, not only as an observer but also as a researcher. This is reflected in over two hundred items posted on my website (2) dedicated to the subject. Experiments in which I participated did not provide a clear and undeniable confirmation of claims in which I was interested. The biggest problem was absence of reproducibility. But that does not mean that cold fusion research is pseudoscientific. Irreproducibility is an indicator that researchers are not aware of major influences. Electrostatic demonstrations were also highly unreliable untill the role of ambient humidity was recognized. Science is highly bureaucratic today and many think that nonscientific factors, such as competition for limited financial resources, have played an important role in the shaping of attitude toward cold fusion researchers (3). Historians of science have a unique opportunity to study cold fusion, as suggested, for example, in (4). Do not miss an opportunity to interview major cold fusion players; it may no longer be available in several years.

Fraudulent con artists (pseudoscientists) described by Park are usually people without recognized credentials. Cold fusion protoscientists, on the other hand, at least those I met at two conferences, have excellent academic credentials. I suspect that the controversy surrounding cold fusion attracts con artists and charlatans. But such an assertion is difficult to prove. More obvious is the fact that people working in different areas of protoscience tend to attract each other. I do not know, for example, what the so-called "zero point energy" field (5) has in common with cold fusion. But research reports in that field can often be found in journals and magazines devoted to cold fusion (6,7,8). Likewise, research on hydrinos -- atoms of hydrogen "excited" to presumably-existing states below the ground states -- and on perpetual motion devices, can be seen intermixed with cold fusion papers. My definition of cold fusion, described in (9), focuses on a correlation between a chemical process, such as electrolysis, and a nuclear process, such as emission of alpha particles.

Sometimes people say that experimental validation of cold fusion would inevitably result in "paradigm shifting." I do not take this for granted. Many investigators try to understand cold fusion in terms of new theoretical models. But, as far as I know, their models are not able to identify conditions under which cold fusion anomalies (chemically induced nuclear reactions) become reproducible. Who said that the existing paradigm (the arsenal of existing models) will not be able to make sense out of reported experimental observations? On the other hand, how can a theory be validated when experimental data are not reproducible? The phrase "theories guide but experiments decide" describes the essence of scientific methodology. It implies that making cold fusion reproducible is a precondition of possible evolution from cold fusion protoscience to cold fusion science.

And here is my last question. How long can an area of research remain protoscientific without becoming pseudoscientific, by default? Yes, I know that asking questions is much easier than answering them. The main point is that the questions I am asking here belong to the sociology of science; they do not belong to science per se. I will end with a quote from wikipedia, an editable encyclopedia of science on the Internet (10). The description of cold fusion one finds there is worth reading. But keep in mind that anybody can change anything in wikipedia at any time. I can not be sure that what you will read there will be the same as I read several weeks ago.

" 1) A pseudoscience is any body of knowledge purporting to be either both factual and scientific, or of an even higher standard of knowledge, but which fails to comply with scientific method. Motivations for the advocacy or promotion of pseudoscience range from simple naivety about the nature of science or of the scientific method, to deliberate deception for financial or other benefit. Some people consider some or all forms of pseudoscience to be harmless entertainment. Others, . . .consider all forms of pseudoscience to be harmful, whether or not they result in immediate harm to their followers.

2) Pseudoscience is distinguishable from revelation, theology or spirituality in that it claims to offer insight into the physical world by "scientific" means (i.e., most usually in accordance with the scientific method). Systems of thought that rely upon "divine" or "inspired" knowledge are not considered pseudoscience if they do not claim to be scientific or to overturn well established science.

3) Pseudoscience also differs from protoscience . The latter may be defined as speculation or hypothesis which has not yet been tested adequately by the scientific method, but which is otherwise consisent with existing science or which, where inconsistent, offers reasonable account of the inconsistency. Pseudoscience, in contrast, is characteristically wanting adequate tests or the possibility of them, occasionally untestable in principle, and its supporters are frequently strident in insisting that existing scientific results are wrong.

4) The boundaries between pseudoscience, protoscience, and "real" science are often unclear to non-specialist observers. They can even be obscure to experts. Many people have tried to offer objective criteria for the term, with mixed success. Often the term is used simply as a pejorative to express the speaker's low opinion of a given field, regardless of any objective measures.

5) After more than a century of active dialogue, the question of what marks the boundary of science remains fundamentally unsettled. As a consequence the issue of what constitutes pseudoscience continues to be controversial. . . . Examples of fields of endeavor that many consider – to varying extents – pseudoscientific include Cold fusion, Pseudoarchaeology, Gene Ray 's Time Cube, astrology and homeopathy.

6) There are also young fields of science that are sometimes frowned upon by scientists from established fields, primarily because they are speculative in nature. [For example]: (a) exobiology /astrobiology, (b) Search for Extraterrestrial Intelligence (SETI) and (c) Communication with Extraterrestrial Intelligence (CETI). These fields are not considered pseudoscientific or protoscientific by most scientists, though, and they are studied at many universities and specialized institutes."

Please share your comments about cold fusion and other social topics associated with it. My e-mail address appears above. Information about ongoing cold fusion events can be found at (11).

References:

1) R. Park, "Voodoo Science: the Road from Foolishness to Fraud," Oxford University Press; 2000.
2) <http://blake.montclair.edu/~kowalskil/cf/index.html>
3) E.F. Mallove, "Fire from Ice: Searching for Truth Behind the Cold Fusion Furror," John Wiley & Sons, Inc., New York, 1991.
4) <http://blake.montclair.edu/~kowalskil/cf/180library.html>
5) <http://www.spiritofmaat.com/archive/feb2/bearden.htm>
6) Probably still available from Hal Fox in Salt Lake City, Utah.
7) <http://www.infinite-energy.com/>
8) <http://www.lenr-canr.org>
9) <http://blake.montclair.edu/~kowalskil/cf/212gluck.html>
10) <http://en.wikipedia.org/wiki/Pseudoscience>
11) <http://newenergytimes.com/news/news.htm>

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220) Two cold fusion workshops and conferences

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It is remarkable that two cold fusion workshops are going to take place nearly simultaneously this month. One, organized by ISCMNS (International Society for Condense Matter Nuclear Science), is starting today in Siena, Italy, and another will start at MIT, Boston, next week. The Boston workshop is dedicated to Eugene Malove, "Cold Fusioneer, Investigator and MIT Graduate '69." Two cold fusion conferences will also take place this year:

1) The 12th International Conference on Condensed Matter Nuclear Science (ICCF12) in Japan
2) The 13th Russian Conference on Cold Nuclear Transmutation of Chemical Elements and Ball-Lightning (RCCNT&BL-13)

Here is the agenda of the MIT workshop (May 21, 2005):

Experimental Reports of Cold Fusion Systems (A.M.):
1) Mitchell Swartz - Introduction
2) David Nagel - Evidence That Cold Fusion Involves Nuclear Reactions
3) John Dash - Characterization of Titanium Cathodes after Electrolysis in Heavy Water
4) Peter Hagelstein - Cold Fusion
5) Mitchell Swartz - Parameter to Describe Optimal Operating Point Width

Theoretical Strides in Understanding of Cold Fusion Systems (P.M.):
6) Yeong Kim - Mico/Nano High-Desity Plasmas and Cold Fusion/Acoustic-Induced Cold Fusion
7) Talbot Chubb - Bloch Nuclei and Phonon De-excitation
8) Scott Chubb - Understanding Cold Fusion using Conventional Condensed matter Physics
9) Robert Bass - Do Current Concepts Resolve the Chief Challenge to Cold Fusion Theory?
10) Keith Johnson - Anomalous Superconducting Properties of the PdHx/PdDx System and their Possible Relationship to Cold Fusion
11) Mitchell Swartz - Absence of Bremsstrahlung is Consistent with Conventional Physics
12) Group Discussion
13) Robert Rines - Patents and Science from Cold Fusion and Other Fields
14) Peter Graneau - Alternative Energy using Latent Energy of Water
15) Ken Shoulders - Charge Clusters and the Hutchinson Effect

Poster presentations, not yet listed, will also be displayed. Responding to the suggestion made by Mitchell Swartz I prepared a poster presentation shown below. I will refer to it in the group discussion; perhaps my answers will generate some interesting comments.

Short questions and answers

1) What is Cold Fusion (CF)?
It is not pseudoscience, as many think. It is protoscience claiming that nuclear activities result from some chemical (atomic) processes (for example, in electrolysis, or when hydrogen ions diffuse through some solids).

2) Why was “excess heat” not mentioned in the above answer?
Convincing others that excess heat (generated at the rate of one watt or less) has a nuclear origin is much more difficult than validating a nuclear signature, such as neutrons, protons or alpha particles. The essence of the controversy is not the excess heat; it is the nuclear origin of that heat.

3) Why hasn’t a single truly-reproducible setup been offered (in 16 years) by the CF community to demonstrate a nuclear effect caused by a chemical process?
Because CF research (at least 100 people in several countries) is not coordinated.

4) Why don’t CF researchers criticize each other?
They probably feel "we are surrounded by enemies; mutual criticism would only weaken our fortress."

5) What is experimental protoscience?
Any new claim made by a qualified experimental scientist belongs to protoscience unless it is independently validated in at least three different laboratories.

6) How long can protoscience exist before turning into pseudoscience?
In the case of CF the controversy has not been resolved in 16 years. Unless recognized by the scientific establishment, CF might be universally perceived as pseudoscince in about ten or twenty years.

7) Why twenty years?
Because nearly all CF researchers are old.

See my CF website at <http://csam.montclair.edu/~kowalski/cf/>
Comments will be appreciated.

This website contains other cold fusion items.
Click to see the list of links

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222) The majority of nature's treasures are still hidden

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Three days ago I received a thank-you note from a person who read some of my units. Replying to this note I made a specific suggestion and I asked a general question about cold fusion. The answer was interesting and worth sharing. It is an example of healthy attitude toward science in general and toward cold fusion in particular. **Tom Benson wrote:**

"Thanks for the reply. Let me answer your questions in reverse order. No, cold fusion isn't my full-time occupation. I have a job as a marketing director for a large software firm. I do cold fusion work just for the scientific pleasure of it, I suppose, the joy of hunting for a hidden, fundamental physical process, which is very controversial and therefore even more of an adventure.

I have very much enjoyed reading your website and some of the 'motivation' type posts, so if you don't mind, let me relate to you my own motivation. I loved science as a kid. I grew up in one of those post-WWII science towns, where everyone on the block was a nuclear engineer or scientist. Seaborg and Fermi and Rutherford were heroes, and the idea of being some part of a similar discovery, however small a part, was something to dream about. But instead of science I ended up starting a software firm, going the silicon valley route -- very satisfying in many ways but you know how it is, there were always some regrets not doing science.

Anyway, a couple years after cold fusion was announced, I decided to work on it. It seemed an opportunity to do real, old-fashioned science, the opportunity to do something truly novel. The longest of long shots of course, but I had no reputation at risk, and since other more established scientists couldn't risk their reputations (and who could blame them?) then it was left as an opportunity for an amateur like me.

I felt cold fusion was possible (even if a long shot) because I had read a quote by one of the disbelievers. He said 'If cold fusion existed, we would have seen it decades ago.' That seemed completely backwards. If you know the history of the nuclear science, and if you know about the experiments done to chart the cross-sections and energies all of the hundreds of various nuclear fusion and fission reactions -- it was the fundamental nature of those experiments that, if they were triggering a cold fusion type reaction, or even if cold fusion existed in nature, it would NOT have been seen. By definition, cold fusion reactions, if they indeed exist, are reactions that only produce alpha particles and heat, and they only happen when deeply buried in a condensed matrix. So, by definition, they never produce ionizing radiation that would escape to be detected, and therefore they never produce a signature that would be detected by the equipment that nuclear scientists had been using for the past 100 years.

That was intriguing -- the realization that there WAS a blind spot in the nuclear world, and cold fusion just coincidentally fell right in that blind spot. It was a tenuous link but compelling. It made me think; well, there could be hundreds, or thousands of other types of nuclear reactions, a whole universe of reactions, happening all the time, right beneath our feet. But if they don't produce ionizing radiation, or enough energy to see easily, then how would we know? We can't see them. This is one of the most interesting things about science, how much you can miss even in years of hard study.

I also had seen a real-life example: Fullerenes [carbon molecules, such as C60]. For a hundred years, in universities all over the world, hundreds of thousands of skilled organic chemists had been studying carbon. It was one of the most

carefully studied, profitable branches of science ever; and in that time spent, they simply missed an entire class of carbon molecules, which existed in nature all the time, in every bit of smoke from a match. And now these new structures are proving to be one of the most valuable compounds in the history of science. This reminds us that the majority of nature's treasures are still hidden, perhaps right in front of our eyes. If it can happen for organic chemistry, why not nuclear physics?

The second big 'aha' was hearing Fleischmann speak, at an early conference, about the fact that the original experiment he and Pons conducted was actually designed to test a decade or more of theoretical work they had done. Their interpretation of QED let them to question whether deuterium ions, compressed tremendously inside a matrix, might spontaneously fuse in a very unusual fashion. So they did an experiment to test this hypothesis -- the original Pons-Fleischmann experiments. The result they got confirmed their hypothesis. This is classic, pure science, the kind we read about in textbooks. Very nice, aggressive, science. So when I read the opposition calling their experiments 'junk science' and 'crap' and other smear campaign-type words, I was irritated. It was highly inappropriate.

On the other hand, I can't presume to say the effect is proven. It requires much, much more evidence. It is also true that many of the experiments are very poorly done and not double-checked. But that says absolutely nothing about the existence or nonexistence of the underlying physics - it's simply the result of the fact that cold fusion experimenters are all doing work on pennies, using old scrounged equipment . . . Thanks again for your response, and sorry about the long-winded reply."

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223) Karabut's papers

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Alexander Karabut, whose earlier publication (see item #13) triggered my interest in cold fusion, sent me two posters for the incoming MIT colloquium. He also sent me two corresponding papers in Russian.

Click to see his Paper 1 in English (awkward in some places)
or in Russian.
Click to see his Paper 2 in English (awkward in some places)
or in Russian.

A quick comparison shows that some sentences found in the English paper, do not appear in the corresponding Russian paper, and vice versa. The impression is that English papers are more recent.

In the first paper I read that the excess heats was generated at the rates of up to 10-15 W, and that the efficiency was 150%. That is much more that 10% efficiency reported by Kar abut in unit #13. My understanding of 150% is that for every 100 J of electric energy supplied to the glow discharge tube the amount of heat generated is 250 J. (150 J of ecxcess heat). But I am not sure of that this is a correct interpretation; perhaps the amount of heat generated is 150J rather than 250J. According to my definition, this would be 50% efficiency, not 150%. But even 50% is impressive.

The highest excess power on Figure 3 is about 8 W; most data points indicate excess powers from 1 to 4 watts. Presumabaly efficiencies in the range from 10 to15 W were recorded at input powers higher than 20 W. Reading the paper carefully I was not able to find a protocol for the high efficiency. The only indication was high voltages (1000-1400 V) applied in the form of short pulses (much shorter than intervals between pulses). Can one say that this is a supervave of some unspecified fundamental frequency?

Another interesting result, also reported in init #13, is nuclear alchemy; many new chemical elements were accumulated in the cathode when excess heat was generated. The estimated rate of transmutation is reported as 10^{13} atoms per seconds. Is this enough to explain the excess heat? Suppose that, on the average, production of one atom is associated with the release of 5 MeV of heat. This would generate heat at the rate of $5*10^{13}$ MeV/s, or 8 W. The order of magnitude seems to be consistent with the actually measured excess heat power.

The second paper focuses on generation of X rays; the subject that is also mentioned in the first paper. The rays were studied, using many different cathodes, when the current was 500 mA and the voltages were in the range of 500 to 2500 V. That translates into a lot of input power; more that 1000 W, at the extreme. At the efficiency of the excess heat generation of 150% that would generate 1500 Joules of abnormal heat each second -- enough to bring one liter of water to the boiling point in about three minutes (starting at room temperature). But such exp ¡ectation is incorrect because the applied voltage was not constant. The 1000 W is the momentary power during each pulse; it is not a constant power. Durations of current pulses were between 0.27 and 10 microseconds. and periods of repetition were between 1 and 100 microseconds. Two kinds of X rays were identified:

a) multidirectional (diffused, up to $5*10^5$ bursts per second with up to 10^6 photons per burst.)

b) laser-like beams of short duration (up to 10 beams per second and up to 10 photons per beam.) Emission of laser-like beams takes place not only when the current is flowing but also up to 100 microseconds after the current is turned off. Lasing implies presence of reversed populations of metastable energy levels presumably created by deuterium ions bombarding the cathode.

Soft X-rays (photons whose energies are in the range of 1.2 to 3 keV) are referred as primary penetrating radition. Secondary radiation, presumably resulting from interactions of primary photons with cathodes, were also identified. One of these components is said to be able to penetrate 5 mm of steel. In the second paper the new technology of creating laser beams is characterized as 100% reproducible. Nothing, however, was said (in the first paper) about the reproducibility of generation of excess heat and transmutation products. I suppose that Karabut believes that the 150% efficiency is 100% reproducible. Otherwise his conclusion, at the end of the first paper, would be questionable. The conclusion is that building a new nuclear device is now possible. That device, a cube of 20 cm, powered be electric energy at the rate of 10kW, will generate excess heat at the rate of 15 KW. The main conclusion of the second paper is a possibility of building a solid-state X-ray laser. It will produce powerful pulses of radiation (10 MW) lasting 10^{-11} to 10^{-13} seconds.

Appended 5/19/05:
I am now aware of three independent cold fusion groups working with glow discharge chambers: that of Karabut (also see unit #13), that of Arik (see unit #213), and that of Tom Passell (the paper can be downloaded from the library at www.lenr-canr.org) All three are interested in excess heat. A device generating 10+15=25 kW of heat (10 kW from the electric energy supplied and 15 kW from cold fusion) is not yet a large power plant. But its 100% reliable operation would promote research on a much larger scale.

What would I do if I were a researcher able to build a device generating nuclear energy via excess heat at the efficiency of 150%? I would also try to publicize my work, as Karabut does. But instead of writing about many things (different cathodes, different gasses, different waveforms, different instruments, etc.) I would focus on what has been the most reliable and the most promising. I would describe my protocol as clearly as possible; I would provide other researchers with details needed to replicate my results. Yes, I know that this would be in conflict with desires to benefit financially from being the first in new technology. At my age I would not worry about this but I can understand those for whom commercial success is important. I do not know how to resolve conflicts between personal and general benefits. But the issue is certainly not new; Karabut is not the first scientist claiming to invent something useful. The e-mail message he sent me ends with this sentence. “I take an interest about an grant of a research work.” Will he find a grant in Russia? Will he find it in another country? It depends not only on him; it depends on success of others. The progress he made at the research laboratory Lutch, during the last three years, -- a jump from the 10% efficiency to the 150% efficiency -- is spectacular.

P.S. (5/20/05): 1) The term efficiency, (k.p.d. in Russian), in the context of an excess heat paper, can mean two different things:

a) (Thermal energy generated) / (Electric energy supplied)
b) (Excess heat generated) / (Electric energy supplied)

In my mind efficiency is (desirable output)/(input). That is why I assumed that the 150% refers to (b). But that assumption might be wrong. The excess heat would be much smaller if 150% referred to (a) rather than to (b).

2) If efficiency is defined as (a) then 100% would mean no excess heat at all. And 10% efficiency would mean that 90% of electric energy is converted into a form of energy other than heat, for example light or mechanical energy or chemical energy.

If efficiency is defined as (b) then 100% would mean that the amount of excess heat is equal to the amount of electric energy supplied. In my opinion (b) is more appropriate than (a). The k.p.d., by the way, translates as "coefficient of useful action." The useful thing, in cold fusion devices, is excess heat.

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224 The MIT colloquium

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The moderator of this gathering was Dr. Mitchell Swartz, a veteran cold fusion researcher. His doctoral dissertation at MIT, I was told, was in electrochemistry. He is also a medical doctor specializing in oncology. Here is how this one-day colloquium (5/21/05) was described over the Internet: "Cold Fusion - Science and Technology - plus other Clean Energy Investigations, with Special Tribute to Dr. Eugene Mallove, '69, Cold Fusion Investigator and former Chief Science Writer at MIT. Topics include: Science and Engineering; Discussions of Cold Fusion Materials Science; Review of Current Literature; Experimental Results; Understandings of Theories; Device Engineering; Discussion of Future Developments and Commercialization Potentials. Remembrances of Gene Mallove by family, friends and colleagues. Lunch included in conference fees. Free for MIT Students."

The number of participants was about 60 (my own counting); 15% of them, I was told, were students. The meeting was organized by E-club -- the MIT Entrepreneurs Club. It is an organization sponsoring workshops devoted to all aspects of science and technology. They meet weekly. The event organizer, Dr. Richard Shyndurоff, told me that the colloquium had two purposes; to commemorate Eugene, killed one year ago, and to expose interested students to the controversial field of cold fission.

The first speaker was David Nagel - the topic of his presentation was "Evidence that cold fusion involves nuclear reactions." It was a general review of results on production of helium, tritium, neutrons, new elements, and x-rays. He emphasized that formation of craters and hot spo ⁄ts on cathode surfaces (using scanning electron microscopes) should also be viewed as nuclear signatures. The second presenter was Ross George; his topic was "Acoustic-induced Cold Fusion Experiments." Ross has a company <html:/www.d2fusion.com> conducting practically-oriented research in cold fusion. He described experiments in which generation of excess heat was shown to be accompanied by the accumulation of helium (up to levels exceeding natural concentration in air). Some of his recent sonofusion experiments, generated excess heat at the level of hundreds of watts.

The third and fourth speakers were John Dash (from the University of Portland) and Peter Hagelstein (from MIT). Peter is a theoretical physicist; he said that about 150 different reaction mechanisms have been proposed, in fifteen years, to model cold fusion. In his opinion not a single theory emerged as "much better than others." John, who is a metallurgist, was describing results of his experimental investigations in the area of nuclear alchemy. Using the secondary ions mass spectroscopy method (SIMS) he was able to identify several transmutation products. He also reported on changes in isotopic ratios but these were results from literature, not from his own investigations. Kim Yeoung, Tabot Chub, Scott Chub, Robert Bass and Keith Johnson also talked about theoretical aspects of cold fusion.

Mitch Swartz talked about the electrochemical cell called Phusor. Mitch Swartz talked about the electrochemical cell called Phusor. The electrolyte used in this cell has a very low concentration. For that reason the input power is only several watts then the applied voltage is thousand volts. The highest rate of excess heat generation, according to rapidly displayed transparencies, was 3.5 watts. During the break I heard people saying that the device is now commercially available; potential users are schools willing to teach cold fusion. The most interesting part of Swartz's presentation was his discovery (already described at ICCF10) of the optimal input power. Mitch is convinced that high current (and thus intensive bubbling) is harmful. In his cells the current is very small and single bubbles on the surface of the cathode grow very slowly. But practically useful excess heat implies large output power; how can high output power be produced with a device of low input power?

The last presentation that I attended was that of Ken Shoulders. That item, entitled “EOVs and Hutchinson Effect,” can be downloaded, as a pdf file, from Ken’s website at <http://www.svn.net/krscfs/>. The presentation had three parts. The first was about Hutchinson Effect (shredding metallic structures without heat and with practically no mechanical force), the second was about the Ukrainian project of S.V. Adamenko (see unit # 217), and the third was about his own device -- the EV reactor and cylindrical mass spectrometer. Unfortunately, the amount of time devoted to the third part was very limited. The speaker before Ken was Robert Rines, the MIT patent counselor. He reminded the audience that “in old days” a patent would not be given to an inventor unless a working model were presented. That is no longer required. Then he elaborated on difficulties that patent investigators have in dealing with cold fusion claims.

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226) Reading a strange patent description

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This unit is the continuation of what was in unit #216. It describes a strange initiative. I plan to follow that initiative because I believe that long-lasting commercial success is as important as reproducible experiments in science. How to separate hydrogen from water? By relying on electrolysis, or on thermal decomposition. But these methods are not economically effective ways to produce a fuel. A presumably effective method was patented by Hyunik Yang and Alexander Koldamasov. A description of their Korean patent can be downloaded from www.delphion.com The text is in English but the translation is far from being good. Information appearing on the first page is shown below.

* *
The International PCT Publication Number is: WO2004/041715 A1
The title is: "APPARATUS FOR GENERATING HYDROGEN GAS"
The first inventor is: Yang, Hyun-Ik
The second inventor is: Koldamasov, Alexander Ivanovich
The initial publication date is: 21 May 2004 (21.05.2004)
The priority data is: 10-2002-0069231
* *

1) In the abstract I read that the apparatus uses highly purified water under “predetermined pressure.” The water flows through an narrow opening in the implant made from a dielectric material. I do not understand how high voltage impulses are generated in the implant by “cavitation emission” and how hydrogen atoms are separated from oxygen atoms. But I do know that separation of atoms is an endothermic process. What kind of energy is used to separate atoms? The authors refer to ions (which are magnetically separated) but the nature of ions is not clear to me. Do they refer to ionized molecules of water or do they refer to ions of hydrogen and oxygen?

2) Later I see that the dielectric material must have “tolerance to a cavitation emission phenomenon generated inside of the body.” I know that cavitation consists of formation of bubbles in a liquid. But what is “cavitation emission?” What is being emitted by what?

3) On page 4 I see a reference to “magnetic bodies.” Are these bodies magnetic monopoles? How else can I understand the following sentence? “Herein, magnetic bodies are formed at lateral sides of the channel such as a North pole of a group of the manetic bodies and a South pole of another group of the magnetic bodies face with each other.” How are H and OH ions, mentioned on that page, separated? What material is the “catalytic plate” made of? Would the answers be obvious to me if I were a chemist?

4) On page 6 I see a reference to a “high pressure wave.” But I do not understand the nature of that wave. The author writes: “Because of the high pressure wave, there are created fine cracks on the inner walls of the passage slot of the dielectric implant. Electrons are emitted from the fine cracks due to a property of the material used in the inner walls, i.e., the property that easily resulting in the cavitation emission phenomenon. The emitted electrons are dispersed within the operation fluid, thereby resulting in the Vavilov-Cheronkov effect.” I know that Cherenkov radiation consists of light emitted when the speed of electrons is higher than the speed of light in that medium. How are electrons accelerated to emit light? One thing is clear; a patent description is not a pedagogically written textbook.

5) On the next page I see a reference to “Lorentz force in a perpendicular direction to the magnetic field.” That force separates H and OH ions according to “their electric polarities, i.e. positive charged ions and negatively charged ions.” How fast must the fluid (containing ions) flow and how large should the magnetic field be to make such separation possible?

6) Please be aware that I am writing this as I read the text. The illustrations, whose descriptions begin on page 7, might provide answers to some of my questions.

7) Unfortunately, the illustrations did not help me understand the device. Figure 5, however, was useful. It made clear that “magnetic bodies” are not monopoles; they are N and S poles of two bar magnets (polarized sidewise) used to crate the magnetic field along the path of flowing water. The Lorentz force is due to that field, and to the velocity of ions formed in water. Hydrogen ions H^+ are said to be separated from the OH^- ions by Lorentz force. But how are these ions formed? How much energy is needed to separate H^+ from OH^-?

8) On page 14 I see this sentence: “ The apparatus as recited in claim 1, wherein the dielectric implant is made of one of ruby and sapphire and inner walls of the passage slot contacting the operation fluid include a dielectric layer sensitive to the cavitation emission.” What properties of ruby or sapphire are essential in this application? Why are these substances better than other dielectric materials?

9) My main question has to do with energy. The device is supposed to produce fuel. It is well known that burning one mole of that fuel (2 grams of hydrogen) generates 495 kJ of thermal energy and produces one mole (18 grams) of water. If the device can be 100% efficient, which is an idealization, then these 495 kJ of energy can be used to produce 2 grams of hydrogen. What benefits can one derive from a device using more energy to produce fuel than the energy one can obtain from that fuel?

The only logical answer, without violating the first law of thermodynamics, is to postulate that an additional source of energy is being used. What is the nature of that source? I know how some people would answer this question. They would say that the so-called “zero-point energy” allows us to get more than two grams of hydrogen from 495 kJ of thermal energy. The theoretical idea of a hidden sour úce of energy would be acceptable to me, but not before an experimental confirmation. According to what was quoted in unit #216, commercialization of the device is going to start in several months. Will it use much less than 495 kJ of thermal energy to produce 2 grams of hydrogen? If so then the iESiUSA stocks sold at $4 a share will be worth much more that $4000 per share. But I am not ready to invest in their hydrogen-making machine.

Addendum (6/4/2005):
I WAS ASKED TO REMOVE THIS ADDENDUM. THE REQUSTER REFERRED TO MY PERSONAL SAFETY. YES, I AM CONCERNED !

Addendum (6/4/05):
Browsing the Internet I found this webpage http://100777.com/node/1146. Posted on the “site for Truthseekers,” the document contains a description of something called gCell. I do not know if this cell has anything to do with the iESiUSA invention. They say that catalytic conversion of water into hydrogen and oxygen takes place in a gCell. Here is a quote from the first paragraph:

“ . . . A second process involves a thermo, electro-catalytic reaction that results in the complete separation [of hydrogen from oxygen]. In the third process, small amounts of the hydrogen and oxygen gas molecules created in the second process recombine, providing additional electrical current to subsidize the overall gas generation process. A single Genesis gCell stack (about the size of a small car battery), consisting of several individual gCells, is capable of producing hundreds of cubic feet of gas per day. In comparison, a typical American home located in cold climates consumes approximately five metered cubic feet of natural gas a day. When converted to electricity this represents a 30kW output. The eCell does the reverse reaction to create a cheaper, more powerful, more reliable, longer life, and compacter fuel cell..... ”

Unfortunately, the origin of "free energy," needed to operate the gCells and eCells is not mentioned. This can be contrasted with what is explained at another website http://www.physorg.com/news3083.html . This description makes sense to me; it offers a way of using heat from solar energy concentrators (or high temperature nuclear reactors) to produce fuel for our automobiles. Here is the description:

"The U.S. Department of Energy has awarded Clemson University a three-year, $856,000 grant to develop more efficient methods for producing hydrogen. 'The irony is that today, most hydrogen is produced by consuming the very fossil fuels we're trying to replace,' said principal investigator Mark Thies, professor of chemical engineering at Clemson. 'But, we can also produce hydrogen by splitting water into its two elements, hydrogen and oxygen. The trick is to find the most energy-efficient manner for carrying out that splitting process.'

Thies explains that proposed 'thermochemical processes' are much more efficient than the classic electrolysis method, which uses an electric current, for splitting water. The thermochemical processes require heat -- both solar and nuclear power have been proposed -- to operate and use complex chemical reactions to reduce the energy required to split the water.

These processes are still on the drawing board. Both high-powered experiments and high-powered computer calculations will be needed before hydrogen production can become a reality. Thies has assembled a diverse team of experimentalists, theoreticians and computer-aided design specialists to work on this challenging problem. The team includes Clemson professor David Bruce, John O'Connell of the University of Virginia, industrial consultant Paul Mathias of Cambridge, Mass., and Maximilian Gorensek of the Savannah River National Laboratory."

Addendum (6/5/2005):
Here is a piece from the iESiUSA website: http://www.iesiusa.com/hydrogen.html
We believe that the low-cost hydrogen-generation technology is the "crown jewel" of the company because of its potential to revolutionize the energy supply through the world. iESi's technology allows for the delivery of pure hydrogen directly to the compressor, without the usual requirements for . . . We are in the final stages of completing the manufacture of a hydrogen-generating unit. It will be available for private demonstration to licensees and investors in late 2004.

It is now June 2005; what did they demonstrate? In my opinion, selling reliable generators, under a very strong warranty, would be the best possible publicity. Many investors would buy a device if the "money-back guarantee" was formally offered for at least one year. A reliable device described on the iESiUSA website (already protected by the Korean patent) would pay for itself in much shorter time than one year. I am afraid that a demo might backfire, like the 1989 public announcement in Salt Lake City.

P.S. Units #229 and #230 (and some to be added later) are also connected with this topic.

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227) Fisher's polyneutrons again

Ludwik Kowalski (5/30/05)
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Introduction:
Fisher's theory of cold fusion has been described in unit #191. The theory is based on the assumption that polyneutrons exist and that their exact masses can be calculated. It is important to emphasize that existence of polyneutrons is highly speculative. The same is true for the formula used to calculate their exact masses. John is aware of this. On the last slide of his presentation at Siena (May 2005):

http://www.markfisher.net/johnfisher/

John asks: "Do I believe polyneutron theory? Yes in concept. Polyneutrons are key agents for CF phenomena. They avoid the coulomb barrier, gamma radiation and neutrons. They offer explanations for transmutation and excess energy. Not in detail. Other choices for key parameters may be required. Should experimenters believe the theory? Not yet. But it has suggested novel experiments in the past (Particle showers in the vapor, energetic particles behind the cathode). . . . And it provides a mental framework for interpretation of results. Should theoreticians believe the theory? No. But they should have open minds. There are plenty of new theoretical questions that need attention." Fisher's model seems to be consistent with traditional nuclear physics; it respects the coulomb barrier and the law of conservation of energy. He invented polyneutrons and uses them to explain various cold fusion claims. His theory would be much more believable if it were based on universally recognized facts.

John's Siena presentation was a modified version of his theory. After posting slides of that presentation (available from the above Internet address) he suggested, in a private message, that I replace the earlier version of his theory (described in unit # 191), by his slide show. I am not going to follow that suggestion because I want to be able to compare two versions of John's theory. My task is not to justify his theory; it is to describe it pedagogically, and to share some comments. Here is how John introduced the new version in Siena: "Nuclear physics is incomplete. It can be extended to include polyneutrons. Polyneutrons explain cold fusion phenomena." In the recent private e-mail message John wrote that "for years they [theoretical physicists] had been stubbornly sticking with the idea that DD fusion was responsible for the cold fusion effects, and they were putting all their efforts into trying to find tricky ways of getting around the absence of the trademarks of DD fusion, rather that just accepting the fact that DD fusion did not occur ....."

Slides 5 and 6:
Referring to the "BCS model for polyneutron fluid" John states that "pairs of neutrons of opposite spin and momentum attract each other. Neutrons attract each other forcefully and directly. Not weakly and indirectly as for electrons via phonons. With strong interaction and full access to momentum space polyneutrons will be strongly bound. Collective binding of neutron pairs is expected to be much stronger than binding of electron pairs." I am not familiar with the BCS theory but I do know that traditional nuclear physics postulates existence of attractive nuclear forces between all nucleons. In the new version of the theory John postulates that the smallest possible polyneutron consists of 12 neutrons. I do not know why four or six neutrons, for example, can not form a polyneutron, as postulated in previously version.

The mass difference formula now has two terms, not one, as in the previous version. These are the volume and the surface terms, as used in the well known semi-empirical mass formula. John writes: " I assume a liquid drop model for the polyneutron mass excess. $\Delta(^{A}Nt) = a_v A + a_s A^{2/3}$ (A>12). The mass excess is a minimum at A =12, and for A<12 it rises abruptly toward $\Delta(^{4}Nt)$ [where it is assumed to be equal to zero]. Consequence: reactions that generate polyneutrons ^{A}Nt are endothermic for A<12. "

It is worth emphasizing that the semi-empirical mass formula for ordinary nuclei is much more reliable that the formula

for the polyneutrons. The reason is obvious. Nobody questions existence of atomic nuclei; masses of many atomic nuclei have been measured with great accuracy. Existence of polyneutrons, on the other hand, remains questionable. For ordinary nuclei modeling coefficients, such as a_v and a_s, match experimentally measured masses. In the case of polyneutrons that approach to modeling coefficients is not possible. In other words, the above formula the mass excess should not be called "semi-empirical."

Slides 7 and 8:
Here we see how to calculate D, a difference between mass excesses of plyneutrons composes of A+1 and A neutrons. Consider polyneutrons composed of 126 and 125 neutrons. In that case the value of D (in MeV) is $a_v + 0.133*a_s$. Numerical values of the modeling coefficients, a_v and a_s, are not specified on these slides. In the previous version of the polyneutron theory the a_v was 1.90 and the a_s was zero.

Slides 9 to 12:
These four slides remind us about showers of alpha particles reported by Oriani and Fisher at ICCF10. Using a cell identical to the one they used I was not able to convince myself that such showers exist. In private e-mail messages Oriani wrote that his recent attempts to observe large showers were not successful. Like many other CF researchers, he is struggling with the issue of poor reproducibility. He even tried to speculate about causes of irreproducibility. But that is a different topic.

Slides 13:
Here "composite nuclei," such as $^{16}O^{74}Nt$ are introduced. It is a system composed of an ordinary nucleus and of a polyneutron -- a "nuclear molecules." John writes that such molecules "are bound to each other by a reduction in surface energy over the area of contact. These composites must be stable (except for beta decay) if they are to participate in polyneutron reactions." In other words, he refers to stability with respect to strong nuclear forces only.

Slide 14:
In principle, neutrons can be transferred from one component of a nuclear molecule to another, for example:

$$^{16}O^{74}Nt \rightarrow {}^{15}O^{75}Nt + e,$$

and

$$^{15}O^{75}Nt \rightarrow {}^{16}O^{74}Nt + e,$$

where e, the reaction energy (third modeling parameter) is postulated to be negative and identical for both directions. I think that is not possible; if the first e is negative then the second must be positive, and vice versa. How can direct and inverted reactions be both endotermic?

Slide 15:
Introduces the third modeling parameter, E_b; it represents the energy released when a nuclear molecule is broken. The name of that positive parameter is "composite mass excess." Breaking of a nuclear molecule should not be confused with breaking of a polyneutron (see slide 17).

Slide 16:

Here I see a postulate that the reaction energy E (not to be confused with Eb or with e)
is positive for one particular kind of process:

$$^{18}O + {}^{A}Nt \rightarrow {}^{16}O + {}^{(A+2)}Nt$$

That process consists of growing on a polyneutron in the presence of ^{18}O. About 0.2% of oxygen, in air and water, contains that isotope of oxygen. John takes it for granted, as described in unit #191, that the above reaction was taking place spontaneously in the original experiments of Fleischmann and Pons (and in many other cold fusion experiments). That is why E must be positive in his theory. Accepting this postulate John shows that D (defined in slide 8) must be smaller than 1.955 MeV. Algebraic manipulations leading to this conclusion are straight forward. Note that D, defined, in slide 8, is expressed in terms of his model parameters a_v and a_s.

If one had D=1.955 then it would be an equation involving two unknowns a_v and a_s. In reality it an inequality involving these two model parameters. It is the first constrain on the values of a_v nd a_s. The second constrain appears in the next slide.

Slide 17:
Here I see a postulate that the released energy, E, must also be positive for another kind of reaction, namely:

$$^{18}O + {}^{A}Nt \rightarrow {}^{16}O^{(A-22)}Nt + {}^{12}Nt + {}^{12}Nt \text{..... (provided } A > 33)$$

In other words, large polyneutrons (A > 33), colliding with ^{18}O, can result not only in reactions described in slide 16; such collisions can also result in formations of nuclear molecules accompanied by the emission of two polyneutrons of size 12. I do not know on what bases were the values 12, 22 and 33 chosen. Accepting the positive E for the above kind of reaction John derives the second constrain involving a_v and a_s.

$$3.955 + E_b - 2*a_v + a_s*(A^{2/3} - (A - 22)^{2/3} - 2(12)^{2/3}) > 0$$

The other two constrains are implied -- neither a_v nor a_s can be negative. Two equations with two unknowns would most likely lead to a unique set of two model parameters. The inequalities, on the other hand, can only be used to restrict a region (in the first quadrant of the a_v versus a_s plane) in which the unknown solution is located. I am writing all this as I go from one slide to the next. I do not know how the inequalities are going to be used. What is clear, however, is that the above two processes are going to contribute to a chain reaction which, as explained in unit #191, can develop under favorable conditions.

Slides 18 and 19:
The possible chain reaction consist of two processes, the first consists of the growth of polyneutrons and the second, consists of production of a pair of ^{12}Nt polyneutrons. A chain reaction in air and water would generate enough thermal energy to kill everybody on our planet. Fortunately, this does not happen. What protects us? What stops a chain reaction when it starts developing? According to John, reaction products (right sides of the two equations in slide 18) have the ability to absorb polyneutrons. They are poisons in the same sense as fission products in our commercial nuclear reactors. This is illustrated in Slide 19.

* *

I WILL STOP NOW (6/3/05) AND WILL CONTINUE LATER. BUT BEFORE STOPPING LET ME QOUT AN E-MAIL MESSAGE RECEIVED THIS MORNING. IT HAS NOTHING TO DO WITH PHYSICS. I DO NOT THINK THAT THE WRITER, PAUL BOLOGNA (A BIOLOGIST AT MY UNIVERSITY) IS AWARE OF POLYNEUTRON THEORY OF JOHN FISHER. HERE IS WHAT HE WROTE, ADDRESSING ALL FACULTY MEMBERS OF

CSAM (College of Sciences and mathematics). PAUL WROTE:

“This is a multipart message in MIME format, just some fun summer reading: The Center for Applied Mediocrity of the Institute of Negativity, a major research institution, has just announced the discovery of the heaviest element yet known to science. The new element has been named "governmentium."

Governmentium has one neutron, 12 assistant neutrons, 75 deputy neutrons, and 224 assistant deputy neutrons, giving it an atomic mass of 311. These 311 particles are held together by forces called morons, which are surrounded by vast quantities of lepton-like particles called peons. Since governmentium has no electrons, it is inert. However, it can be detected, as it impedes every reaction with which it comes into contact. A minuscule amount of governmentium causes some reactions to take over 4 days to complete, when they would normally take less than a second.

Governmentium has a normal half-life of 4 years; it does not decay, but, instead undergoes a reorganization in which a portion of the assistant neutrons and deputy neutrons exchange places. In fact, governmentium's mass will actually increase over time, since each reorganization will cause more morons to become neutrons, forming isodopes. This characteristic of moron-promotion leads some scientists to believe that governmentium is formed whenever morons reach a certain quantity in concentration. This hypothetical quantity is referred to as "Critical Morass." When catalyzed with money governmentium becomes administratium, an element which radiates just as much energy, since it has 1/2 as many peons but twice as many morons.”

More Serious Insertion (6/4/05):
Polyneutrons, invented by John, offer a great explanational flexibility but their real existence is not as certain as existence of neutrons. I know of two cases in which neutral particles were first invented and then discovered. They are neutrons and neutrinos. Let me briefly summarize the case of neutrons. Their discovery was announced in 1934 by Chadwick. Beside being a good experimentalist, Chadwick was exposed to 1920s speculations that such particles (hypothetical proton-electron combinations) might exist. This helped him tremendously to recognize real neutrons. Other scientists, especially Joliot-Curie (in 1932), contributed to this discovery.

The first contributors were Germans, Bothe and Becker. They discovered highly penetrating radiation coming from a beryllium foil bombarded with alpha particles. The radiation consisted, as we know today, of neutrons but they believed it consisted of gamma rays. A French scientist Jolit-Curie recognized a contradiction between what was already known about gamma rays and properties of beryllium radiation. In studying penetrating properties of that radiation he discovered paraffin bombarded with it was emitting protons. Neither Germans nor French were familiar with earlier speculations about neutrons. But Chadwick, an English scientist was. After reading Jolit-Curie’s publication he realized that protons might be knocked from paraffin by neutrons. Then he conceived a clever experiment to confirm this hypothesis.

Will the history repeat itself? Perhaps polyneutrons do exist but Fisher’s way of thinking about them is too naive. The first way of thinking about neutrons (proton-electron combinations) was also too naive and has subsequently been rejected. Perhaps existence of polyneutrons will be discovered in experiments that have nothing to do with cold fusion. A scientist familiar with John’s theoretical model might be in a better position to recognize polyneutrons that other scientists. Theoretical speculations are useful, even when they are based on unacceptable postulates.

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228) Cars running on water?

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The webpage whose URL is shown below,
http://www.rexresearch.com/puharich/1puhar.htm#4394230

describes a presumably old, but never widely implemented, invention. "Dr Andrija Puharich reportedly drove his motor home for hundreds of thousands of miles around North America in the 1970s using only water as fuel. At a mountain pass in Mexico, he collected snow for water." But our cars still don't run on water. I do not think that a conspiracy of oil companies is responsible for this. To me it means that something is wrong with the US patent described on the above quoted webpage. This observation is also based on what I know about the first law of thermodynamics. The amount of energy needed to extract hydrogen from water (production of fuel) can not be smaller than the amount of energy one gets from turning that hydrogen back into water (burning that fuel).

But that is not totally satisfactory. Thermodynamics only tell us that "something is not right," it does not identify errors. To i dentify errors one has to address relevant details. Unfortunately, my familiarity with chemistry is very limited. That is why I would appreciate if a qualified electrochemist, reading this short note, could either identify specific errors (in the patent description) or argue that the explanation has no errors. I would be happy to append such input to this note.

P.S.
Figures 6 and 11 reminded me of superwaves described in unit #213. The fundamental frequencies were between 20 and 200 Hz; the modulating frequencies were between 200 and 100,000Hz. My first impression was that the author refers to matching of frequencies of atomic oscillations in water molecules. But, is it not true that such oscillations usually take place at much higher (infrared) frequencies? What kind of resonances can be matched with frequencies smaller than 0.1 KHz?

Short addendum #1 (6/6/06):
William C. Rostron, thinks that "the patent should never have been granted" to Puharich. William wrote:

No doubt there cannot be a violation of the first law of thermodynamics. If it works, then the energy for the breaking the hydrogen-oxygen bond comes from ambient heat; the process acts like a heat pump, ultimately extracting cracking energy from ambient air. In principle, there isn't anything wrong with this idea. Notice the heat fins on "Component III".

The schematic diagram showing the isolation transformer and "Component III" reaction nozzle won't work: there isn't a complete electrical circuit. Somehow, current has to flow to excite the circuit, and that can't happen if the upper winding of the transformer secondary isn't connected to something--even just a capacitor. This is critical, because there is sufficient description of the waveforms and theory to build the circuits designated by the functional boxes, but the output drive transformer circuit is shown explicitly, and it's broken. On that basis alone, the patent should never have been granted, in my opinion.

William emphasized that this is his own point of view, not of the power plant company that employs him at a nuclear

plant.

Short addendum#2 (6/7/05):
Referring to cars fueled by water a physics teacher wrote: "There have been cars driven on water reported previously. The one's that actually perform as claimed use metallic sodium or potassium in addition to the H_2O." Here is my comment on this. As a high school student, more than five decade ago, I saw a demonstration in which a tiny bit of sodium was dropped into water and reacted violently. But I did not know that sodium can be used as an automobile fuel. Looking into a chemistry textbook now I see that hydrogen is released from water by the following reaction:

$2\ Na + 2\ H_2O \rightarrow 2\ NaOH + H_2$

In other words, to produce one mole of H_2 (2 grams) one uses two moles of Na (46 grams). The heat of combustion of H_2 is 495 kJ per m
ole (247.5 kJ per gram) while the heat of combustion of gasoline is close to 50 kJ/gram. This shows that one gram of hydrogen produces about five times as much heat as one gram of gasoline. It also shows that one gram of sodium must be used to generate 5.39 kJ of heat (released when hydrogen is burned). One kilogram of gasoline (little more than one liter) will produce 500,000 kJ of heat. How much sodium is needed to produce the same amount of heat? The answer is 92.9 kilograms. (Feel free to replace the word heat by thermal energy).

Suppose the distance covered by Puharich was only 200,000 miles. A car covering 20 miles per gallon of gasoline would use 10,000 gallons of that fuel. This is equivalent to 929,000 kilograms (nearly 930 tons) of sodium. Note that NaOH is a dangerous pollutant. It is not hard to figure out that a lot of nitrogen hydroxide would be produced during the trip. How was the NaOH disposed of? How often was the vehicle "refueled" with sodium? A question I am not asking has to do with energy needed to extract 930 tons of sodium from NaCl.

Hydrogen cars of tomorrow are expected to be environmentally friendly. Production of hydrogen should also be environmentally friendly. Getting hydrogen out of water with sodium does not satisfy this condition. The iESi device for extraction of hydrogen from water, on the other hand, is said to be not only environmentally friendly but energy efficient as well. I do not think that such claim is valid. But I will be happy to be wrong. Nothing would be more convincing than long-lasting commercial success of their already existing devices, and subsequent scientific papers explaining them.

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229) A Russian patent of Gnedenko et al.

Ludwik Kowalski (6/10/05)
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Today I received a phone call from a reader. That person asked me to translate a Russian patent of Gnedenko and Goriachev. The patent, according to the caller, is linked with inventions described in units #216 and #226. I said I will do my best and post the translation at my cold fusion website.

Addendum (6/14/05). The patent description arrived today and I will start translating it. Actually it is a set of four files. The first is an abstract; it is already in English and I will paste it below. The inventors are listed in alphabetic order; the names of Koldomasov and Yang (see units #216 and #226) appear of the list. But patent holders are Gnedienko and Goryachev.

FILE #1

Russian Office of Patents and Trademarks

(12) Description of application for patent in the Russian Federation

(21) (22) International Application Number 2002128693/06 October 28, 2002
(24) Priority date October 28, 2002

(46) Publication date July 10, 2004
(56) References: RU 2152083 C1, June 27, 2000, RU 2054604 C1, February 20, 1996, RU 2177512 C1, December 27, 2001, SU 334405 A March 30, 1972, RU 2172526 C2 August 20, 2001.

(98) Address for correspondence:
113149 Moscow, Ul. Sivashskaya 6, Bldg 1, Apt. 191, I. I. Petrov

(72) Inventor: **Gnedenko V. G. (RU), Goryachev I. V. (RU), Koldomasov A. I. (RU) Khayunik Yang (KR)**

(73) Patent holder:
Gnedenko Valerij Gerasimovich (RU)
Goryachev Igor' Vital'evich (RU)

(54) **POWER PLANT FOR PRODUCING HYDROGEN AND OXYGEN**

(57) Abstract

An installation designed to produce hydrogen and oxygen, and also for generating power recovered in fusion reactions taking place in a reactor. The installation includes a dielectric housing that is resistant to a cavitating emission for receiving and transmitting a dielectric medium in the form of a mixture of light and heavy water, an insert disposed in the housing and made from dielectric material that is susceptible to cavitating emission and furnished with at least two apertures capable of allowing the dielectric medium to escape. The installation is furnished with a pulser, a pump for

supplying the operating dielectric medium under pressure, a system for deflecting charged particles, and at least two pipes that are electrically insulated from one another and connected to receptacles for collecting hydrogen and oxygen, also a stricture in the form of a Laval nozzle at the outlet from the insert in the housing, and a deflecting system on the housing downstream thereof, after which pipes are arranged. The deflecting system may also be realized by electrostatic or magnetic means. The invention provides expanded functional capabilities. 2 z.p. files, 1 figure.

(*Mixture of light and heavy water*)

FILE #2:
This is also an abstract that has already been translated. I am pasting it below. I wish the emphasis was on scientific principles rather than on the appearance of the device.

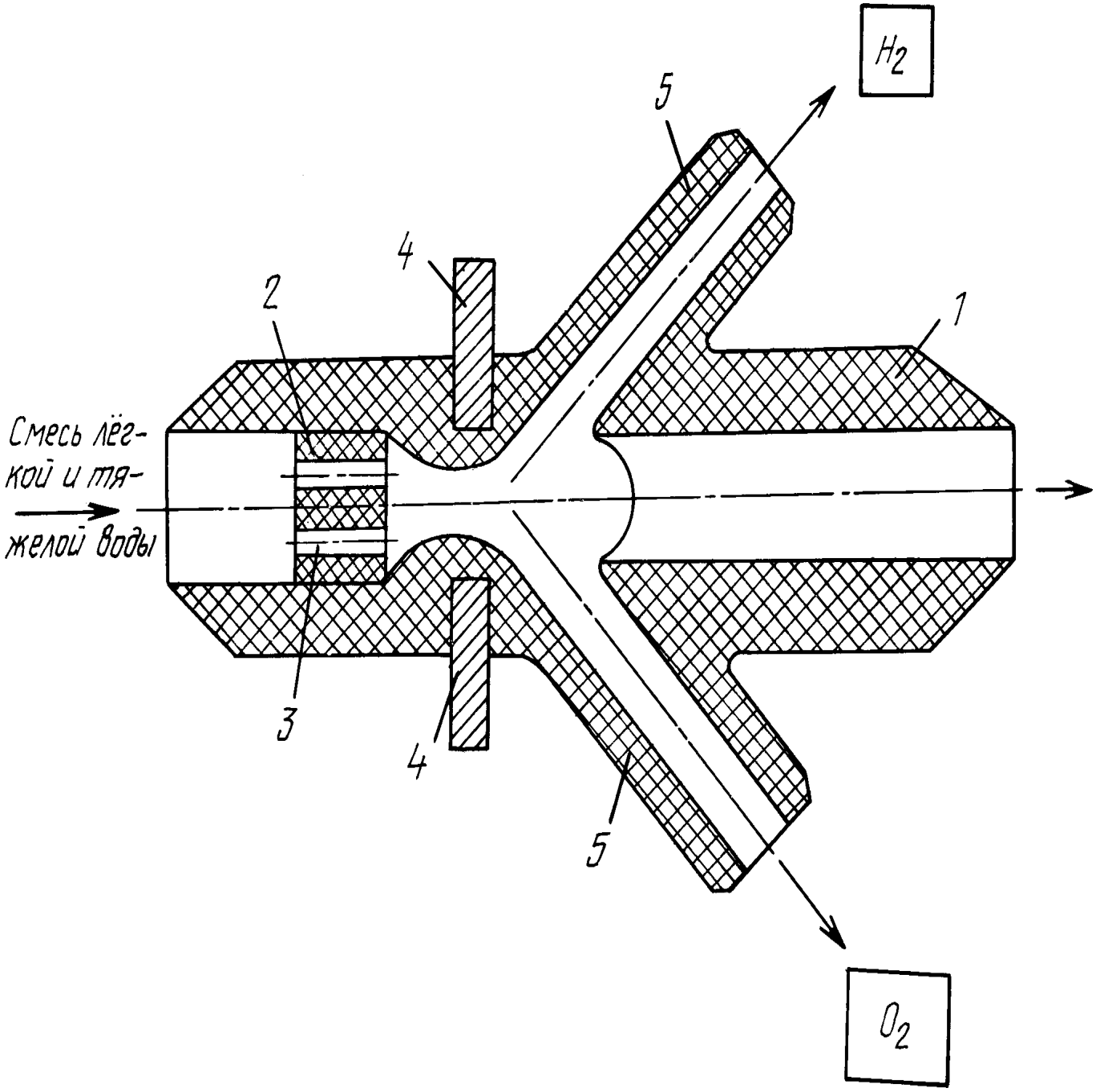
FIELD: **production of hydrogen and oxygen.**

SUBSTANCE: the invention is pertinent to electric power installations. The installation intended for production of hydrogen and oxygen, and also for production of the energy emitted at fusion reactions going in a reactor. The installation contains: a dielectric resistant to cavitational emission housing for intake and gating of the dielectric medium in the form of a mixture of light water and heavy water, an insert mounted in the housing and made out of a dielectric material susceptible to cavitation emission and provided at least with two holes allowing an exit through them of the dielectric medium. The installation is supplied with a impulsator, a pump to deliver the working dielectric medium under pressure, a deflecting system of charged particles and, at least, two branch-pipes electrically isolated from each other and linked with vessels for hydrogen and oxygen collection. At that at exit of the insert in the housing there is a narrowing in the form of Laval's nozzles, behind it along the stream run on the housing there is a deflecting system mounted, behind which the branch-pipes are located. The deflecting system may be electrostatic or magnetic. The invention ensures expansion of functionalities of the installation.

EFFECT: the invention ensures expansion of functionalities of the installation.

3 cl, 1 dwg

FILES #3 AND #4:
File #4 seems to be an illustration accompanying file #3; that is why I am showing it first. It shows how a “mixture of light and heavy water” is delivered to the body of the device (from the left input) and how molecular hydrogen and molecular oxygen are removed (up and down, respectively, as labeled).



File #3 is in Russian. It begins with the abstract (already translated -- see file #1). The abstract is followed by the text shown below. This text is not clear to me; it makes references to presumably "well known" facts and solutions. Be aware that I am going to translate without understanding. Most words are clear but I can not make sense of sentences. Some technical terms are new to me and I have no dictionary at home. For example, what is the "miezelectronnaja camera"? Literally it is a chamber between electrons. But which electrons are they talking about?

Likewise I am confused by the term "cavitational emission." I know that cavitation consists of formation of bubble but I do not know what kind of emission is associated with bubbling. What is being emitted -- particles or waves? If cavitational emission consists of particles then what is their nature and why are they emitted? If it consists of waves then what kind of waves? Is it sound? Is it light? Is it X-rays? How much energy is emitted? I hope that a reader of this translation will somehow understand it. But I would not be surprised of being accused of gobbleygooking; that is how the text appears to me, probably due to my limited background in the area of electrical water processing.

Description of the invention:

This invention, based on physics and chemistry, is related to technique of producing hydrogen and oxygen. Being

related to nuclear engineering it can also be used to produce energy liberated in a fusion reactor.

One known solution [a reference to a 1987 Russian book on water purification is given] consists of a tube through which the processed liquid is delivered to the discharge chamber (with flat and pointed electrodes) and a tube through which it is removed. Also known is a solution [US patent #3969214, 1976] containing a body with an axial opening, the fluid delivery input located in the lower section, the “miezelectronnaja camera” [see above], the anode connected to the positive terminal of the power supply, the cathode connected to the negative terminal of the power supply, and a device producing variable magnetic field.

The limitations of these known inventions is the fact that the anode and the cathode are located on the same side of the “miezelectronnaja camera” [see above]. Oxygen released at the anode is thus mixed with hydrogen released at the cathode. Mixing of these two gases is accompanied by endothermic reactions (production of hydrogen peroxide and ozone). These reactions lower the overall efficiency because they absorb part of the electrolytically generated heat.

Nuclear reactions taking place in the deutron-containing media are well known. They are described by Lipson et al. [two reference to papers published in 1990 and 1992 -- in the Russian journal “Technical Physics” -- are provided]. Continuos operation is not possible in nuclear reactors of known type because heat loses increase rapidly with plasma temperature ($\sim T^{7/2}$) and energy sources extinguish themselves. Nuclear reactions are thus not able to supply excess energy needed to overcome coulomb repulsion and sustain nuclear interactions.

The problem can be technically solved [a 1972 reference is mentioned] by inserting one or several dielectric elements into the passage of the liquid. The pump imposes fluctuations of pressure on the passing dielectric liquid and powerful resonance oscillations at acoustical frequencies (about 1 kHz) are generated in the system. positive electric charges, of high density, appear at the front face of the dielectric insertion through which the dielectric fluid if passing. The charge density depends on the intensity of cavitation, on the dielectric properties of the insertion and of the liquid. This leads to generation of quasi-stationary plasma in the liquid, in front of the insertion. The energy content, however, about 1 J/cm, is not sufficient to compensate for what is used to sustain the plasma discharge.

A known setup, using hydrogen and its isotopes, can generate energy continuously. It consists of a body through which a dielectric medium is flowing. The body should not be affected by cavitational emission; it should contain a dielectric insertion able to produce cavitational emission. The insertion should have at least one opening for the passing dielectric fluid, for example light water whose electric resistivity is about 10^{11} ohm*m. Then approximately 1% of chemically pure heavy water, with identical dielectric parameters, is added. An electrical charge of high density is then formed in the openings of the insertion. High potential due to that charge is able to ionize hydrogen atoms and accelerate them sufficiently to overcome coulomb barrier and lead to nuclear reactions. This was described in the 2000 patented prototype (see patent #2152083 of Russian Federation).

The described unit uses only part of the released energy -- excess heat. Our invention removes this limitation of analogous proposals, and of the prototype. It expands functional possibilities by increasing the intensity of plasma to a level at which energy loses (to sustain plasma) are compensated, and at which hydrogen and oxygen can be efficiently produced.

The setup produces hydrogen and oxygen. Its body is made from the dielectric material resisting cavitational emission. The insertion is made from a material able to produce cavitational emission. A dielectric fluid, in the form of a mixture of light and heavy water, passes through at least two openings in the insertion. The unit is equipped with a pulse generator and with pumps able to supply the compressed dielectric fluid. The setup is also equipped with a deflection system and with two exit pipes delivering hydrogen and oxygen. The pipes are electrically insulated from each other. After passing through the insertion the fluid is directed to the Laval’s nuzzle. The deflection system, either electric or magnetic, is situated after the nuzzle.

The attached figure is a schematic diagram of the energy setup for production of hydrogen and oxygen. Body (1) is made from the dielectric material that can not be damaged by heat and by cavitational emission. It can be ceramics, sapphire, etc. The insertion (2) is made from a dielectric material favoring cavitational emission. It can be asbo-

cement, ftoroplast, etc. There should be at least one opening (3) in the insertion. Each opening is a cylindrical channel whose length is 25 to 30 mm and whose diameter is 1 to 2 mm. The deflecting system (4) is mounted in the body of the device. After the insertion the body has the Laval's nuzzle and at least two exit pipes (5). The pipes are connected with containers collecting hydrogen and oxygen. The deflecting system can be either electric, for example, a parallel plate capacitor, or magnetic, for example, permanent magnets or electromagnets. The magnetic system creates a steady magnetic field in the region between the end of the insertion and the nuzzle.

The dielectric liquid flowing through the setup pulsates with the frequency that is close to the natural frequency of of the insertion opening. The resonance amplifies pulsation of the liquid and cavitation takes place at the entrance the of the opening in the insertion. This is associated with cavitational emission. Exposed to that emission the material from which the insertion is made starts emitting electrons; these electrons enter the flowing fluid. At the same time positive charge of high density is formed at the entrance of the opening. The difference of potential between the charged area and earth approaches one million volts. Hydrogen atoms of the working liquid are ionized when they are in the high voltage region. Positive ions (atomic nuclei of hydrogen isotopes) are then accelerated, inside the liquid, toward the axis of the opening. The concentration of ions (plasma) increases near the axis. Recombination times are sufficiently long and nuclear reactions can take place.

Impulse that ions receive from positive charges on the wall of the opening can exceed 10 keV. [Why is the term "impulse" used instead of kinetic energy?] This creates conditions for nuclear fusion reactions. Accelerated nuclei are able to react despite the coulomb barrier. The rate of nuclear processes can be regulated by changing the concentration of heavy heavy water in light water. The working model has been built; it works in the following way:

Working fluid is a dielectric -- a mixture of light and heavy water in the ratio of approximately 100:1. It is pumped into the body of the device (1) under the pressure of 5-7 MPa. The mixture of water flows through the opening (3) in the insertion (2). The insertion is made from a dielectric material, such as ftoroplast. The length of the opening is 25-30 m; its diameter is 1-2 mm. A pulsator, whose frequency is close to 1 kHz, is used to induce variations of pressure in the flowing mixture. Neither the pump nor the pulsator are shown in the diagram. The resonance frequency of the opening depends on its length and diameter; it also depends on physical parameters of the fluid. The frequency of the pulsator is changed slowly to approach the resonance frequency.

The body of the unit can be made from a transparent plastic material. The beginning of nuclear processes can then be recognized because ionizing radiation emerging from the opening ionizes the surrounding air. It can also be recognized by emission of neutrons, by the increase of temperature of the working fluid, by chemical changes taking place, and other parameters, using appropriate detectors (neutron counters, thermometers, etc.) The nuzzle (Laval's narrowing) is used to speed up working fluid after it becomes ionized. The deflecting system (4) is used to separate ionized particles. Positive and negative ions are deflected in opposite directions by either electric or magnetic field. By increasing the concentration of heavy water in light water, for example, to the 100: (3-5) ratio, a change in ionization can be observed. Production of hydrogen and oxygen increases at the same time. Ionized dielectric fluid moves rapidly through the magnetic field in the nuzzle and ions are subjected to Lorenz force. That is how positive ions are separated from negative ions. Positive ions move to one exit and negative ions move to another exit. Relaxation is difficult because the two flows are electrically insulated. In collective vessels Ions recombine and become either hydrogen or oxygen.

[The last paragraph is the summary of the invention: It is a single sentence consisting of 121 words. It would be very difficult to translate. I am going to leave this task to a professional translator. In my opinion sentences of that kind are not useful.]

Addendum (6/17/05)

The magnitude of the centripetal Lorentz force is F=q*v*B, where q is the charge of the ion, v is its speed and B is the magnetic field. The centripetal acceleration due to that force is F/m, where m is the mass of the ion. Comparing this acceleration to v^2/R one finds the formula for the radius of curvature, R=(m*v)/(q*B). The m and q are fixed, for a

given kind of ions, but v and B can be adjusted. For ions of hydrogen ion $q=1.6*10^{-19}$ C and $m=1.67*10^{-27}$ kg. Assuming, for example, that v=0.1 m/s (speed of water in the nuzzle) and that B=0.1 T (due to common magnet) one gets $R=10^{-8}$ m. That is a very small radius; I do not understand how to make it consistent with deflection of hydrogen ions in flowing water. Even when v is 10 m/s R is only one micron.

I wish a person familiar with the device could explain cavitational radiation and separation if ions in a flowing water. A student in an introductory science course would probably asked for the source of energy used to separated atoms of hydrogen from atoms of oxygen in water molecules. S/he would argue, correctly, that the separation energy, per molecule, must be equal to the energy released when hydrogen is burned in air (producing water). The authors claim that thermonuclear energy is released in the device. What evidence do they have that this really happens?

The iESi device is claimed to generate excess heat at the rate of 10 MW. Suppose that this is due to production of helium via cold fusion. How much helium would be produced in the device each day? It is not difficult to answer this question. The energy released in producing one helium atom (from two deuterium atoms) is 23.7 MeV. This is $3.8*10^{-12}$ J. The excess energy generated at the rate of 10 MW will produce 10 MJ of energy per second. The number of helium atoms produced in each second must thus be equal to $10*10^{6}/3.8*10^{-12}=2.6*10^{18}$. In one day the number of helium atoms would be 24*60*60=86,400 times larger ($2.3*10^{23}$ atoms). That amounts to easily-detectable 1.5 grams of helium per day. Air, on the other hand, typically contains no more than several microgram of helium per cubic meter. Reproducible detection of grams of helium, commensurable with excess energy, would be a highly convincing argument that the excess heat is indeed due to cold fusion of deuterium, as claimed by the inventors.

Addendum (6/19/05)

Browsing the Internet I found the website of PhysOrgForum Science. There I found a recent (6/11/05) posting in which Bearded Clem wrote: "I've been keeping an eye on IESI since I read another cold fusion thread on these boards and recently read on their website that the 2 chief scientists of IESI have come here to Edmonton where I live and I am very interested to find some unbiased proof that what they claim is real because if it is, it will be the breakthrough we need to survive the 21st century. If anyone is interested, here is the text form of a .pdf you can download of the <www.iesiusa.com> website. [See below.] It's a little vague for a skeptic like me but compelling none the less.

White Paper On Plasma Heat and Hydrogen Generator

By Dr. Norman L. Arrison

iESi has acquired the most significant technology of the 21st century through Dr. Hyunik Yang and his team from around the world. Their technology draws on the energy of the atom and converts that energy into useful energy in the form of heat in one device and the splitting of the water molecule into hydrogen and oxygen in another device. Dr. Yang's team has varied in size and composition over a ten year period of research. The consistent aspect of the team has been their international stature and dedication to hard work under the leadership of Dr. Hyunik Yang. Dr. Hyunik Yang is 47 years of age and has a distinguished record beginning with his B.Sc. in Engineering from Korea followed by his Ph.D. and post doctorate degrees at Columbia University in New York. He then had a successful career with Hyundai where he was contracted out to NASA and where he won the Eastman Kodak Award for the best paper and an ASME conference. Dr. Yang then went to Russia where he became a member of the Russian Academy of Science. With the distinguished scientists Dr. Yang had worked with, they decided to build a unit to produce power for mankind based on the energy in the atom.

The approach they used was brilliant. They used resonate harmonic frequencies to expose the nuclei of atoms so they could put the nuclei together to obtain the energy from the fused product. Their system is inexpensive, safe, and easy to operate and construct. The first plasma device will produce heat by taking water and converting it to steam. This device is expected to be working by late 2004 and an early prototype is already functioning. the early prototype produces 14 times the energy put into it and the final product is expected to produce 200 times the energy going into the unit. The second plasma device is expected in early 2005 and it will use it's energy to split the water molecule into hydrogen and oxygen. This device is already working in an old prototype which produces the hydrogen and oxygen and immediately recombines the two in a hot hydrogen and oxygen flame. The old hydrogen-oxygen device was the first proof that the team had successfully tapped the energy of the atom. It only produced 50% more energy out than went into the device but showed that the energy of the atom was being drawn upon.

iESi got control of this technology through the special relationship which exists between Dr. Hyunik Yang and Mr. Patrick Cochrane the founder of iESi. Mr. Patrick Cochrane is married to a Korean attorney and through her got to know of Dr. Yang's work. Mr. Cochrane was so fascinated

with the technology that he helped Dr. Yang with his funding which up to that time had been carried by Dr. Yang and his immediate family. Because of Mr. Cochrane's assistance for Dr. Yang's work, a close relationship blossomed which has resulted in the formation of iESi as it exists today. We at iESi feel very proud that we are the ones bringing this historical changing technology to the world. Plasma heat generation alone guarantees that the cost of electricity will be stable for all mankind. The hydrogen and oxygen producing technology guarantees a clean planet for humanity. The result is that iESi should be the most significant company of the 21st century.

Quoting a well know cold fusion researcher Clem wrote: "he had reason to believe that a commercial cold fusion device would be marketed within a year by a group out of Edmonton, Canada." I am not surprised that a person from that town is so excited about what is going on. In a 6/14/05 message another contributor (Panthom) wrote: "Since the MIT CF Colloquium, both Professor Peter Hagelstein and Martin Fleischmann have personally witnessed this cold fusion technology in the facility in Edmonton where IESI has their scientists sequestered. These two men are providing the unbiased proof the world needs that what they claim at IESI is true. But Patrick Cochrane at IESI is in legal trouble with the securities authorities in Alberta, so he's being forced out of the company by their investors, which can only help the company."

I do not know how reliable this information is. But, as some cold fusion researchers know (from a recent e-mail message sent to them by X), Martin Fleischmann was invited to participate in the demonstration of the device. The author of that message, X, was also invited. I would be happy to append a description of the last week Edmonton demonstration.

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230) Two Russian papers

Ludwik Kowalski (6/20/05)
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1) Please be aware that my unit #229 now has two added sections, at the end. They were not there when the unit was posted, several days ago.

2) It seems to me that **we really need a forum for discussion**, among cold fusion researchers. Unfortunately, the discussion list of ISCMNS, promised at ICCF11, did not materialize. And what happened to the online journal promised one year earlier? Let me make Éa suggestion. I have an idle discussion list at the university server; it can become a forum we need. To be a participant simply send me a private message whose subject line should say: "Discussion of CF." The first line (in the body of the message) should show your name (first, then last) followed by the e-mail address. The next line (or lines) should say why you consider yourself to be a cold fusion researcher. Participation in at least one of the ICCF* conference, for example, is a sufficient reason, as far as I am concerned. My e-mail address is <kowalskil@mail.montclair.edu>. The list will be activated as soon as the number of participants exceeds 20. Please, send this invitation to serious CF researchers you know; not everybody will see it here. Cut this paragraph and paste it into messages you sent.

3) **Today I received two Russian papers** from ∞ one of the readers. They seem to be related to the device invented by Koldamasov and Yang. (see units #216, #226 and #229). These papers prompted me to start this unit. I will try to translate them and make some comments. The copies of pages mailed to me do not identify the place of publication. But the sender wrote that the papers were "published in 2001, following the 2000 Sochi conference."

Paper 1: Studying of thermal and radiation effects in Koldamasov's cell;" by D.S. Baranov.

Paper 2: An experimental setup based on Koldamasov's cell;" by A.P. Andreev, D.S. Baranov, A.K. Molodov, A.K. Pokrovski and N.N Sikavatkin.

The authors of these papers are from Moscow (Scientific Institute of Crystallography, Russian Academy of Science, and MRTI (?)).

Translation of sections from Paper #2

Abstract: A temperature increase of short duration ø was observed in Koldamasov's cell when heavy water was locally added to the working fluid. The temperature did not changed when ordinary water was added, under analogous conditions. Adding lithium chloride to water resulted in registration of radiation in the scintillation detector.

This study was undertaken to reproduce previously reported results (1,2) and to enlarge the scope of a more recent investigation (3). The central element of that installation, the Koldamasov cell, was made from the transparent organic glass. Bright glowing discharge (plasmoid) was observed in front of the opening in the insertion when cavitation was taking place. The insertion changes the cross section, available to the fluid, by two orders of magnitude. Pressures in cell were changed in the range of 10-40 atmospheres at the frequency of ~1000 Hz.

The working fluid was oil whose temperature was ~33 C. At that temperature the viscosity of Δ heavy water exceeds

the viscosity of ordinary water by ~20%. This, however, could not lead to a significant increase of friction (resulting from addition of heavy water) because the viscosity of oil is ~20 times higher than that of water. Sensitive calorimeters were used to measure difference in temperatures between the input and output of the cell. The electrical diagram is shown in Figure 1. It is essentially a Wheatstone bridge in which R1 and R2 are thermistors. Medical injectors were used to introduce heavy or ordinary water at the entrance of the pump. The injected volume was one cubic centimeter and the time of each injection was ~10 seconds. The time was sufficiently long to mix the injected water with ~ 1000 cubic centimeters of oil.

In the first experiment heavy water was injected first and ordinary water was injected later. In the second experiment ordinary water was injected before heavy water. Figure 2 shows that difference fls in temperature occurred after injections of heavy water only. [In that figure changes in difference of temperatures are expressed in volts but the bridge was calibrated]. According to calibrations, changes shown in Figure 2 were the same as from the injection of one cubic centimeter of water at 100C. Trying to optimize generation of excess heat one observed cases in which no excess heat was generated and cases when changes in temperatures were sufficiently high to melt ftoroploast insertions. Destruction of the insertion was observed three times, but only when oil was mixed with several cubic centimeters of heavy water. Insertions were never destroyed when oil contained no heavy water (even at much higher temperatures and pressures).

Radiation emitted by the cell was measured by using a 5 cm by 5 cm cylindrical NaI detector. The detector was ~8 cm from the insertion. Radiation entering the detector had to pass through an aluminum foil of 0.5 mm, 5 cm of organ flic glass and a 5 mm layer of ftoroplast. [In other words it consists of gamma rays.] The setup was calibrated by using the annihilation line (511 keV) and a sodium line of 1460 keV. The multichannel analyzer spectra are shown in Figure 3. ..... [The background does not change significantly when the motor of the pump is turned on to off. A significant above-the-background component appears (in the gamma ray energies up to 250 keV) after 0.8 cubic centimeters of the water solution of lithium chloride, concentration 4 N, is injected into oil. The conclusion is:] Radiation measurements and temperature effects due to heavy water are with Koldamasov's findings and show that nuclear processes do take place in the cell.

[I am certain that scientists who prepared the last week demonstration in Emonson did not miss an opportunity to show, using a widely available detector, that gamma rays are indeed emitted from their setup. The setup generating excess heat at the ra Ëte of 10 MW must be much larger than the setup used by Baranov. When will the results of the last week demonstration be announced?]

References:
1. A. I. Koldamasov "Nuclear fusion in the field of electrical discharge;" 1995 Sochi conference report.
2. A.I. Koldamasov "Nuclear reactor;" Patent of Russian Federation #2152083

3. A.P. Andreev et al. (see paper 3 below).

Translation of sections from Paper #1
Abstract: A compact experimental setup, analogous to that described by Koldamasov (1,2), was built. It shows that stable glowing discharge (plasmoid) is produced in the liquid under the influence of cavitation. [Does this mean that what was called "cavitational emission," in unit #229, stands for sonoluminescence?]

According to (1,2) a new approach approach is possible to generate thermonuclear energy. An independent confirmation of this could lead to a broad investigation of that approach. Direct c onfirmation of Koldamasov' work is difficult because the working fluid in his device was pure (double distillation) dielectric water. The reason is simple; the dielectric properties of pure water change drastically after it passes through the cell. Changed water must be purified before it can be reused.

In this investigation we tried to use a different working fluid -- organic oil. Dielectric properties of oil are more favorable but that substance is about 20 times more viscous than water. We were able to find conditions under which bright glow discharge (plasmoid) is formed in oil. The diagram of the setup is shown in Figure 1. [I shows a loop in which oil is forced to circulate under pulsating pressure. Numerous technical details (pump rpm, pressures,

frequen Ocies, etc. are provided. A set of three dots in the part of the article that I decided not to translate. Set of three dots below stand for was not translated] . . . The central element of that installation, the Koldamasov cell, was made from the transparent organic glass. It has a dielectric insertion with a narrow opening (cross section being 100 times smaller that in the tube through which the oil is supplied to the cell). Stable glowing discharge was observed at the entrance of insertions made from organic glass, ftoroplast and polysterol. It was observed when the insertion was made from vinoplast. Ftoroplast turned out to be the most effective when the pressure was ~30 atm. Appearance of plasmoid must be due to cavitation..... Characteristic speed of the fluid through the insertion opening (radius 0.5 mm) was 50 m/s. .....~

The total volume of working fluid was ~6 liters. It is remarkable that oil was not spoiled during its prolonged (`10 hours) circulation through the loop. An attempt to use less fluid liquid, a mixture oil and kerosine, also resulted in formation of plasmoid. But the pump deteriorated rapidly when that fluid was used. The setup is sufficiently compact to be used in practical applications.

References:
1. A. I. Koldamasov “Nuclear fusion in the field of electrical discharge;” 1995 Sochi conference report.
2. A.I. Koldamasov “Nuclear reactor;” Patent of Russian Federation #2152083

4) Post scriptum:
This piece shows how main players of the company were described in the 5/23/05 “news and information” section of PRNews <http://sev.prnewswire.com>. The title of the article was “Esteemed Quantum Energy Physicists Dr. Hyunik Yang and Dr. Nahm Cho Arrive in Edmonton.” They will certainly become famous if the company’s offerings (“Direct Electrical Power Generation, low-cost Hydrogen Generation and low-cost Heat Generation, all three of which were developed by Dr. Yang”),turn into commercially successful technology.

Innovative Energy Solutions, Inc. (iESi) today announced the arrival of the Company's co-founder and Chief Technical Officer, Hyunik Yang, Ph.D. and Dr. Nahm Cho in Sherwood Park, Alberta, just southeast of Edmonton. The recent relocation of Dr. Yang and Dr. Cho should expedite the efforts that are already well underway on the Direct Electrical Power, Heat and Hydrogen Generation units and allow the Company's team of scientists to advance the impressive work they accomplished for iESi while in South Korea. "Much of iESi's proprietary intellectual property is based on the research in the field of quantum energy generation authored by Dr. Yang," said Patrick Cochrane, CEO, iESi. . . .

In his career, Dr. Yang has also held positions as professor at Hanyang University and as senior research engineer at Hyundai Electronics. Dr. Yang has designed several new inventions in the field of quantum energy and cold fusion. Dr. Yang received his Engineering B.S. from Hanyang University in South Korea, and completed his Engineering M.S., Ph.D. and post-doctoral work at Columbia University in New York. He is a member of the American Society of Mechanical Engineers, Society of Automobile Engineers, Korean Society of Mechanical Engineers, Russian Academy of Natural Science, Korean CAD/CAM Society and the Korean Society of Machine Tool Engineers. Since 1997, Dr. Yang has been listed in the Who's Who in the World, and in the Who's Who in Science and Engineering since 1998.

Dr. Cho also held positions as professor at Hanyang University. Dr. Cho is the author of four patents in South Korea and is an expert in the field of Nuclear Transmutation and Fusion. Dr. Cho received his Precision Mechanical Engineering B.S. and M.S. from Hanyang University in South Korea, and completed his Ph.D at the Tokyo Institute of Technology. He is a member of the Japan Society of Mechanical Engineers, Japan Society of Precision Engineers, Korean Society of Mechanical Engineers, Korean Society of Precision Mechanical Engineers, Korean Society of Manufacturing Engineers and the Micro Biochip Center.

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231) Gold from carrots?

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A reader made me aware of a much older "revolutionary invention" that has never been explained scientifically -- Papp's engine. Google brings many descriptions of that extraordinary device. In 1968 Bob Said <www.rexresearch.com/papp/1papp.htm> wrote:

". . . Let's take a hard, skeptical look at this engine. It was designed by a Hungarian-born inventor name Joseph Papp. He and financial backer Don Rosen, who have set up a firm called Environetics, Inc., to develop the engine, are saying mighty little about the details of what makes it work until their patent applications have been granted. But this much they will say:

The engine operates on a charge of gas blends, hermetically sealed inside each cylinder above the piston. A charge of low-voltage electricity, which can come from either a 12-V or 24-V source common to light aircraft, is used to create an electrical field in or around the cylinder. This causes the gas to change from its original form to a new form which requires m /ore space. As it expands it does two things: pushes the piston down and creates --- they aren't saying how --- the conditions for returning to its original form. The heat generated by its expansion is absorbed by its contraction. When it is contracted, another charge of electricity causes it to repeat the expansion-contraction cycle, and so on ad infinitum. When this sequence of events occurs in an orderly phase among 4, 6 or any convenient number of cylinders, suitably connected to a crankshaft, you get useful work.

How much work? It's pretty much a paper solution at present, but the developers say any amount you want, from the amount necessary to drive a lawnmower on up through automobiles and airplanes to the amount needed to power a locomotive or a battleship..... Rosen stressed that what Papp has done is design a fuel, not an engine. The secret blend of gases is the key. Using a power source to move pistons and a crankshaft is old hat to reciprocating engine technology. And Papp's "key" can be applied to turbine technology as well. . . .

Moreover, any reasonably sophisticated reader will wonder about safe, tidy little scientific ideas like entropy, conservation of energy and one or two others. Because they are unwilling to divulge hard details on the concept until patent protection is complete, Papp and his associates leave us nibbling at these annoying problems. But then, the scientific community thought Edison, Marconi, the Wright Brothers and Robert Goddard were out of their minds, too. And look what they did for us. So don't scoff too soon. After more than two decades fas reporter and editor for newspapers and magazines from coast to coast I have been exposed to more than my fair share of perpetual motion machines, miracle cancer cures, devices for communicating with poltergeists, methods of extracting gold from carrots and similar schemes. I do not recall one which caused my reporter's instinct for a good story to resonate more strongly than Papp's engine does..... "

Some interesting comments on this never-commercialized invention were made in 2003 by Eugene Mallove; his article is in the compilation posted at the above website. Even more interesting is the piece attributed to Richard Feyman. The posted articles are worth reading and thinking about. Was Koldomasov (see my units #216, #226, #229, and #230) inspired by Papp's work? It would be great if devices described at the iESi website could perform as outlined by promoters. Our polluted world is in urgent need for "clean energy" inventions. Such inventions should be explained and used immediately. Small scale applications, if they work, would be rapidly scaled up and used all over. Secrecy

seems to be working against desirable things. Without secrecy the premature death of Papp would not prevent others from working on the fuel he invented.

In a long run, technology cannot develop without scientific understanding. I think that “tidy little scientific ideas like entropy and conservation of energy” should not be ignored by inventors.

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232) Free energy and its impact

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An piece about an inventor:

Arthur A. Axelrad (MD, PhD, FRSC, Emeritus University Professor University of Toronto) is a friend, and a biomedical coworker, of Dr. Paulo Correa. In an interesting article:

http://www.aetherometry.com/axelrad_letter.html

about Correa (April 3, 2002), entitled "**PAGD, Aether Motors, and Free Energy**," he wrote:

"I would like to tell you what I know about Dr. Paulo Correa and his partner and wife Alexandra, two people who have recently done something marvelous. What they have done is to make a series of startling discoveries in basic science - beginning with their work in plasma physics, a field that is almost certainly going to have a major impact on our world in the near future.

. . . One day, Barbara, my wife, and I witnessed a demonstration by Paulo and Alexandra Correra of this 'Aether Motor' - it was an electrical generating system that could deliver electrical power without any external power input save its connections to two 'orgone accumulator' boxes or to either our insulated bodies or a ground pipe. Since the device moved a motor and drove a circuitry, it had to consume some power; this appears to have been provided by the environment. The event occurred with incredible calm - no explosion, no noise even, no sudden heat, no bright light, just the quiet pulsation of a discharge tube and a quiet turning of a small rotor. Save for the driving of the motor from contact with our bodies, the effect was almost disappointingly banal. It has not always been that way. There were occasions during the evolution of these discoveries when accidental electrical discharges did threaten the lives of our intrepid pair. Fortunately, these accidents never deterred them.

The realization of what we were looking at was mind-boggling. Here before our eyes was what I was brought up to believe to be absolutely impossible! The implications were also enormous - a world of literally free energy without pollution by a 'product readily producible by available equipment and processes at a cost that allows mass marketing for multiple applications'. You would have expected a scene like a Boxing Day Sale in Toronto. But nothing like that happened. Why? I have given a lot of thought to that question.

When an investigator presents the scientific community with a concept that challenges previous beliefs, there follows a series of stereotyped responses: 'He (or she) is wrong.' 'He can't be right because it goes against what has long been accepted as true by everyone.' 'He is self-deluded but wants so desperately for his concept to be widely accepted that he unconsciously selects the data that fit and rejects the data that don't,' or - 'He's lying!' Or 'This isn't even his field, what right does he have to challenge the work of many years by highly trained experts?' Or 'He doesn't work out of a renowned university or institute or major company. How could he be doing anything like what he claims to be so important?' Or 'If we support a thing like this and it turns out to be a fraud, we'll have wasted our company's money

and we'll be considered fools.'

Once all of these responses have been uttered and evidence overwhelmingly shows each to be unable to account for what is actually being seen, then it is time for a paradigm shift. I believe that this is what has been happening in the case of Paulo and Alexandra Correra. Despite all the exciting developments, however, money to commercialize these discoveries has not been forthcoming from anywhere. This has not been for lack of trying by the Correas, nor for lack of interest by potential backers. Many have come to them from all over the world and have seen striking demonstrations of the XS NRG TM PAGD reactor, the motors it drives and the batteries it charges, or of the Aether Motor developed at ABRI. These inventions are solidly protected by world patents. They are extensively documented in the patents themselves and recently on the Internet. Nevertheless, the Correas are, at the present time, in the process of shutting down their laboratory for lack of funds."

I see no evidence that Correas' inventions, or inventions described at the iESiUSA website, have anything to do with fusion of atomic nuclei.

An old piece written by a physics teacher:

In a 1993 book, entitled "Uncommon Sense: The Heretical Nature of Science," I found a section devoted to cold fusion. The author, Alan Cromer, is a professor of physics at Northwestern University. I remember that name because, many years ago, I was using an introductory physics textbook he wrote. Cromer's description of cold fusion is worth reading; it represent a typical point of view of a mainstream scientist.

"..... It thus is clear that cold fusion is possible if two deuterium atoms can be squeezed together closer than they are in a deuterium molecule. It is also well known that many metals, including palladium, absorb hydrogen. Therefore, isn't it reasonable to , suppose that if deuterium were forcibly incorporated into palladium by using an electrical current, deuterium atoms could be squeezed together close enough for their nuclei to fuse?

No, it isn't. The palladium atoms are themselves three times farther apart than are the two deuterium atoms in a deuterium molecule. The palladium is able to absorb deuterium molecules because the spacing between the palladium atoms is larger than the diameter of the deuterium molecule. No squeezing is involved. In fact, the deuterium molecule breaks apart inside the palladium, and its two deuterium atoms end up being farther apart in the palladium than they were in the free deuterium molecule.

Furthermore, Fleischmann and Pons claimed that their fusion reaction generated a large quantity of heat. A simple calculation shows that if the heat they claimed was due to fusion, there would have been enough neutrons generated to have killed the experimenters. They interpreted the absence of the neutrons as the discovery of a new type of nuclear reaction.

Scientists weren't immediately aware of all this when the announcement was made at a press conference. So when reporters asked scientists for their assessment of the Utah experiments, there were mixed responses. Philip Morrison observed, 'Based on the information I have, I feel it's a very good case." He said his confidence in the reality of the reaction was "high, but not conclusive.' The Nobel Prize-winning physicist Sheldon Glashow said, 'I don't believe a word of it' (Chandler, 1989a, p. 5). The amusing comment by Kim Molvig -- 'I am willing to be open-minded, but it's really inconceivable that there is anything there' (Pool, 1989, p. 1661) probably reflects the ambiguous use we often make of 'openminded.' Most alarming were the comments from scientists that put extraordinary confidence in Fleischmann and Pons: "I'd be extremely surprised if they've done anything stupid. They have a very good track record in electrochemistry. I am pretty excited about this" (Chandler, 1989b, p. 29). In fact, stupidity in science is far less surprising than is the radical overthrow of well-established doctrine.

I was one who was convinced from the beginning that the results were bogus. You could say I was closed-minded on the subject in the sense that I had a deep inner conviction that the experiments were wrong. Within days of the announcement, we had an impromptu meeting in my department to discuss the matter. By then, prepublication copies of the papers of Fleischmann and Pons and the Jones group had arrived over the fax machine. As I examined them, I judged the first unsuitable for publication and the second an interesting and publishable work that was probably

wrong. My judgments at that time were more negative than those of my colleagues but were in accord with those of the reviewers at Nature, who rejected the Fleischmann and Pons paper while accepting the Jones group's paper.

In their original press conference, Fleischmann and Pons stated that they had been working on cold fusion ‘in secret’ for five years. This tipped me off at once. Science isn't done in secret. It's too easy to make a mistake. This is true for electrochemists doing electrochemistry and even truer for electro chemists doing nuclear physics. A theoretical nuclear physicist myself, I have a small idea how hard my experimental colleagues work to get everything right. It's a process of constant refinement in which comments and criticisms of other scientists play a vital role. To me, Fleischmann and Pons were acting like a couple of alchemists for whom the scientific revolution had never happened. The public, on the other hand, and many scientists as well, were taken by the romantic notion of two chemists working alone to defeat the billion-dollar hot-fusion physicists. The chemists ate it up.

As soon as [saw the Fleischmann and Pons paper, I knew my worst suspicions were correct. The paper lacked any references to nuclear physics and, indeed, had hardly any references at all. References are used in all scholarly writing to establish the author's knowledge of the relevant work in the field. A typical scientific paper has about thirty citations, most to recent related work. Fleischmann and Pons had five references, three to their own work and two to works on hydrogen absorption in metals. (One of these was to a 1940 Russian paper.) There was no reference to the huge literature on fusion. A scientific paper differs from a nonscientific one by the requirement that it be consistent with the previous work in the field. This means it must acknowledge the existence of this previous work and show how the new results fit in with the old. A paper may challenge previous work, but it can't ignore it.

The Jones group's paper has eighteen references. They indicate their knowledge of the unlikelihood of cold fusion, but argue that something unusual happens when the hydrogen is absorbed into matter. They interpret some geophysical data as indicating that cold fusion is going on inside the earth. This connects their work to the rest of science. They also give their raw data-the number of neutrons they detect when their cell is off and when it is on. The difference, if any, can be attributed to fusion. Their data are shown in Figure 8.1. The vertical bars on the points indicate the probable error; that is, the true value is likely to lie somewhere on the bar. To accept cold fusion, you have to believe that the positive results around the lOO-channel mark isn't a statistical fluctuation. This 1 find very difficult, given the Jones group's claim that the neutron production "dropped dramatically" after eight hours. Statistical fluctuations are well known to produce dramatic changes in small numbers; that's what makes gambling so exciting. Many other laboratories have since tried to duplicate the Jones group's result. Although some claimed to see a few neutrons, none of these results has held up over time."

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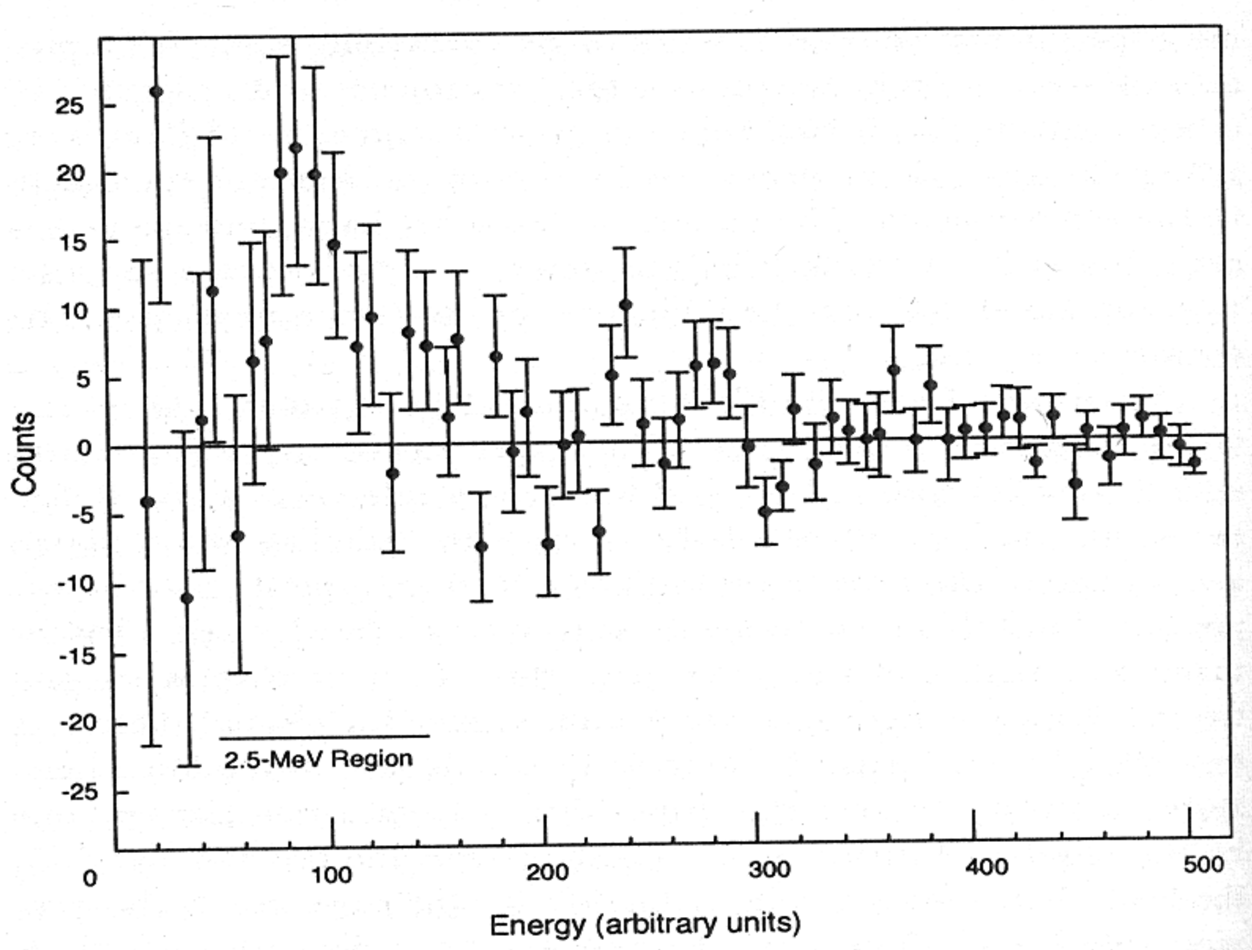

Figure. 8.1 The number of neutrons of different energies detected by Jones et al. with the electrolytic cell on, minus the number detected with the cell off. There is a positive difference in the energy region expected for fusion neutrons (2.5 MeV). Although this difference is statistically significant on its face, such results are sometimes found to result from a bias in the way the data are handled. (Reprinted with permission from S. Jones et al., 'Observations of Cold fusion in Condensed Matter,' Nature" 338 [1989): 737) [I do not know what kind of "bias" was Cromer referring to. Emission of unexplained neutrons was confirmed in subsequent experiments of Jones, as described in units #113 and #131.].

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The fact that two Utah research groups both had reported cold fusion at the same time greatly confused the picture, as they seemed in some way to confirm each other. However, the most casual reading of their papers, or of the newspapers at the time, clearly showed that the two results were highly inconsistent with each other. The small fusion rate indicated by the neutrons detected by the Jones group, though inconsistent with current nuclear and solid-state theory, isn't as inconsistent as the trillion times higher rate reported by Fleischmann and Pons without the production of the necessary neutrons.

The Fleischmann and Pons paper, for all its extravagant claims, offers no data. Although Fleischmann and Pons measured the temperature change in their fusion cell, they give none of their raw readings. They report only highly processed numbers, without giving any indication how they were calculated. When they saw only one-ten-trillionth (10-13) of the neutrons that their claimed fusion rate should have produced, they claimed the discovery of 'an hitherto unknown nuclear process or processes.' When they came in one morning and found their cell burned out, they claimed that fusion did it.

I was surprised how many scientists were prepared to relax their usual skepticism in the face of such an unscientific paper. The chemists were particularly excited, and at a meeting they had great fun claiming that two of theirs had done in five years and with a few thousand dollars what hundreds of physicists with billions of dollars haven't been able to

do in forty years. A little team spirit is a good thing, but this was ridiculous. Back home, I was lecturing my physics students on the absurdity of the Fleischmann and Pons claims while their chemistry professor was excitedly expounding on the importance of the Utah result.

There are many lessons from this episode. First, scientists themselves are often poor judges of the scientific process. This isn't as surprising as it seems, since their training is purely technical. Many don't appreciate the seriousness of violations of procedures, such as giving a press conference before publication. More surprising, many took Fleischmann and Pons's incredible conclusions about their own work at face value, even before reading their paper.

Second, scientific research is very difficult. Anything that can go wrong will go wrong. For instance, Fleischmann and Pons forgot to stir their cell while measuring its temperature, thereby totally invalidating their measurements. Stirring is a routine procedure, taught to freshman in their first heat experiment, but neglected by a Fellow of the Royal Society working with the chairman of the Chemistry Department of the University of Utah. This isn't to criticize their professionalism; it would happen to any small group working in isolation on a fixed idea. No one, no matter how experienced, can do a complex experiment without the guidance and criticism of others. Isolation is the death of science.

Third, science isn't dependent on the honesty or wisdom of scientists. As a collective enterprise, it rises above individual shortcomings. It will survive Fleischmann and Pons, but only after the wasteful expenditure of hundreds of man-hours of work and at least one death."

The third conclusion is worth thinking about. Science is a collective enterprise with more or less well established rules of validation. One of these rules states that, in principle, theories guide but reproducible experiments decide. That does not mean that irreproducible experimental observations, reported by qualified people, should be ignored. In the present state of its history cold fusion research is guided by not-always-reproducible experimental observations. All by themselves such observations are not in conflict with theoretical models, they are in conflict with other observations. Conceptual conflicts appear when attempts are made to explain experimental data. The model of two fusionning nuclei, for example, is in conflict with the well established theory of tunneling effect. Attempts to interpret irreproducible "excess heat" results turned out to be counterproductive.

History of the excess heat discovery would probably be very different if the phenomenon were labeled as "anomalous heat," rather than "cold fusion." Premature interpretation, implied by the term cold fusion, combined with highly unconventional press release, were partially responsible for the prevailing attitude toward Fleischmann and Pons. But all this would probably become inconsequential if experiments became widely reproducible. Promoting technological applications of irreproducible science does not make any sense to me. The emphasis, at this time, should be on science not on technology. Scientifically unjustified claims of "great inventions" hurt the reputation of honest research. I do not know why a working model of a "free energy" device is not required before a patent is granted to an inventor.

What will do more harm to real cold fusion research, scientifically unjustified claims or negatively-biased attitude of scientific establishment? Inventors should respect scientific explanations, editors of our journals should not deny the peer review process to qualified cold fusion investigators. As a novice-part-time-cold-fusion researcher I would like to dissociate myself from those whose claims are not validated. Saying that something is a cold fusion device, without giving any evidence for it, makes no sense to me. What would be the best evidence? Working devices independently produced (as described by inventors) in at least two or three shops. Theoretically inclined people will certainly find ways to explain reproducibly performing gadgets. Emission of alpha particles, neutrons or gamma rays, from an apparently non-nuclear device, would also be a strong indication that something highly unusual is taking place. Words and promises alone, like "gold from carrots" or "cheap hydrogen" from water, are not sufficient. When will the iESiUSA devices be available on the marketplace? Why is there so much secrecy about them?

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233) Free energy

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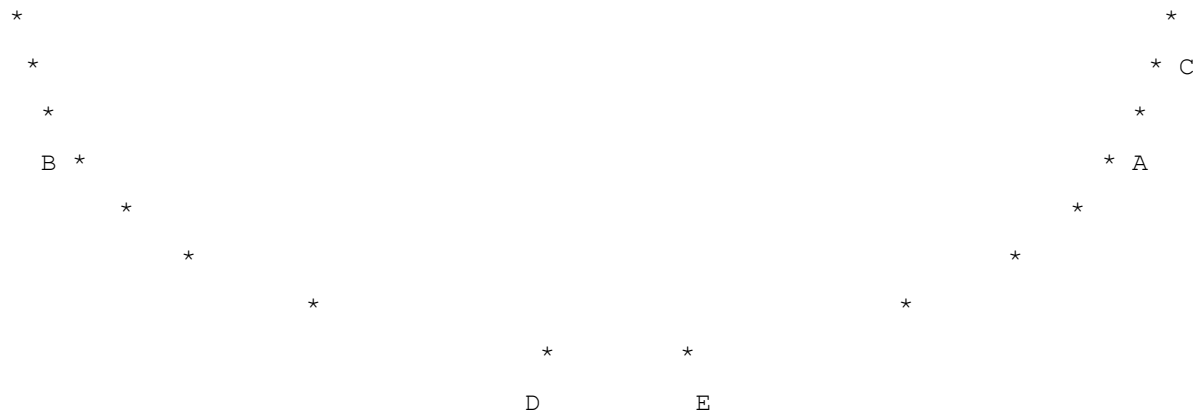
The term “free energy” is often used to describe gadgets presumably delivering more benefits than seems to be possible according to laws of physics described in our textbooks. Some refer to it as zero point energy, vacuum energy, black matter energy, etc. How can this concept be described to a layman? One way is to use a simple hypothetical illustration. Imagine a valley between two mountains. The slopes are covert by ice and an object is placed at a point A as illustrated below.



It starts sliding down gaining speed. Then it slides up loosing speed. Without friction it would slide to the point B (located at the same elevation as A), then back to the point A, then to B again, etc. etc. That is an ideal situation. In reality some friction is always present and consecutive round trips would become shorter. This happens because mechanical energy of the sliding object decreases and is converted into thermal energy (heat). The assumption is that no external energy is supplied. Under that assumption our object would never slide above the elevation AB.

But suppose that some energy is received during each round trip, for example, from a push (or blow of air) at the bottom of the valley. It is not difficult to imagine, for example, that pushes occur only when the object travels from D to E. In that case the object will be able to slide higher and higher during consecutive round trips. The same result would be observed if chemical energy, for example, in the form of fuel, were used to gain additional speed during each cycle. The mechanical energy gained can be used to perform some useful work, or to generate some heat. Free energy is similar to synchronized pushes and blows (in the above illustration) except that it cannot be identified with any known source or agent.

Now consider an inventor who invented a device that, according to a patent description, delivers excess energy at a steady rate. He is selling shares for the device and people buy them. Why do they buy shares without seeing a working device? Because they think that the claim is valid. The patent granted to the inventor is the basis of that belief. Naive investors assume that the patented device have been examined by experts; they do not know that working models are not required by patent laws. My impression is that even impossible-to-build devices can be patented, provided they are new. A patent is not a guarantee that a device will perform as described, it is only a document whose purpose is to protect the inventor, if the device turns out to be successful.

The law of conservation of energy, also known as the first law of thermodynamics, makes a clear distinction between what is and what is not possible. It tells us that some energy must be responsible for the excess heat or excess work. In the case of cold fusion that energy is believed to be thermonuclear. The best proof of this, from the scientific point of view, is accumulation of helium at the rate commensurable with excess heat. Unfortunately, that proof has not been accepted as valid by all scientists who evaluated cold cold fusion recently, on behalf of the DOE.

My impression is that the term "free energy" is used to provide a justification for perpetual motion devices of all kind. Entering this phrase into Google I had 962,000 hits. By adding "Antigravity" I got 17,800 hits. Adding "paranormal" I got 595 hits. In one of them I found this promotional description (by Tom Valone):

". . . Now at the start of a new millennium, new technologies are emerging that create "'free energy' (electricity without an electric bill), 'free propulsion' (drive for miles without paying for fuel) and 'anti-gravity' (leaving the surface of the road while you are driving). With diagrams, pictures, and video clips, this report details electrogravitics, inertial propulsion, free energy, magnetic motors, N-machines, the Searle effect, the Hutchinson effect, nuclear batteries and much more. Learn what these 21st century technologies will allow you to do. See demonstrations of anti-gravity and inertial propulsion that defy explanation. Become an expert on the new realities of free energy. K401 • VHS Video Tape • 90 min • $24.95 " As you can see, the term free energy a justification for all sort of science-fiction devices. The impression I got was that authors of most messages have no familiarity with conventional science.

Some authors, on the other hand, try to explain free energy in terms of physics that is too advanced for me. Instead of blaming authors for not explaining things I blame myself for not being educated enough. The net result is the same -- dissatisfaction. Those who really understand zero-point energy, for example, inventors, should be able to explain it, at least partially, to students and teachers familiar with basic physics. The "believe me; I know better" attitude is not helpful. Cold fusion is not the same thing as free energy. Cold fusion processes, when they becomes universally reproducible, will probably be recognized as nuclear reactions; free energy, on the other hand, is still a general mystery concept invented to turn impossible into passible.

What is being offered to the world by the iESiUSA company? Are they offering us cold fusion devices or a free energy devices? Unfortunately, as far as I know, no scientific explanations are available. Why is it so? Why those who were invited to see the demonstration do not share what they saw, and heard, with the rest of us? What do they think about emperor's cloths? Their silence is highly undesirable, as far as I am concerned. For the time being I have no other choice but to assume that iESiUSA devices, all three of them, belong to science fiction run on free energy.

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234) About levels of complexity

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I would like to describe a paper, "Physics and the real world," published by George Ellis in the most recent issue (July 2005, pages 49 to 54) of Physics Today. The blue text below consists of quotes from his article.

"Physicists reduce matter first to molecules, then to atoms, then to nuclei and electrons, and so on, the goal being always to reduce complexity to simplicity.....The extraordinary success of that approach is based on the concept of an isolated system. . . . The problem is that no real physical or biological system is truly isolated.....Consequently, reductionism tends to ignore the kinds of interactions that can trigger the emergence of order, patterns, or properties that do not preexist in the undergoing physical substratum..... Physics might provide the necessary conditions for such phenomena to exist, but not sufficient conditions for specifying the behaviors that emerge at those higher levels of complexity."

That is why biologists, for example, can do their job without knowing as much physics as physicists. Likewise, economists can do their job without knowing as much about human bodies as biologists or medical practitioners. And people can write programs in high-level languages without knowing what happens at the level bits and bytes. In principle, everything in the world depends on atoms but each discipline has its own laws (such as "supply and demand") and its own rules. One does not have to know chemistry to be a good cook. "Unique properties of organized matter arise from how the parts are arranged and interact, properties that cannot be fully explained by breaking that order down into its component parts. You can't even describe the higher levels in terms of of lower level language."

That is interesting. But let me emphasize that general rules of scientific methodology apply to all levels of complexity. The most basic rule: "theories guide but experiments decide" is valid not only in physics but also in chemistry, biology and economics. Experimental data gathered by qualified researchers should never be rejected on the basis of discrepancies with existing theories. Another basic rule is equally important. It states that experiments should be reproducible. Only reproducible experiments should guide us. That is why situation in cold fusion is so tragic. After sixteen years we still do not have a single everywhere-and-every-time reproducible nuclear signature associated with a chemical process. And we know what effect will it have on mainstream science when those of us who take some irreproducible results seriously are no longer alive. Discovering essential "hidden realities" behind such results should be the first priority of researchers. Promoting a practical application makes no sense unless the underlying phenomena are reproducible.

"Many lower-level states could correspond to the same higher level state. Higher level states are thus relatively insensitive to details of the lover-level states of a system. ..... When I move my arm, for instance, it moves because millions of electrons attract millions of protons in my muscles. ..... Laws of physics beget laws of chemistry, which beget laws of biochemistry, and so forth. But conversely, higher-level variables can control what happens at the lover levels. When I move my arm, for instance, it movers because I have decided to move it. My intentions thus instruct many millions of electrons and protons to behave in certain way." ..... Similarly, social constructions drive what happens in our everyday lives. Rules and regulations govern health care systems, housing policy, and how games such as football and chess are played. Money, another convention..... is vital for constructing bridges, jumbo jets, and most other manufactured objects in our world."

After these introductory comments Ellis focuses on the role of the feedback control systems and on the limits of

physics. In the section about limits of physics he criticizes those who believe that "predicting human intentionality is difficult only because we don't know enough about brains to make the calculations. Physics is all there is. Despite its appeal to some, that kind of claim is an unprovable philosophical supposition about the nature of causality; the claim is without predictive power -- that is, no observable consequences follow from it -- and without experimental support. . . . Predicting probable outcomes of the working of the brain would be possible only if we were to take into account the higher-level entities that shape its outcomes -- including abstractions such as the value of money, the rules of chess, local social customs, and socially accepted ethical values. These kinds of concepts influence what happens in the world but are not physical variables -- they all lie outside the conceptual domain of physics . . . Furthermore, you cannot understand or predict a mind's behavior without taking into account its interaction with other minds."

How can one disagree with such observations? Life is much more than just physics. "We should also recognize that the enterprise of science itself does not make sense if our minds cannot rationally choose between theories on the basis of available [experimental] data. A reasoning mind able to make rational choices is a prerequisite for the discipline of physics itself." Ellis' observations can be applied to cold fusion. Unlike hot fusion, cold fusion is not a simple collision between two isolated positive ions. It takes place in metallic crystals. The level of complexity of cold fusion might be responsible for some of its reported peculiarities. This is often emphasized by theoretically inclined cold fusion researchers. They say that what is nearly impossible in low pressure plasma become possible in crystals. I view this as the effect of complexity similar to those discussed by Ellis. Cold fusion, practically impossible in an isolated system of two particles, becomes observable in condensed matter.

The irreproducibility of experimental results, however, cannot be blamed on collective effects involving millions of interconnected ions and electrons. It can only be blamed on our ignorance. "The higher, many-body levels are more complex and less predictable than the lower levels; we have reliable phenomenological laws of physics and chemistry, for instance, but not at the level of psychology and sociology. [Social aspects of cold fusion, due to unfortunate 1989 events, constitute another level of complexity. One has to deal with it to make progress] "Higher-level variables are often aggregates of lower-level variables, and determined by them. But [they also] reveal important properties of the hierarchy that are otherwise hidden." Yes, something essential, as often emphasized by Edmund Storms, is still not under control of experimentalists. I hope he is right in predicting that the discovery of hidden factors, probably by material scientists, will eventually lead to desirable applications of cold fusion. Trying to promote practical applications, without understanding what is going on, is likely to become counterproductive.

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235) It was only one year ago

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Due to collaboration with Dennis Lett (and his team) I was highly excited one year ago. I was convinced that their results, reported by me at the 11'th cold fusion conference in Marseilles, were the beginning of something that should turn the controversial field into just another area of mainstream science research. My findings were described in units #182, #183 and #184; they will also be described in the ICCF11 proceedings. What I did not know one year ago was the footnote that had to be added to my report at the stage of its galley proofing. Here is the footnote:

On May 2005 Ludwik Kowalski sent an e-mail message to Dennis Letts. He wrote: "I am going to galley proof our ICCF11 presentation in Marseilles. This puts me in an awkward situation. If I were reporting on my own work I would add a short paragraphs, something like this: 'No additional experiments were conducted to confirm observations reported 6 months ago. The unexpected delay is due to . . .' But in this case I was only a messenger; you are real players. A reader is likely to be interested in the current status of our investigation. I think that it is not right to report positive results only and keep negative results hidden. Do you agree? . . ." The following reply was received several hours later.

"No additional experiments were conducted to confirm observations reported 6 months ago. The unexpected delay is due to the fact that experiments seldom work on a schedule. The MOAC had to be modified slightly to re-store design stability and precision. Also, we have not observed laser-triggered excess power since August 2003. Of course I agree [with your last statement] – since changing metals at the end of 2004, my success rate has been zero. This is compared to a success rate of 87% during the years of 2000–2004. Other than changing Palladium stock, I don't know what has caused the sudden loss of the laser effect. Experiments have been conducted in a high quality calorimeter (MOAC), in a moderate quality calorimeter (my Avanti) and on the open bench. The laser effect has not re-appeared under any of the above calorimetric conditions.

Experiments are being conducted now to re-establish the laser effect or to explain why it stopped working. You may use this information in any way you wish, including an addendum. With regard to reporting negative results, consider this: Cravens and Letts discovered the laser effect in September 2000 and reported the positive results publicly in August 2003. We spent 3 years testing the credibility of our result before reporting publicly. We anticipate behaving in a consistent manner now – we have negative results but we're not in a rush to report until we're sure that we have negative results and try to provide some reasons why the results are negative. I believe that reporting results formally by 2007 will be consistent with our previous work and should not be considered 'keeping negative results hidden'."

I agree that one should not jump to conclusions too rapidly, and that negative results should be investigated as thoroughly as positive results. And I admire his courage to continue researching. Let me share another piece of disappointing information. On June 20, 2005 a reader wrote to me: "As far as I know, the only Japanese CF laboratory still in operation is Mizuno's lab. ..... Iwamura is closed down because of the opposition and because the company is in a crisis. Spring8 and some others are supposedly working to verify Iwamura, but I do not think this involves experiments. The other researchers have all retired as far as I know." Another line of cold fusion research, supported by Toyota -- Fleischmann's laboratory in Southern France -- also ended suddenly two years ago, in spite of presumably positive results. This was said to due to death of the supporter.

Iwamura's findings (see unit #123) dominated last two cold fusion conferences; they were said to be confirmed in other places (see unit #105), but not independently. Why did two automobile industries stop productive scientific research? Probably because they are more interested in short-term benefits from research than in its long-term scientific significance. Let me also mention Oriani effects. I started investigating them after the 11th cold fusion conference, as reported in unit #188. This work was also described in units #189, 192 and 197. Unfortunately, what was highly reproducible is no longer reproducible, as reported to me by Oriani three months ago. Like Dennis Letts, Oriani is not giving up; he believes that the cause of irreproducibility can be found. Somebody hinted that fluctuating solar activity might be correlated with negative results of some cold fusion experiments on earth. That is an interesting idea but its validation would be even more difficult than validation of cold fusion claims.

I have no doubt that researchers with whom collaborated in the last year (Jones, Letts and Oriani) are component and honest. That is why I am not yet discouraged by negative results. I still want to find somebody who can help me to produce a student-oriented cold fusion demonstration on the basis of a scientific research claim. Please contact me if you have something feasible, and not very expensive, to suggest. Keep in mind that my background is nuclear physics, not chemistry and not calorimetry. But I would be willing to work on an excess heat demo, provided the rate of generation of excess heat, at least 10 W, is significantly higher than the rate at which energy is supplied to the unit. Aside of this pedagogical project I would like to have a chance to observe an interesting research work and write about it at this website. My email address is <kowalskil@mail.montclair.edu>.

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236) Promises promises

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The iESiUSA website has an item that I do not remember seeing several days ago. It is a one-year-old paper entitled " Alternative energies are looking good again." The author, Michael Kanellos, is a writer. Here is the first sentence: "Companies promoting solar power and other alternative-energy concepts are rapidly attracting venture funding, research grants and, just as important, the interest of many of the tech industry's deep thinkers and influential figures." This is followed by comments on the oil crisis, terrorism and "Enron-related" blackouts in California. Then I see a section about enormous profits of companies that benefited from investments in new technology fields.

Contrary to my expectation, the article turned out to be devoted to technologies of photovoltaic cells; I expected it to shed some light on the iESiUSA technology. That is why I am disappointed. When will we hear from people who witnessed the June, 2005 demonstration in Edmonton? According to the Internet rumors, Martin Fleischmann was only one of several qualified witnesses. When will the revolutionary devices be described on the company's website? When will company scientists share with us what they understand? The longer I wait the more pessimistic I become about the three iESiUSA promises. As far as In know, it was a fraudulent claim. The company no longer exists and millions of dollars were lost by naive investors. Here is a quote from "Understanding Phisics;" by Kowalski and Hellman (a textbok published in 1978, by Dickenson Publishing Company). "Getting more out of a physical system than is put into it has been a persistent dream of humankind. Those who have tried to do this honestly -- by inventing their own 'perpetual motion' machines -- have invariably failed. The only people who have come out ahead in his area are those who claimed success and then got others to pay to see it. That it seems, was not difficult to do. In 1812 Charles Redheffer traveled through Philadelphia and New York, charging a dollar admission (a dollar was a lot of money in these days) to see his perpetual motion machine made up of wheels, gears, and pulleys which kept moving continually with no apparent source of energy. He did very well until someone discovered a little man in a back room turning a crank.

Another method of making money is to 'invent' such a device and then get people to invest in it. So gullible are some people, and so anxious to get something for nothing, that they will put money into all kinds of money-making schemes." The iESi company was certainly not the first instrument to get money from greedy people; and most likely not the last one. Even people who studied science are not always immune to preposterous claims. They know that many ideas that turned out to be good were initially rejected as nonsensical. The germ theory of disease, for example, was ridiculed when it was first introduced by Pasteur. Today bacteriology is an essential part of medical science.

Or think about Wright brothers' idea of airplanes. It was ridiculed by those who believed that only balloons "lighter than air." could fly. But the brothers did not give up and their persistence paid off. How can one deny this today when heavier than air machines fly all over the world? Dismissing an idea which seems to be in conflict with what is already known may or may not be justified. That is why scientists are often reluctant to reject unreasonable claims. Sometimes it pays off and sometimes it leads to disappointments.

Inserted on 7/15/07
Scientific people behind the iESiUSA technology are A.I. Koldomasov (from Russia) and H. Yang (from Korea). A set of essays, including my own, about their work can be found at:
http://www.rexresearch.com/koldomsv/koldomsv.htm .
This reference was sent to me this morning, from Google.

This website contains other cold fusion items.
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237) A very important report

Ludwik Kowalski (7/10/05)
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I have several pieces of information connected with the story of current interest. THESE PIECES CONTAINED NAMES AND PERSONAL MESSAGES WHICH SHOULD NOT HAVE BEEN MADE PUBLIC. APOLOGIZG TO THOSE WHO WERE INVOLVED I AM REMOVING THIS INFORMATION NOW (7/14/05).

Added on 7/11/05:

I like the idea of making working iESiUSA devices available to independent investigators, as suggested in the report shown below. Peter is a leading theoretical cold fusion researcher and I think that his report is worth preserving; it will become an important part of history of science. The report is already a "very nearly public document." The blue text is his; black insertions, such as [*1] and [*2], are mine; they refer to comments that follow the report. As indicated above, Hagelstein's report was sent to me, and to other people, by Jed Rothwell.

"This letter is to provide a brief report on the iESi site visit that took place on February 13, 2005. This company has developed a new energy generation technology that is based on a highly unconventional approach, and I was requested to evaluate it to see whether I thought that it was suitable for investment purposes. As you know, I have been involved in research in the area of cold fusion for nearly 16 years, and was the conference chair of the Tenth International Cold Fusion Conference held near MIT in 2003. I was also involved in the recent DoE review of cold fusion that was held last year. This is relevant as the new technology developed at iESi appears to be related to, or make use of, cold fusion phenomena **[*1]** as the basis of its operation.

I note that a previous site visit was done, and that iESi provided a demonstration of their technology at that time. This earlier report provides a detailed description of physical parameters associated with the test. The numbers in that report are similar to the numbers presented to me during my visit, so I will not overly belabor points that were covered earlier **[*2]**. I was interested in understanding some of the issues about how the technology works, as well as understanding the level of expertise of the researchers involved. For example, measurements of current, voltage, temperatures, and flow rates are reasonably straightforward, and the effects under discussion are not small. It was possible to verify by touch the presence of a sizeable temperature increase, and one could see discharge phenomena consistent with the production of significant charge imbalances. Hence the basic claims of energy and charge creation correspond to massive effects that are readily observable independent of any of their diagnostics. **[*3]**

Real or Faked?

It would be reasonable to ask whether the results presented might have been faked in some way. The production of excess heat would require roughly a 4 kW heater somewhere in the flow stream, along with a power supply from somewhere to drive it. If someone wished to simulate a large heat effect by cheating in this way, it could have been done with the apparatus presented under the conditions of the test. Let me be clear that I do not believe it was, but this cannot be ruled out by any tests or observations that I made.

An electrical discharge was readily observable inside the cell. The breakdown strength of the cell and oil are likely in excess of 100 kV/cm, with breakdown clearly evident, and 2-3 cm distances involved. I would not be surprised if internal voltages on the order of 500 kV were present. Had one been determined to fake this, one could have put in a charge source somewhere in the flow stream, and then taken advantage of an internal Van de Graaf effect to make a discharge. Once again, I do not believe that this was done, but the possibility cannot be ruled out by my observations.

I note also that in the course of my review, essentially no scientific data was presented. In this I was disappointed. **[*4]** However, Professor Yang provided me freely with many helpful pieces of information that were helpful in learning about their work and observations.

Focus of the Review

It would have been a simple matter to determine whether their system operates as claimed if it worked according to well known operating principles that one could look up in textbooks. As the research effort involves cold fusion effects, there are no textbooks explaining basic operating principles. Nevertheless, there have accumulated a fair amount of understanding of cold fusion systems and how they work over the past 16 years, and my goal was to try to understand how this device might be operating based on previous knowledge of the field. **[*4]**

Hence, my approach to the review is as follows. Since some of the experiment is inaccessible the possibility of being tricked is ever-present (once again, I do not believe that iESi is engaged in tricking people. The ultimate defense against such things is for an independent lab to construct a version of the device themselves and see it work.), I wanted instead to understand what it is they did and what they saw. Instead of reporting on temperatures and voltages, which has been done previously, I was more interested in understanding what the device was doing and why, based on iESi's experience, it was doing it. I note that within the scientific community, scientists interacting with other colleagues on experiments usually learn about the experiments over time from discussions, from papers, from data, and from working with the experiment. Over the course of a one-day visit **[*5]** and review, it is not possible to come away with a complete understanding, but one can make every attempt to learn some of the basics of an experiment.

Essential Claims

To make life simple, I will boil things down to a small number of basic claims:

The iESi team claims that a very large amount of energy is being generated as its primary claim, and that energy gain on the order of a factor of five is observed at a maximum temperature on the order of, but less than 100 C. By itself, this claim would seem to be amazing, but it is actually neither unique nor overly interesting for applications. Within the area of cold fusion research, several groups have reported higher gains, or experiments where the gains could easily have been higher with minor design modifications. A heat boost of a factor of five might be interesting for commercial applications were electricity priced lower. **[*6].** Thermal to electric energy conversion below 100 C through conventional means is sufficiently inefficient that it would be difficult if not impossible to run the system with its own converted power done this way. Highly significant is the amount of power being generated, as there have not been previous reports of excess power generation at this magnitude. Even more highly significant is the power per volume ratio that one might associate with the working area of the cell, which is on the order of a few kW per cubic centimeter **[*7]**. similar to power generation densities obtained in cold fusion cells. I note that the iESi team claims that power gains of 20 or more can be produced with their technology.

Also claimed is a very high rate of reproducibility, approaching 100%. The system was demonstrated successfully three times during my stay in Edmonton. I have no reason to doubt that it is reproducible in their hands.

Perhaps the most significant claim is one of an ability to generate large amounts of voltage, current, and electrical power. It was claimed that the team had observed DC electrical power production at a level of 200 kW [4 Amps at 50 kV] with the device under consideration, with a much lower electrical input power. To support this claim, the system was run under conditions where rather memorable discharges were generated, and voltages between 10 and 40 kV were measured on an electrode that extended out from the cell. The discharges were seen to dig tracks through more than an inch of Plexiglas surrounding the active chamber of the cell, which is consistent with the presence of high voltage as mentioned above. I note that a device that can produce electrical power gain significantly greater than unity would be of large commercial value. The iESi team did not demonstrate such a conversion during my visit, but was building up toward such a demonstration for the near future. **[*8]**

Excess Energy Generation Issues

As discussed above, the excess energy generation claimed, and demonstrated, is on the order of a few kW with an energy gain of about 5 [thermal energy out over electrical energy in]. I asked Professor Yang how he thought that the system produced energy. His explanation involved ideas about proton-proton reactions, and that he was creating conditions similar to that in the outer part of the sun in the discharge areas inside the cell. I will not go into an explanation here as to why such thoughts are not going to be helpful (Professor Yang's area of expertise is in mechanical engineering) **[*9]**. Consequently, I came to the conclusion that while the research effort has discovered the effect, their understanding of what is happening is not very good. In essence, they have no relevant models for what is occurring within their cell **[*10].**

After reviewing their cell design, and after thinking about things, I have tentatively come to the conclusion that the basic operation of their cell is ultimately very similar (?) in many regards to that of a conventional cold fusion cell **[*11]**. The iESi design, viewed from this perspective, has present all of the elements that, according to my understanding of such devices, are required. There is fuel, there appears to be appropriate local matter conditions within the cell, and there is vibrational stimulation. Moreover, aspects of the operation of their cell appear to be consistent with such a picture. The cell appears to turn on and off in accordance with the presence of stimulation, as expected. A proprietary external stimulation increases the heat effect, also as expected. **[*12].** The cell seems to give a higher output at increased temperature, which is consistent with expectations associated with the availability of the fuel. Discussions about their experience with different local matter conditions appears to mesh with expectations. The power density associated with excess heat production appears to be very similar to those observed in a conventional cold fusion cell. The ``ash'' expected from this kind of experiment would be ^{3}He, and the iESi team is claiming to have observed ^{3}He from this kind of experiment in experiments elsewhere. **[*13].** My present view is that they probably are seeing a very large excess heat effect that works very similarly to other experiments in terms of mechanism, and that the design of the iESi cell in this regard is very good, allowing practical access of a high power operating regime which has not been reported previously.

Verification: Energy

One would always like certainty, especially in matters of science and financial investments. Thinking about things after the visit, I was wondering what kinds of things might be done in order to verify the energy claim. How can one be certain that it works as claimed? How could one prove that the device operates according to one principle or another? The normal way these things are done in more normal scientific circumstances is to do tests of one sort or another. For example, sending a test unit out to a trusted independent lab for an independent measurement of the energy gain would be a simple way to arrange for a confirmation. **[*14]**. A measurement of ^{3}He in quantitative measure to the energy produced would not only provide a confirmation of the underlying reaction pathway, but would provide an independent measurement of energy production. Simply measuring an anomalously large amount of ^{3}He would be provocative; as such an observation has so far eluded everyone that has tried within the cold fusion community (whereas quantitative ^{4}He has been measured in other experiments). There are a host of more subtle tests that can be done which could shed light on the underlying physical mechanisms. I would recommend that iESi seriously consider the possibility of an independent test in the relatively near future from a respected laboratory. Other tests and experiments should probably be part of a research effort devoted to understanding the device in the months and years to follow.

Charge Generation: Issues

The consequences of the appearance of electrical charge can be seen in the discharge effects associated with the experiment. I asked Professor Yang whether he understood how the charge was produced, and my understanding is that this has not yet been clarified in this experiment. Charge generation is known in association with cold fusion experiments. The effect appeared in a number of gas-loading experiments that I was involved in at MIT in the mid-1990s, and has been reported by some of my colleagues elsewhere **[*15]**.

There are several things that are interesting about the charge generation in the iESi experiment. The charges are probably generated with reasonably high associated energies. This I conclude from observations that Professor Yang reported in which voltages on the order of 1 MV were measured **[*16]**. In addition, the iESi team reports the observation

of a blue glow that can be seen in the oil and Plexiglas when charge is being generated **[*3]** when the lights are turned down. I was not able to see this effect during my visit, but the device was run during the daytime and there was no way to block incoming light from the outside. A nuclear radiation detector brought near the cell registered counts when the cell was running, but much less or none when the cell was off, consistent with the absence of accessible long-lived beta emitters in significant quantities **[*17]**. I asked whether they had attempted film exposure after their runs, and they had not done such a test. Apparently x-ray or gamma-ray measurements have been performed on the cells elsewhere, and a signal of some kind has been seen. One would expect a Bremsstrahlung signal in the presence of fast charged particles. It would be fun to see data of this kind.

In my view, this charge **emission [*18]** effect is probably real. I think that it is a cold fusion phenomenon that has been made to be dominant in this experiment. What is special about this device is that it has been pushed to maximize the charge emission effect **[*18]** so that it has become a primary reaction channel, such that it dominates the energy budget. This innovation allows for direct in-cell cold fusion to electrical energy conversion. **[*19].** This is really interesting, and probably makes obsolete other cold fusion electricity-generating technologies that require an external thermal to electrical energy conversion step.

Verification: Charge generation

The test that would have been nice to see would have been the application of the electrical output from the load to drive an electrical load. Such a demonstration is planned for the near future. Once again, the standard way that a confirmation is normally arranged for in scientific circles in such a situation is to arrange for an independent laboratory to run a test. There are a variety of tests that are possible that would help clarify the physical mechanisms involved, but these should more properly be part of a research effort devoted to understanding the device **[*14].**

Summary

The primary functionality claimed for the new iESi technology that I reviewed in Edmonton is excess heat generation and electrical energy generation. The level of power generation in the demonstration was several kilowatts for about 15 minutes, and an energy gain in excess of 5. **[*20]** . Higher power generation and energy gain was claimed in experiments carried out previously. The presence of high voltages was demonstrated, and the ability to drive electrical loads at high power levels (200 kW) with high energy gain was claimed in earlier experiments**. [*21]**. The usual route to confirm such claimed results would be to send a unit to an independent lab for verification, in this case, of the excess energy generation and electrical output.

The goal of my review was primarily to try to understand what they had done. Thermal and electrical energy generation in this device is due to new physical processes not found in textbooks. In my view the iESi team has managed to get the technology to work in spite of a lack of understanding as to why it works **[*22]** . The thermal energy generation appears to be closely related to energy generation effects reported in smaller scale cold fusion experiments, with a comparable excess power density, but with a much larger operating volume than done previously due to an innovative design. The electrical energy generation also appears to be closely related to charge generation effects **[*15]** observed in a smaller scale in cold fusion experiments. In the development of the device, the iESi team has maximized the efficiency of the charge emission, resulting in the direct generation of electricity without a need for external thermal to electric conversion." **[*23]**

My comments and questions:

[*1] Nuclear activity constitutes the essence of CF claims. I see not evidence of nuclear activity. Emission of X-rays, mentioned at the end is not necessarily connected with a nuclear process.
[*2] Who visited the company and when? Why is Peter assuming that the customer knows about that visit. Where can learn about these details?
[*3] I suppose that "charge creation" should be replaced by "separation of positive and negative charges," as in frictional experiments. How can a retired physics teacher miss an opportunity to make this correction?
[*4] I am also disappointed that "essentially no scientific data was presented." How can this be explained? The purpose

of the visit was to convince an MIT professor that technical claims are valid. Why was he treated as a layman by another scientist?
[*5] Who set up the time limit and why? If I had to travel from Boston to Edmonton I would request at least three full days.
[*6] Am I missing something or is there a typing error here? I am thinking about devices producing hot water in the areas where electricity is very expensive, for example, above the polar circle in Northern Canada or Russia.
[*7] Cubic cm of what?
[*8] Should we expect this to be done in several months or several years?
[*9] Yang introduced to investors (by the company CEO) as quantum physicist?
[*10] What part of the device is called cell?
[*11] Similarity is not a t obvious to me.
[*12] I suppose that, in this case, expectations are not based on a theory; they are based on what has been reported by other cold fusion researchers.
[*13] Was the rate of generation of ^{3}He consistent with the rate at which excess energy was produced?
[*14] Yes, this would be a normal way to convince, if one was certain that the claim is valid. Avoiding this way of convincing might indicate that they know that the claim is not valid. In my opinion a reliable operation of a device is more convincing that an attempt to explain its operation. Both components of convincing seem to be absent.
[*15] Is this a reference to generation of alpha particles reported by Jones, Lipson and Oriani? I would be happy place CR-39 detectors into their operating cell and look for presence of charged nuclear particles.
[*16] Voltages between what and what?
[*17] What kind of detector was it and what was detected?
[*18] Emission from where?
[*19] Yes, that would be highly desirable.
[*20] Was it excess heat or was it electric energy?
[*21] I am glad that Peter makes a clear distinction between what was actually demonstrated and what was claimed. I am also glad that possibility of fraud is not excluded by him.
[*22] Somebody would help them to understand science if a clear description of experimental facts were made available to other scientists and engineers.
[*23] Please, be aware that, according to Peter's message to me (see above), something has to be added at the beginning of the report. I guess it would be a reference to earlier inventions made by Russian researchers.

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239) What is cold fusion?

Ludwik Kowalski (12/7/05)
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1) Two physicists commented on unit #237; they asked for anonymity. Yes, I will respect such request, except in situations in which statements are "already very nearly public." One physicist made an interesting observation about a simple way of convincing others that a very large excess heat, in the iESiUSA device, is real. That device consists of circulating fluid (water, or oil). What can be more simple than to compare the temperature of a unit volume before the cell and after the cell? That would show how much heat is generated.

James Joule would insist on this kind of test. I am thinking about his experiments at Niagara Falls (comparing the temperature of water after the fall with the temperature before the fall). In the case of the iESiUSA device, the task should be much easier, because changes in temperature are said to be much larger than a fraction of a degree. Those who were invited to witness the device demonstration -- "Hagelstein, Fleischmann, McKurbe, etc." -- should have asked for a calorimetric test. If I had a large excess heat device to promote I would certainly make such test an essential part of a demonstration. The costs would be negligible in comparison with other investments. And I would not be secretive about the demonstration; I would not ask invited guests to sign an NDA (a legal non disclosure agreement).

2) The 11th issue of New Energy Times,

http://www.newenergytimes.com/news/NET11.htm

published yesterday, has a link to the result of a recent survey conducted by Peter Gluck and Steven Krivit:

http://newenergytimes.com/Reports/2005GluckKrivitSurvey.htm

It is an interested compilation of answers to the following four questions: (a) What Is cold fusion? (b) How Does It work? (c) What chances does it dave to be scaled up to a technology? and (d) What must we do in order to attain this? Here are some answers to the first question.

I wrote that cold fusion (CF) "is protoscience claiming that nuclear activities result from some chemical processes, such as those taking place in electrolysis, or when hydrogen ions diffuse through some solids." **Gimpel** wrote: "Electrolysis and other chemical processes can supply enormous pressure that compresses and crowds atoms. It can also supply very energetic particles (electrons, protons, and deuterons from heavy water) near the surface of the cathode of electrolysis for example. Some of these particles find there way toward the nucleus of neighboring atoms. If penetration is deep enough, there is remote possibility (but a reality how ever small) that the particle may find itself on the other side of the coulombic barrier."

And **Li** said that CF "is a kind of a nuclear reaction inside the condensed matter at low energy. It is a nuclear reaction without strong neutron or gamma radiation." **Biberian**, just next to me on the list, puts emphasis on the excess heat. "During loading of metals like palladium with hydrogen and-or deuterium, under some unclear conditions, more heat is produced than energy introduced in the system. This occurs in such a way that chemistry or other traditional

reactions can be excluded. The only alternative being nuclear reaction." According to **Spaandonk** CF is "any process generating heat beyond normal chemistry, &/or resulting in transmutation of elements, that happens at near room temperature." **Haffner,** on the other hand, mentions that heat may or may not be generated in "a loaded lattice environment."

These answers compliment each other. I am saying that CF is protoscience (because phenomena are not reproducible), Li puts emphasis on the unusual nature of reaction products while Biberian reminds us about unexplainable amounts of excess heat. **Dufour** writes that in cold fusion "the enthalpy is in the order of keV per atom, intermediate between chemical reactions (eV) and nuclear ones (MeV). This reaction has none of the characteristics of a nuclear reaction." **De Nino** writes: " In condensed matter the coulomb barrier is modeled and weakened by the dynamic of the plasmas of palladium d- electrons and deuterons. Such a mechanism envisages the existence of a threshold in the deuteron concentration in order to sufficiently modify the potential barriers.

The fusion between two deuterium nuclei creates an excited (hot) helium nucleus (2+2=4). In vacuum, this nucleus will break in pieces (^{3}He+n or T+p) that will carry away the excess energy, or, with a very lower probability, it can emit a very energetic photon (gamma ray of 24 MeV). In condensed matter, however, the electromagnetic fields acting in the medium can drown away the energy of the excited nucleus and convert it into thermal radiation. It is worthwhile to note that here we are talking about classic electromagnetic fields, not about quantum fields, that is, many low energy photons instead of one high energy photon, such as a gamma ray." **Storms** defines cold fusion as "the initiation of various nuclear reactions within special solid structures without the need to apply an amount of energy normally required to overcome the Coulomb barrier." **Lietz** thinks that biologically assisted nuclear reactions, reported by some researchers, are part of cold fusion.

T. Chub writes that cold fusion "combines mainly nuclear physics, condensed matter physics, and surface science, but also includes inputs from physical chemistry and thermodynamics, quantum mechanics, atom Bose condensate studies in optical lattices, etc." Likewise, **Beaty** thinks that "there may be more than one fundamental energy-producing phenomenon involved." **Kozima**, for example, thinks that cold fusion reactions are due to neutrons. **Miles** puts emphasis on "coherent field effects provided by the solid state" while **Gluck** thinks that the field "is immensely complex and difficult to comprehend. .....[It is] nanoscience at its best and worst." And **Krivit** starts answering the question by saying that "Cold fusion is clean nuclear energy."

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239) A dangerous cold fusion identity theft ?

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Tonight I sent the following e-mail message to about 100 members of the cold fusion community:

I was one of those to whom a message received yesterday was sent. A reader wrote: "I've tried to retrieve [without success] patents given on the iESiUSA web site:

<http://www.iesiusa.com/intellectual.html>

. . . Was anybody successful in retrieving any of the patents given on that website?" Replying to this another reader wrote: "iESi is in 'stealth mode' at the moment and not taking any inquiries . . . I have some, very limited, contact with them at the moment, though they are not giving me any answers at this time."

It is clear that researchers from the iESiUSA company are not using scientific methodology of convincing. Secrecy might be consistent with trade but not with true science, except in military research. To the best of my knowledge, cold fusion community is guided by rules of scientific methodology. That is why I think that we should disassociate ourselves from iESi claims. They say that their excess energy comes from cold fusion but refuse to justify this to our satisfaction. If we do not criticize them openly people will associate us with unpleasant consequences of making probably-not-valid commercial stock market claims. I feel that somebody is stilling our identity. How can we protect our reputation for honesty and hard work? Please write to me what you think about the iESiUSA claims. I would be happy to summarize your points of view on my CF website:

http://blake.montclair.edu/~kowalskil/cf/

And I would be glad to protect anonymity of those who ask for it. It is a serious matter; we are in a dangerous situation. Am I the only one who feels that way?
Ludwik Kowalski
P.S.
Items #216 and #226, #229, etc., on my website, are connected with iESiUSA claims, and with related patents.

The space below is reserved to replies. Anonymous senders will be identified by X. I will wait for at some messages before posting this unit. Then I will update it from time to time by appending additional messages, or comments about messages. Replies will be numbered to simplify referencing.

Appended on 7/13/05:

1) X wrote: Since there is no public discussion I don't feel it is necessary to start one. As far as I know they have approached the CF community (Hagelstein, Fleischmann, Krivitt). We could accept their way of operating, since this is indeed a very slippery terrain.

2) X wrote: It stinks; people promising great commercial benefits to investors should first demonstrate the setup.

3) X wrote: It seems to be a game with iESi shares at the highly speculative Canadian stock market.

4) Scott Little wrote: In my experience a significant fraction of the folks involved in CF research don't follow ordinary scientific methodologies. It does hurt the overall credibility of the field but that's pretty well shot anyway by now. I suggest you just distance yourself from those guys whenever the subject comes up. But don't go out of your way and make a big deal out of it. That would actually draw unwanted attention to them. If they've got something that works, we'll all find out about it soon enough. Meanwhile, if they don't want to play scientists, there's nothing we can do about it. (BTW, judging from their stupid website, I'd guess they're all smoke and mirrors).

Even we have not bothered to publish any papers describing our work. Basically we're looking for a successful commercial venture and, when the experiments don't show excess heat, we don't bother writing them up. However, I am planning to write a paper describing our new flow calorimeter and submit it to Review of Scientific Instruments. I hope such a publication would attract some people to submit their cells to me for evaluation. (1406 Old Wagon Road, Austin TX 78746).

5) Ludwik Kowalski wrote: Senders of messages will be identified, unless the first sentence is a request for anonymity.

Appended on 7/14/05:

6) John Coviello wrote: Given the history of alternative energy, which is littered with exaggerated claims and broken promises, iESiUSA's claims of a revolutionary energy technology based on a creative use of the cold fusion reaction must be greeted with deep skepticism by cold fusion proponents (as much as we'd all like to see it work). Cold Fusion proponents do not want to be associated with iESiUSA if they are ultimately proven to be a fraud; it would be a setback for the field of cold fusion. Therefore, until more concrete scientific evidence is provided to prove their technology works as described, iESiUSA's claims must be treated with a healthy skeptical approach.

It is troubling that iESiUSA is taking the path of secrecy and limited demonstrations. These are hallmarks of fraudulent technology schemes of past decades. However, since a revolutionary energy technology that would replace the current fossil fuel energy regime would be literally worth Trillions of dollars, it is somewhat understandable that a company, such as iESiUSA, would want to keep their technology secret until they are ready to go commercial with it. It is encouraging that they have invited credible scientists to review their technology, even if they aren't revealing all that they need to reveal at the moment to prove it beyond a doubt. I guess it is best to treat iESiUSA as just another alternative energy company making an unproven claim, until they give us reason to think otherwise. I would be in the camp that advises ignoring them until more substantial evidence or a commercial product is provided by the company. There is no harm in ignoring them, but there would be harm in endorsing them prematurely only to find out later that they are frauds.

7) Jed Rothwell wrote: Do not worry about iESi. Every year, dozens of strange secretive groups pop up claiming they are doing cold fusion. This has been happening since 1989, but it has not hurt the reputations of actual CF researchers. You are correct when you say iESi is doing business-style R&D, not academic science. The people at iESi would agree with you. I doubt that any substantial progress in CF can result from secretive research, because I think not enough is known about the basic physics yet, and we are still at the stage when fully open, traditional academic science is called for. Perhaps I am wrong about that . . . we shall see.

The iESi claims may actually be real. It is impossible to judge today, but we might find out 9 months from now, when the secrecy period for the latest patents expires. Some of the people who have visited iESi have been impressed, although not so impressed they are willing to say with certainty that the effect is real. Obviously that will have to wait for independent replications.

8) X wrote: I was not aware that cold fusion community had many recognized intruders who did not use scientific ways of validation. Are the episodes of criticizing them recorded somewhere? Books about cold fusion focus on its conflicts with mainstream science; they do not mention, as far as know, conflicts within the CF community. That is why one is inclined to think that the iESi episode had no precedents.

9) Dennis Cravens wrote: I know that others have said iESi has claimed cold fusion events, but I never saw anywhere where iESi have made such claims. (for example I have seen allegations by Clem that iESi claims cold fusion, but nothing from iESi about it). I do see where iESi has said Yang has worked on cold fusion systems. I do not see where they have claimed that they are making excess with a cold fusion system. Having been in this game a long time I know that there is often a big difference between what is said about you and what you say. I see all kinds of comment about iESi from you but I have yet to see any link to iESI where suchCF claims are made. Perhaps they have made such claims, it is just I see not primary evidence that they have. I see all kinds of patents about H generation and such but nothing to the CF point.

I will wait for evidence before I judge them. Right now it just looks like rumors and talk about them from others. I have asked both iESi and those who have visited the lab. I get no information. Perhaps they say nothing now because they are still working and verifying and do not what to misslead anyone. I think it is best to just wait for a report by them and ignore the rumors by others. If it is real it will eventually come to light. Even the so called visit reports are not posted by those that visited the site but by those that have not. If it is just talk by others who do not know what is going on - it will pass.

Appended on 7/16/05):

X wrote: I hope to see a proper scientific presentation of aspects of the work in an appropriate forum in the near future.

Ludwik Kowalski wrote: No scientific ways of validation of hard to accept claims has been offered. That is why I agree with those who suspect fraud. Another reason for being suspicious is based on putting myself in a position of an inventor who, without understanding basic science, was lucky to invent and build a highly desirable, and not very complicated, device.I am thinking about an invention similar to that of Volta or Roentgen. How would I proceed to benefit from the invention? Suppose that several million dollars have already been gathered from inventors; as posted on the iESi website. Being absolutely certain that my claim is valid I would sell one or two devices to a reputable firm; such firm would certainly not hesitate buying a device if a very strong “money back” guarantee was offered. The guarantee would state that “the device will work as described for at least one year.” Immediately after that I would apply for a patent. An application based on the working model would certainly be granted. Then I would start looking for new investors. Such strategy would work much better than promises on the iESi website.

I do understand why an inventor might want to keep things secret before being protected by a patent. If I were a “quantum scientist”, as Yang and Cho are said to be, I would probably start thinking about Nobel Prize, the ultimate scientific recognition. This would motivate me to write a paper, perhaps in cooperation with a more qualified scientist. The paper would be submitted to a reputable journal, such as Physical Review. Knowing about working models (and possibly given a chance to communicate with independent users, reviewers would be convinced that experimental data are not based on “wishful thinking” only. Perhaps such work is in progress. But seeing no evidence I prefer to remain skeptical. Yes, seeing “a proper scientific presentation” would be very helpful. But nothing would be more convincing than lasting financial success, based on well performing devices.

P.S.

iESiUSA is not the only company promising to deliver very attractive clean energy devices without telling us how they are supposed to work. For more information go to this website:

http://www.zpenergy.com/modules.php?name=News&file=article&sid=1357

You may also click on the ”published” link there to see how little the company has to say about the product that is said to be nearly ready for commercialization. I see no attempts to validate the too-good-to-believe claims. Being a physics teacher I am highly dissatisfied.

Appended on 5/31/2008
Here is the old item devivered today by Google:

Captured from www.iesiusa.com on 24 Oct., 2005

The intellectual capital within iESi consists of expert industry leaders in engineering, finance and marketing.

Patrick Cochrane – President & CEO

Patrick Cochrane has over 25 years of experience working in the Petro-chemical,Pulp & Paper and the energy technology industry. Prior to co-founding iESi, he founded and served as the chief operating officer of the engineering company Canadian Environmental Equipment & Engineering Technologies Ltd/Genoil Inc. in Edmonton, Alberta. In his career, Cochrane also held senior-level positions overseeing large industrial complexes & projects with water, petrochemical, oil and gas at Husky Oil, Daishowa Canada Co., Zeotec Ltd. and Fidinam Canada Ltd. Cochrane spent four years in the Canadian Armed Forces and attended Fairview College and the Northern Alberta Institute of Technology for formal training in industrial electrical technology and Power Engineering. He has authored numerous patents in the heavy oil industry, environmental and energy technologies. Cochrane was awarded a prestigious award form Cetac - West and nominated for the AsTech awards (Alberta Science and Technology Awards) in 1999 in Calgary Alberta Canada, recognizing his achievements in these areas.

Ronald Foster –Vice President of Business Development >BR> Ronald Foster is a co-founder of iESi and currently serves as the company's chairman of the board of directors. His primary responsibilities include finance, compliance and business development. In his career, Foster has held a number of senior-level executive positions with several publicly traded companies, including ValCom Inc., SBI Communications, Inc., EL-Phills Inc., Golden American Network and ROPA Communications Inc. He created and produced "Stock Outlook 87, 88, 89", a video presentation of public companies through Financial News Network (FNN), a national cable network. Foster has a tremendous amount of experience with mergers & acquisitions and the operations of publicly traded companies. He founded and served as president of a publicly traded marketing company that had annual revenues of $60 million. iESi's Scientific Advisory Board consists of world-class physicists and scientists in the area of quantum physics.

Hyunik Yang, Ph.D.

Much of iESi's proprietary intellectual property is based on the numerous patents in the field of quantum and hydrogen energy generation authored by Hyunkik Yang, Ph.D. Dr. Yang co-founded iESi and serves as the company's chief scientific officer. In his career, Dr. Yang also held positions as professor at Hanyang University and as senior research engineer at Hyundai Electronics. He has published dozens of international research papers on revolutionary quantum energy generation and anomaly effects of science. Dr. Yang has designed several new inventions in the field of quantum energy and cold fusion. Dr. Yang received his Engineering B.S. from Hanyang University in South Korea, and completed his Engineering M.S., Ph.D. and post-doctoral work at Columbia University in New York. He is a member of the American Society of Mechanical Engineers, Society of Automobile Engineers, Korean Society of Mechanical Engineers, Russian Academy of Natural Science, Korean CAD/CAM Society and the Korean Society of Machine Tool Engineers. Since 1987, Dr. Yang has been listed in the Who's Who in the World, and in the Who's Who in Science and Engineering since 1998. **Norm Arrison, Ph.D., P.Eng.**

Dr. Arrison is a senior scientist who worked for Atomic Energy of Canada Ltd. (AECL) at the Whiteshell Nuclear Research Establishment as a Research Officer and at Sheridan Park as a Design Engineer and rose to the position of being in charge of all safety systems for all CANDU Nuclear Reactors for AECL. He was first appointed to the Science Council of Canada by Prime Minister Trudeau and served from 1983-1986 at which time he was next appointed by Prime Minister Mulroney and served from 1986-1989. Dr. Arrison has authored several patents and been involved in numerous inventions, most of which have been related to the solving of environmental problems that make life better for people all around the globe. In his career, Dr. Arrison served as the director of research for Global Thermoelectric Power Systems, Ltd. and worked as an international consultant in the development of patented energy conversion processes that are utilized in Europe, Asia and Australia. He has consulted and helped develop specialized coatings products for specific industrial applications that are utilized extensively in North America and are gaining acceptance around the world. Dr. Arrison received his Ph.D. in Chemical Engineering from the University of Calgary. He is a member of the Association of Professional Engineers of the Province of Ontario and The Association of Professional Engineers Geologists and Geophysicists of Alberta.

Dumitru D. Fetcu, Ph.D.

Much of iESi's proprietary intellectual property is based on the 11 patents in the field of energy conservation, heat

transfer and waste heat recovery authored by Dumitru Fetcu, Ph.D. In his career, Dr. Fetcu has held positions as professor at the University Transilvania of Brasov in Romania and holds the chair of thermodynamics and fluid mechanics at that university's Department of Mechanical Engineering. Leveraging the new legislation that took place in Romania during the late 1980s, Dr. Fetcu founded the company Transterm. He is the author of two books and more than 60 publications in the field of heat exchange and heat recovery and has conducted over 45 scientific research contracts. Dr. Fetcu received his Ph.D. in thermodynamics from the University Transilvania of Brasov in Romania.

Board of Directors

Patrick Cochrane –President & CEO
Ronald Foster –Vice President of Business Development, Director, Board Chairman
Frederick S. Dornan –Director
Thomas Bugg –Director
Dr. Hyunik Yang –Director
- - - - -

iESi Ltd. - United Kingdom
Mark Boocock – Managing Director
Mr. Boocock has over 22 years experience delivering critical business systems and technical consultancy to blue chip clients across a range of sectors in the UK. After graduating in Computer Sciencess from the University of Glamorgan Mark commenced his career with BT. Here he developed a range of customer service and accounting systems used to run BT's then £20Billion a year business. In 1990 Mark joined Syntegra where he became involved in the development of Import Export systems for the British government. Prior to establishing iESi (UK) Mark held senior client & programme management positions with a global business service and consultancy provider. Clients have included major UK energy providers where Mark was closely involved with the de-regulation of the UK electricity market. Mark also co-founded a Facilities Management Company set up to deliver environmentally friendly cleaning and maintenance services to the UK commercial property sector and has interests in a recently established IT outsourcing business.

David Muxworthy – Marketing Director
After graduating from University in Bristol with a BA (Hons) Accountancy and Finance, Muxworthy a qualified Chartered Accountant started with Price Waterhouse with responsibility for the provision of audits to water companies; major banks and subsidiaries of major multinational groups such as Courtaulds, as well as many smaller and medium sized companies. David moved to the Welsh Development Agency in 1992 starting in Corporate Audit, undertaking independent reviews and appraisals on the operation and effectiveness of any aspects of policies, programs and procedures over the entire spectrum of the Agencies activities. In 1995 David became the agencies International Project Manager, Far East, with responsibility for attracting inward investment from the Far East (Hong Kong, Singapore and Malaysia primarily) to Wales. In 1998 David was promoted to Vice President, Business Development – Asia Pacific responsible for the smooth operation of the ASEAN office, based in Singapore, attracting inward investment from ASEAN and India, previously Hong Kong, China and Australia and growing indigenous Welsh business through joint ventures with ASEAN partners. David returned to Head Office in January 2003 to establish a division within the WDA responsible for attracting companies with converging technologies. This work took him into the Energy and ICT/Healthcare space and specifically the use of innovative technologies and services in delivering solutions for the energy and automotive industry. David is responsible for attracting a number of alternative energy companies to invest in Wales. More recently David has left the WDA to focus more closely on ICT/Healthcare and clean energy through his involvement with IESi (UK) Ltd.

Andrew Williams – Client Services Director
Mr. Williams is an experienced Chartered Engineer & Nuclear Engineering specialist, with a wide ranging but deliberate civilian & military career progression, built upon a strong foundation of engineering design & subsequently developed across a full spectrum of operational nuclear engineering & associated disciplines. His early experience as a Design Engineer with the United Kingdom Atomic Energy Authority, provided a springboard for his subsequent operational military career within the UK Royal Navy's (RN) nuclear submarine programme. His engineering credentials are also complimented by extensive Oceanography and Training Management skills developed during his

naval career. He has operational, engineering and safety management expertise in nuclear power electricity generation and marine propulsion technologies, coupled with an understanding of the impact of such technologies on the physical and in particular marine environment. Since leaving the forces in 2001 Andy has continued to develop his nuclear and maritime expertise, working in the nuclear safety, security, decommissioning and environmental management fields, again reflecting his interest in the sensible management of the Physical and in particular Marine Resource.

Hywel Rees – Sales & Partnerships Director
Mr. Hywel has over 25 years experience of servicing the needs of UK business and domestic consumers alike. After an early career in property sales and consultancy in and around South Wales, Hywel joined London based Merrill Lynch. At Merrill Lynch Hywel was responsible for looking after the relocation and housing needs of blue chip company directors throughout the United Kingdom. An opportunity arose back in Wales and over the next 10 years Hywel was able to grow his own corporate relocation company, SRMS Relocation. SRMS Relocation now provides services to blue chip companies and their staff throughout Wales and parts of England. Recently SRMS has taken over some of the provision of relocation work for the regional economic development authority whilst at the same time acquiring the largest competitor within Wales.

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240) Helium is produced in spent CF cathodes

Ludwik Kowalski (15/7/05)
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Searching for something else over the Internet I discovered an interesting paper published in 2002 by Robert Bass. That paper, a file in the pdf format, is automatically downloaded, as a pdf file, when one clicks on the first link that appears after the following URL is specified in the browser.

http://www.google.com/search?hl=en&q=donquixote%40radix.net&btnG=Google+Search

The title of the paper is "Five frozen needles CF protocol;" it appeared in Journal of New Energy, 2002. **6**(2): p. 30. The protocol is designed for a layman who, presented with data, would be convinced, with 95% certainty, than helium is produced in a cathode during the electrolysis (in a Fleischmann type cell). In the last paragraph Bass wrote: "I showed my one-page proof to an internationally reputable expert in Statistics and he agreed that it was correct and said that if the experiment turns out as I *predict* then he will give me a Certificate that the Confidence Level is that asserted and be willing to testify in Court as an Expert Witness that according to accepted principles of Statistics the preceding protocol is definitive as stated."

The emphasis in the paper is on statistics. But experimental details for the proposed experiments are worth paying attention to. The difficulties, as we know, are often in details. I see no evidence that the experiment has been actually performed by the author. Here is what would be required:

1) Having a strip-shaped cathode of cold-fusion-suitable Palladium. As emphasized by Bass, recognizing suitable cathode material is not a trivial task.

2) Loading the strip with deuterium up to a level of about one atom of ^{2}H per one atom of Pd. This task is also not trivial. Quoting E. Storms Bass lists four methods of measuring ^{2}He/Pd ratios.

3) Keeping the foil at the liquid air temperature till five main experiments can be performed (to minimize losses of ^{2}H due to diffusion).

4) The foil should be cut into five narrow strips, called needles. These needles should then be used in five nearly identical experiments (k=0, 1, 2, 3 and 4). The only difference between the experiments is duration of electrolysis, t_k=k*T. If T=2 hrs, for example, then the durations of consecutive experiments, would be 0, 2, 4, 6 and 8 hours. The difference of potential between the electrodes should be 17.7 volts (I do not know why) and the current should be kept constant. The k=0 experiment would be a control.

5) All five cathodes should be sent to a laboratory specializing in measurements of the amounts of helium, A_k. Using the results -- A_0, A_1, A_2, A_3, and A_4 -- one would calculate statistical parameters defined by Bass. Unfortunately, I am lost in the conclusive part of the paper. And I am puzzled by the fact that random errors, associated with individual A_k, are not being used in the analysis. Plotting A_k versus t_k, and showing the bars of A_k errors, is likely to be more convincing. I saw that kind of a plot in several cold fusion reports; the most recent was a paper describing a real experiment performed by Russ George.

The issue that Russ had to address was presence of helium in air. The challenge was to show that helium from air, or from something else, was not confused with helium from nuclear reactions in the cathode. Those who remember the 1989 DOE report know that absence of nuclear ashes (such as He), in the amounts commensurate with the amount of excess heat, was one of the decisive arguments against the claim of Pons and Fleischmann. The topic was discussed by pannelists of the second DOE report, several months ago. This time some of them were convinced that generation of He in cold fusion experiments is real.

Appended on 7/18/05:
The protocol described by Bass should not be viewed as applicable only to generation of helium. But the title of this unitis is "Production of helium." It is therefore appropriate to quote what Michael McKubbre wrote on that subject meny years ago. His paper on the subject is in the compilation of articles publishe in Infinite Energy. That compilation, a file named samplearticle.pdf, can be downloaded from <www.infinite-energy.com>. The method used by McKubre was not electrolytic; generation of ^{4}He was reported to occur in foils that were kept in a vessel filled with duterium (at high temperature and pressure). Here is the quotation:

". . . we submit a sample of gas from each of the cells for analysis to the mass spectrometer, a high-resolving, low-mass mass spectrometer. We're capable of separating the two masses of species, deuterium ^{2}D and ^{4}He.....On the monitor you see displayed, in fact, the mass spectrum from one of these samples. This is a relatively high level of helium-4. The peak here is the helium-4 peak, the deuterium peak would normally appear here; it's completely absent. This particular example shows 10.5 ppm helium. We compare the samples each day that we perform the analysis, we compare the samples of gas from the various active cells and blanks with a sample of room air, which we have measured many, many times and know to be 5.22 ppm. And we have some standards, which we typically use—that is, gas samples of helium in deuterium and argon which we submit to the mass spectrometer for the purpose of calibration.

The mass spectrometer simply sweeps a mass from low mass to high mass, in this case from 3.96 mass units to 4.06 mass units, which encompasses the range in which helium is to be found. In fact, this peak is helium, and deuterium D2 is to be found which will be found somewhere in this region. We use a liquid nitrogen cooled carbon trap in order to remove D2 so that we're able to see quite low levels of helium. We're accurate to probably 0.1 ppm helium and we can clearly resolve the presence of ^{2}D and ^{4}He. This spectrum is, in fact, the sum of a number of spectra that the mass spectrometer simply sweeps for the period of time that we pre-program, and this is the cumulative signal representing the integral of all helium which was present in the sample when we submitted it for analysis. To acquire this spectrum takes us about five minutes.

It's clearly not possible to produce helium from a chemical process. If we observe helium in our experiments it's either because it leaked in from the atmosphere—we can rule that out by the blanks that we do and the fact that the helium signal that we have seen is larger than the helium in the ambient. It's possible that the helium pre-existed in the sample and was simply released to the gas phase with long term exposure. We can rule that out largely because we've analyzed the catalyst that we're using and found that it contains no measurable levels of helium. The only possibility that remains, and remains to be checked, is that the helium is produced by a nuclear process. If the helium is produced by a nuclear process, then necessarily there will be an associated release of heat....."

Recognizing the major problem in the entire cold fusion field McKubre wrote: "One of the difficulties in the cold fusion field is the apparent apparent lack of replicability of experiments: many people performing the same experiment get apparently different results; different experiments performed in the same laboratory give apparently different results. So it's obvious that if you do the same thing you must always get the same result. What this is telling us is that there are some important parameters of our experiments that are not under our control. Some of them I know and understand, and still [we] can't control some of these parameters we don't know about yet. We just don't know what the process is that we are studying, so we don't know what parameters we need to control in order to yield a consistent result. An experiment which always gives the same result --can be performed in several different laboratories to yield the same result--would be very valuable to us, in part in helping to convince the remaining skeptical scientists in the world that there is a phenomenon to observe. But, in fact, in order to use the scientific method to observe scientific results, we have to be able to reproduce the results of our own experiments so that we can see what the effects of small

changes are on these experiments.

One experiment which has been reported to produce consistent and reproducible results is that of Professors Arata and Zhang, both of them are very, very experienced and very well recognized scientists in Japan. They performed a very careful experiment, reproduced it apparently a number of times in their own laboratory—producing both anomalous excess heat in fairly significant levels and helium-4 and, perhaps more interestingly, helium-3. The helium-3 to helium-4 ratio that they observed in their experiments is different from that in the air that we're breathing. Sufficiently different to indicate that there is clearly an anomalous nuclear reaction occurring. The difficulty with Arata and Zhang's experiment is that it's only been performed by them and only in their laboratory. What we're attempting to do here is to produce their same results with their apparatus and with their help. This is a collaborative effort between Arata and Zhang and the SRI group, to produce in *our* laboratory the same results as they have obtained repeatedly over the years, which would indicate that we have some degree of mastery over the experiment.

The experiment that we have running here, in fact, is relatively young; it hasn't been operating for very long. One of the difficulties with Arata's experiment is that it requires many, many months to produce a result, and quite literally we're not very experienced with Arata's methods, so we've had some difficulty getting his experiment set up and operational. Certainly, it's caused me to have an increased level of respect for Arata and Zhang's technical competence. They are very, very good scientists. Within a month or two, we hope to have reproduced their experiment faithfully and reproduced their result."

It should be clear that factors mentioned by McKubre are not at all addressed in the statistical protocol described by Bass. His protocol was not designed to deal with systematic errors; it was designed for experiments in which all uncertainties are due random fluctuations about mean values.

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241) Simulating experiments described in unit #240

Ludwik Kowalski (7/17/05)
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Important clarification have been made in inut #242. It was added after Dr. Bass commented on what is shown below. Please consider the next unit to be the continuation of this unit.

1) Introduction:
In unit #240 I wrote about a paper of Robert Bass. Since then I found that the peper can also be downloaded from the library at . Here I am summarizing observations based on computer simulations of experiments described by Bass. My cathodes, like his idealized cathodes are assumed to be made from a CF-suitable material. I was motivated by desire to verify Robert's statistical protocol. Instead of relying on his mathematical derivation I decided to use the brute-force approach -- the Monte Carlo method. Like Bass, I assumed that the only uncertainties result from random errors of measuring five quantities: A0, A1, A2, A3 and A4. These are amounts of helium produced in five experiments. The percentage errors. E0, E1, E2, E3 and E4, presumably independent of each other; are standard deviation for the corresponding value of Ak (k=0, 1, 2, 3, and 4). For example, if A2=1,000 units, and if the standard deviation associated with this result is 100 units then E2 =10%. I tested Robert's protocol and tentatively found it to be not reliable.

Let me begin by describing basic assumptions. Each atom of helium, produced from fusion of two atoms of deuterim is known to liberate 23.6 MeV of nuclear energy. Assuming that this energy appears in the form heat (and not photons of gamma rays) I know that excess heat generated at the rate of 10 watts ($6.24*10^{13}$ MeV per second) would produce $2.25*10^{17}$atoms of helium per hour. That quantity, helium generation rate, allowed me to calculate exact values of A_k for experiment durations of t_k= 0, 2, 4, 6 and 8 hours. The theoretical results were:

A0=0
A1=$0.45*10^{18}$ atoms
A2=$0.90*10^{18}$ atoms
A3=$1.35*10^{18}$ atoms
A4=$1.80*10^{18}$ atoms

The experimentally reported results are expected to fluctuate around these numbers and my Monte Carlo program confirms this, as expected. Sizes of fluctuations are controlled by changing desired values of E0, E1, E2, E3 and E4. The program can rapidly simulate thousands of measurements. Parameters of Robert's protocol are calculated, for each set of five measurements.

2) Bass' protocol:
Robert claims that, as stated in the abstract, his protocol was designed to "instantly reject any doubt that the electrical pulse was creating ^{4}He from some form of nuclear-chemistry process." A pulse is simply one case of the Fleischmann-like dc electrolysis going on for a fixed length of time. In unit #240 I wrote that those who actually performed experiments described in the paper used more convincing graphical displays. But that might be a matter of taste. The main issue has to with validity of the proposed numerical protocol. Would it be confirmed or contradicted by simulations of experimental results?

Robert's protocol consists of calculating three parameters (P1, P2 and P3) from available experimental data (from A0, A1, A2, A3 and A4). By definition:

P1 = 3*(A0 + 2 * A1 + A2 - A4) / 5

while

P2 = (A3 +2 * A4 -2 * A0 - A1)/10

Note that without random fluctuations of A1, A2 and A4, the value of P1 would be zero. This is simply a consequence that, under ideal conditions A2=2*A1 and A4=4*A1. My simulations, as expected, show that P1 approaches zero when bars of errors advance toward zero. This shows that P1 depends on precision of measurements of A1, A2 and A4. I find it strange that, according to the above definition, P1 is not affected by changes in A3. This conflicts with the idea that P1 is an indicator of the overall accuracy. Also note that P2, under ideal conditions, is positive. This too is a consequence of conditions imposed on durations of experiments. Once again I am puzzled by the absence of A2 in the definition of P2.

The third parameter is defined ambiguously; it involves k and Ak

$P3 = (1/3) * (Ak - P1 - k*P2)^2$

where k is the integer between 0 and 4. In what follows I will assume that (1/3) has to be multiplied by the sum of five quadratic terms, one for each k. In other words,

$P3 = (1/3) * \mathbf{SUM} (Ak - P1 - P2)^2$ (Bass confirmed this on 7/20/05)

Perhaps there was a typing error and the symbol SUM was omitted. Note that I am avoiding Greek symbols because they are sometimes changed into nonsensical characters over the Internet.

3) My results:
Simulated sets of five Monte Carlo experiments are always successful, by definition. Knowing this I calculated P1, P2 and P3 from the simulated values of A0, A1, A2, A3 and A4. The results from five simulated sets are tabulated below. The first four sets refer to situations in which E0=E1=E2=E3=E4; the valus of assumed percentages are shown in the first column. In the last set I used a mixture (mx) of arbitrarily assumed percentages: E0=3%, E1=12%, E2=7%, E3=30% and E4=4%.

%	A0	A1	A2	A3	A4	P1	P2	P3	sqr(P3)
1	0	4.59e17	9.06e19	1.36e18	1.81e18	-2.04e15	4.54e17	1.52e31	3.90e15
10	0	4.53e17	9.61e17	1.39e18	1.91e18	-2.04e16	4.88e17	1.52e33	3.90e16
30	0	4.61e17	1.09e18	1.49e18	2.31e18	-6.14e16	5.65e17	1.36e34	1.17e17
60	0	4.71e17	1.27e18	1.62e18	2.82e18	-1.22e17	6.81e17	5.48e34	2.34e17
mx	0	5.51e17	9.82e17	1.36e18	1.85e18	-6.14e15	4.61e17	1.36e32	1.17e16

I see that P1 is negative in all sets. This might be due to the suspected typing error (ommision of A2 from the definition of P1). Journal of New Energy, as far as I know, does not filter publications through the peer review process. The last column shows square roots of P3. In the published paper that parameter is labeled as sigma; it does not differ from P2 by as much as P3 does. According to Bass, the experiment is successful when P1<<P2 and when sigma<<P2/2. I cannot comment on these inequalities without knowing that published definitions do not suffer from typing errors. For the time being I simply observe that, in the 60% set, sigma is NOT MUCH smoller than P2. I am confident that the trivial Monte Carlo code (shown below) has no errors.

If I were asked to peer-review Robert's paper I would first insist on the removal of the ambiguity from the definition of P3. Then I would ask for the confirmation that printed definitions of parameters P1 and P2 contain no typing errors. If I were assured that definitions of P1, P2 and P3, in the Monte Carlo code below, are consistent with the protocol then I would suspect that the protocol is not reliable. My recommendation (to the editor of the journal) would be to

send the manuscript to a statistician able to examine the mathematical derivation of the protocol.

4) Final comments

a) My guess about the definition of P3, as used in the code, might not be valid. If this is true then I would like to receive the correct definition. It will not be difficult to modify my code and to generate a new table of simulated results. I would be glad to do this.

b) Note that Robert's protocol does not depend of explicit values of E0, E1, E2, E3 and E4. My results, on the other hand, show that relations between P1, P2 and P3 do depend on the imposed random fluctuations. This troubles me. But I cannot take anything seriously without being certain that definitions of P1, P2 and P3, in the Monte Carlo code, have no errors.

c) My Monte Carlo code, in True Basic, should be readable by a person familiar with programming in any procedural language, such as Fortran or Pascal. That is why I am listing the code below. Be aware that in True Basic exclamation signs identify comments. The tiny code was written to emphasize readability, not compactness. Comments concerning simulations would be appreciated.

```
Program robert_bass                                  ! Coded by Ludwik Kowalski (July of 2005)
! ************************
dim A(0 to 4)                                        ! place holders for A0, A1, A2, A3 and A4
dim D(0 to 4)                                        ! for durations of five experiments in hours
let E0=0.1                                           !  this means 10%
let E1=0.1
let E2=0.1
let E3=0.1
let E4=0.1
let wattage=10
let D(0)=0                                           ! five durations (in hours)
let D(1)=2
let D(2)=4
let D(3)=6
let D(4)=8
let  rate=2.25e16*wattage                            ! expected atoms/hour
for j=0 to 4
     let r=rnd                                       ! random number between -1 and +1
     let A(j)=(rate+r*E0*rate)*D(j)                  ! simulated A0, A1, A2, A3 and A4
next j
let  P1=(3*A(0)+2*A(1)+A(2)-A(4))/5                  ! P1 was dA in Robert's paper
let  P2=(A(3)+2*A(4)-2*A(0)-A(1))/10                 ! P2 was deltaA in his paper
         let P30 = (A(0) - P1 - 0*P2)^2
         let P31 = (A(1) - P1 - 1*P2)^2
         let P32 = (A(2) - P1 - 2*P2)^2              ! five components of the sum (to get P3)
         let p33 = (A(3) - P1 - 3*P2)^2
         let P34 = (A(4) - P1 - 4*P2)^2
let  P3=(P30+P31+P32+P33+P34)/3                      ! P3 was sigma squared in the paper

print  "A0=";A(0);"  A1=";A(1);"  A2=";A(2);"  A3=";A(3);"  A4=";A(4)
print "P1=";P1;"  P2=";P2;"  P3=";P3
```

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242) Monte Carlo validations of two protocols

Ludwik Kowalski (7/20/05)
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Be aware that his unit is the continuation of what was described in units #240 and #241.

1) After posting units #240 and #241, I wrote to Bass: "I want you to know that the two last units on the my CF website are based on your 2002 paper. Your comments will be appreciated, especially about how the three essential parameters should be defined in my description of the protocol. You will see that I suspect typing errors in the pdf file (your paper). In the immediate reply Robert wrote: " . . Moreover, it seems to me that you have entirely 'missed the point.'. . . In the version [of the presented to ICCF10, the formulae are concise and elegant. For all I know they may already have been published in some standard book, but the expert, (who is a 'household name' in the field of Statistics) who told me that my derivation was 'correct' did not give me any literature reference. Therefore I shall stick with my own 'de novo derivation from first principles' as explained sufficiently adequately in the Abstract to be verifiable by anyone who knows undergraduate calculus. To suggest that these rigorously derived formulae are 'unreliable' is ludicrous."

In a message received today Bob stated that all three parameters (P1, P2 and P3; see unit #241) were used correctly in my Monte Carlo code. My suspicion of typing errors, in P1 and P2, was not justified; my assumption that the summation symbol was omitted turned out to be correct. I will return to the issue of reliability later in this message. Before addressing that issue let me show another quote, also from the immediate Bob's reply.

"Ludwik, Thanks for pointing out what you have posted in your items #240 & #241 regarding my presentation to ICCF10 in August 2003. You once told me that you can post documents in MSWord or ".doc" format; therefore I would much prefer that you post the attached ICCF10 Abstract, which is a great improvement by Mike McKubre over my original 2001-published version (in that numerous constraints have been relaxed without detriment to the main idea). .....Also I would appreciate it if you would refer to this protocol as the 'Bockris/Bass/McKubre Protocol' or the 'Bockris-Bass-McKubre Protocol' because the main idea is something I learned from publications of John Bockris. Moreover, it seems to me that you have entirely 'missed the point.' ....."

2) Following the suggestion I am going to display the description of the BBM protocol in the appendix, at the end of this unit. You will see that it is quite different from the first protocol (described in units #240 and #241 and downloadable from the library at <www.lenr-canr.org>). For that reason I prefer to address the issue of validity of each protocol separately. I will try to do this a little later. Let me point out that the adjective actually used was "tentatively unreliable;" not simply "unreliable." Instead of trying to validate the protocol mathematically I decided to explore it numerically. What is wrong with this? A Monte Carlo simulation, if done correctly, is also a powerful tool of validation. Here is an illustration. Suppose a claim is made that by following an X protocol a person would be a casino winner, in a long run. To check this protocol one may decide to play many games and see what happens at the end. But that might be very expensive. Instead of following this approach one can play virtual computer games and see what happens at the end. Presence of a hidden error in the derivation of the protocol could be revealed by playing virtual games. But a reliable Monte Carlo code, like a reliable derivation, must be based on realistic assumptions.

The main issue, in this particular situation, has to do with my code, not with the idea of using the Monte Method of

validation. What is wrong with my code? What am I missing? I am going to discuss this privately with Robert. Expect to see my conclusion in several days...

3) Appended on 7/22/05:
Messages from Bass are interesting and useful. Our discussion is in progress; I will probably write my conclusions it in two or three days. Meanwhile let me make another general observation. According to what Robert wrote (see above), a layman is expected to "reject any doubt." Any doubt about what? That person, I suppose, knows the meaning of the term proportional; s/he should have no doubt that the term is used correctly when doubling, and quadrupling the duration also doubles and quadruples the amount of helium produced. But here are two questions that a critical thinker might really have:

a) Why should the stated values of A0, A1, A2, A3 ad A4 be taken for granted?
b) How can I be sure (after taking stated facts for granted) that helium was produced in nuclear reactions taking place in cathodes?

The purpose of statistical protocols, as far as I know, is not to answer that kind of questions. Such protocols are designed to deal with random experimental errors. For example, is a relation inferred from experimental data real or apparent (caused by randomness)? Robert's protocol, in step 9, mentions the confidence level of 95%. What does the concept "level of confidence" mean? Statisticians deal with samples but their pronouncements refer to populations from which samples are taken. Levels of confidence tell us how reliable such pronouncements are. Samples of larger sizes usually correspond to more reliable inferences than samples of smaller sizes.

The important point is that statistical protocols are used to validate statements about populations, not about samples. In the situation described by Robert samples (experimental values of A0, A1, A2, A3 and A4) are taken for granted. His protocol can then be used to validate the claim of proportionality. Is the amount of helium really proportional to the duration of electrolysis (under specified conditions) or is the inferred proportionality apparent? After all, other kinds of relations, such as quadratic, cubic or exponential, are also possible. In that context a statistical protocol could be useful. But questions (a) and (b) above are not statistical. I do not think that a statistical protocol would help me to answer them. To decide about (a) I would ask about work of other scientists; do they also observe similar facts? To decide about (b) I would ask about other indicators of nuclear processes, for example, about excess heat, or about tritium. Experimental data are usually easier to accept when they are consistent with confirmed theories. But, according to scientific methodology, theories should be justified by experimental data, not the other way around.

4) Appended on 7/23/05:
A newspaper may refer to a confidence level while describing a gallup poll. Suppose that a random sample consists of 15,300 questioners collected from a very large population of potential voters. After using a protocol a statistician makes a prediction (an inference based on the sample) that the election will be won by X. The level of confidence, such as 95%, refers to a particular prediction about the entire population; it does not refer to the sample.

Suppose that 8307 of questioned voters said that they will vote for X. The statistician would probably assume that only a negligible error, if any, was made in extracting this number from the pile of returned 15,300 questioners. Numbers, like 15,300 and 8,307, are assumed to be exact. But the inference made from the sample might not be very reliable. In that respect the situation described by Robert is different. Each of the five numbers (five laboratory measurements: A0, A1, A2, A3 and A4) is always associated with random errors, such as 5% or 25%. The bar of error associated with a single laboratory measurement depends on the instrument used; it can be determined by a technician after performing measurements on many samples containing the same amount of helium. Systematic errors can be minimized, if not totally eliminated, through frequent calibrations of laboratory instruments. But random errors are always associated with results of single measurements. That seems to an important difference between samples of physicists and samples of sociologists.

Suppose the goal is to decide whether or not the true relation between the amounts of helium (A_k) and durations of electrolysis (t_k) is linear. That is an inference and the concept of level of confidence is applicable. Results of five laboratory measurements of A_k, when plotted against durations, usually do not fall on a straight line. That is why bars

of errors, associated with numbers, become important. The initial hypothesis is that the relation is linear. But then it is either accepted or rejected, acccording to known error bars. It is impossible to be 100% certain that the true relation is linear (or quadratic, cubic, etc.) when error bars are very large. Yes, I know that most readers are familiar with all this.

Before finishing these observations let me focus on another well known fact. Physicists make inferences about laws of nature. Such laws are either deterministic or probabilistic. The field of cold fusion has no laws; only working hypothesis, based on irreproducible data. Are these hypotheses deterministic or probabilistic? How can an error bar be associated with the result of a single irreproducible measurement? I do not know how to answer that question. But I can think about a similar situation in sociology. Suppose a survey is conducted to predict the result of an election. A social scientist uses a protocol and makes a statement that 57% (plus or minus 5%) people support X. The level of confidence of that inference, is said to be 95%. Then a scandalous revelation about X appears in the news and less than 1% of people actually vote for X on the election day.

Does this mean that the protocol was faulty? Not at all. The protocol is based on the assumption that the conditions influencing people to vote, one way or another, do not change significantly. Behavior of social groups becomes unpredictable when such conditions change frequently. Essential conditions on which successful observations of cold fusion phenomena, such as generation of helium, depend have not yet been recognized. Presumably these conditions change frequently without our ability to control them. That is how irreproducibility is usually interpreted by cold fusion researchers. In my opinion inferences (based on logically derived protocols, or on Mont Carlo codes, are useless when one is dealing with irreproducible data. Identifying essential conditions, and finding ways of controlling them, should be the number one priority of cold fusion researchers. Everything else, including attempts to commercialize "magic devices," should wait till experimental data become reproducible. Yes, I know that everybody knows this.

5) Appended on 7/24:

I have no doubt that both Robert's protocols were written under the assumption that experimental data are reproducible. That would certainly apply to generation of oxygen (rather than helium) during ordinary electrolysis of water. In one of my messages about the first protocol I wrote that the "much larger than" seems to be too vague to be associated with the single level of confidence. The reply is worth quoting.

Ludwik, Thanks for pointing out that there is some vagueness in the original 2002 Bockris-Bass-McKubre (BBM) Protocol <http://www.lenr-canr.org/acrobat/BassRWfivefrozen.pdf> as published in JNE by me alone, before Mike McKubre suggested the improvements in generality leading to our ICCF10 version [attached], which would benefit from clarification. I was tacitly assuming that any randomness in the errors is a Gaussian stochastic process. Also I should not have assumed that dA > 0 so I should have written |dA| instead of dA in the first test. In that case, if one accepts the approximate sample value of sigma as an "estimator" for the Standard Deviation sigma, then One Sigma gives a 68% Confidence Limit and Two Sigma gives a 95% Confidence Limit, and Three Sigma gives a 99.7% Confidence Limit, etc.So, my first test

```
|dA| << delta-A
```

should be understood as a preliminary test to ensure that sigma has a good chance of being a fair estimator, i.e. the "bias" dA should be negligible in comparison to the real quantity of interest, namely delta-A. Therefore by " << " I mean "an order of magnitude", i.e. I would HOPE that

```
        2*sigma  <  delta-A
```

and in order to claim that the confidence is much "greater" than 95% [as I said] one would HOPE that

```
        2*sigma  <<  delta-A  in the sense that 3.sigma/(delta-A)  <  1,  or
        sigma/delta-A   <  1/3  =  0.33
```

In retrospect, my 2002 paper would be more clear [since sigma & dA & delta-A all have the same dimension {in the original case, energy}] if I had formulated the conclusions as:

```
Test 1:
        (|dA|/delta-A) < 0.10   ==>   estimator of sigma is OK
Test 2:
        (sigma/delta-A)  <  1.00   [68.3% Confidence]
        (sigma/delta-A)  <  0.50   [95.4% Confidence]
        (sigma/delta-A)  <  0.33   [99.7% Confidence]
```

If anybody ever performs the BBM Protocol, then I would take that as an opportunity to similarly clarify the test at the bottom of the Bass-McKubre Abstract for ICCF10 [attached]; the reason for its lamentable brevity is that I ran out of space and didn't wan't to go onto a second page. ..... but my REAL intent is that any layman (like a Senator who had studied humanities & law) who can merely count on his or her fingers will need only to see a ROUGHLY linear fit to the data and instantly conclude that "this is not an accidental correlation; this is a physical Cause & Effect which should be further researched because of its self-evident immense potential benefit to humanity!" Thanks again for your interest, Bob

a) So much about the first protocol; I would be happy to check the validity of the BBM protocol (with another Monte Carlo code) after learning more about how to turn the inequality shown in the last line of the appendix into confidence levels of specific inferences.

b) Personally I would not rely on a protocol to convince a layman that a relation is linear; I would simply plot A,sub>k versus t_k. But that is only a matter of pedagogy and taste.

c) A relation, by the way, does not have to be linear; a positively exponential relation, for example, would likely to be more desirable, from practical point of view.

d) As you probably know, from the 2004 document at:

http://powermarketers.netcontentinc.net/newsreader.asp?ppa=8knpq_ZknqphiqYUok%22EN%26bfej%5B!

a Canadian company, iESi, made a difficult-to-accept claim: "The new clean energy plant will enable Norwood Foundry to generate six times (12 MW) more electricity than it consumes (2 MW) at its foundry located in Nisku, Alberta, Canada." Please write to me what you know about the present status of that plant, and about the origin of excess energy. I would be happy to post a unit containing what you write, unless I see a clear indication that the message is confidential.

APPENDIX:

Generalized Cold Fusion Demonstration Protocol

Robert W. Bass, Michael C. H. McKubre

==========

Greek mathematical symbols were changed (by Ludwik Kowalski) when the good-looking MSWORD document was turned into the web page. This was necessary to compensate for his limited familiarity with html tags. Saving the original document as a webpage produced a monster that was hard to edit. That monster displayed different symbols (in the same equations) when viewed in different browsers.

Lower case sigma was replaced by **SIG**
Capital sigma (summation) was replaced by **SUM**
Lower case delta was replaced by **DEL** (also note that **DEL**E stands for deltaE)

Lower case alpha was replaced by **ALP**
Lower case beta was replaced by **BET**
Lower case gamma was replaced by **GAM**

Multiplications are indicated by asterisks. In the inequality (LEFT << RIGHT), in the last line, the LEFT is simply either **SIG** or dE, depending which is larger. That what the max is for. The RIGHT, in the same inequality, is the product of **DEL**E by the smallest value in the {Ck} set. That what the min is for.
==========

This is a generalization of the protocol proposed by Bass [1], which is more realistically flexible in several respects. An arbitrary number N <= 3 of similarly prepared samples is allowed, and neither the voltage nor the current is required to be constant. However, the previous protocol may be recovered as a special case when N = 5.

Let N >= 3 denote the number of similarly prepared samples. Let the suffix k denote any particular sample, k = 0, 1, 2, ... , (N -1), where the suffix k = 0 is reserved for the case of a control blank. Let { Ck }, k = 0, 1, 2, . . . , (N -1), denote the hypothesized causal inputs, where, as in Bass & Gleeson [2], each Ck may be e.g. the result of continuous monitoring of the input electrical power [product of instantaneous voltage & current] and its numerical integration over the complete duration of the preparation of the kth sample to give the total amount Ck (0 of electrical work done on the sample, or the total energy input. By definition, C0 = 0. Similarly, let {Ek }, k = 0, 1, 2,. . ., (N -1), denote the measured effect outputs. Three possibilities for the output effects { Ek }are:

(1) Ek = amount of excess enthalpy;
(2) Ek = amount of Helium-4;
(3) Ek = amount of Helium-3.

Now define the estimator of variance **SIG**^{2} by the sum over all k of

SIG^2 = [1/(N-2)] * ({ (Ek - dE - Ck *(E)2 })

where dE denotes mean effect-bias error, and where (E denotes mean effect-increment factor. Next, again summing over all k, define

ALP = **SUM** { Ek },

BET = **SUM** { (Ek *Ck.) },

GAM = **SUM** { Ck },

DEL = **SUM** { (Ck)^2 },

and verify by setting to zero the gradient of **SIG** with respect to the vector [dE, **DEL**E]^T that a necessary & sufficient condition for sample standard deviation **SIG** to be minimized is that

dE = (**DEL***AL - **GAM***BET) / DENOM

DELE = (N***BET** - **GAM***ALP) / DENOM

where DENOM stands for the N***DEL** - **GAM**^{2}

The confidence in cause/effect correlation is great if

max(**SIG**, |dE|) << |**DEL**E|*min {Ck}.

Dr. Robert W. Bass, BAE SYSTEMS, 44414 Pecan Court, California, MD 20619
robert.w.bass@baesystems.com

Ergerences:

[1] Robert W. Bass, "5 Frozen Needles CF Protocol," Journal of New Energy, vol. 6, no. 2 (Fall, 2001), pp. 30-32. Also available online at: http://www.lenr-canr.org/acrobat/BassRWfivefrozen.pdf
[2] Robert W. Bass and Wm. Stan Gleeson, "Theoretical and Experimental Results Regarding LENR/CF," Trans. of the American Nuclear Society, vol. 83 (Winter, 2000), pp. 355-56; and: http://www.padrak.com/ine/BASS_7.html

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243) Another rejection by Physics Today

Ludwik Kowalski (7/31/05)
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Physics Today rejected my "Open letter to the DOE, and its team of 18 scientists." No explanation of any kind was given. That is not appropriate. I have been a member of APS for many decades and I find the absence of the explanation not appropriate. What did the reviewers say? Why did they decide not to publish my letter? They received my piece on February 7, 2005. Why did it take so long to decide? Here is the emailed letter of rejection (receive on 7/15/05): "Dear Dr. Kowalski, We have completed our review of your letter dated 7 February 2005, which you titled "Open letter to the DOE, and its team of 18 scientists." We have decided not to publish it. I apologize for the length of time it has taken to completed this review and get notification to you. Thank you for writing and for your interest in Physics Today. Sincerely, Marty Hanna Letters Editor Physics Today."

And here is my immediate reply: "Dear Dr. Hanna: I suspected that this would happen, considering the delay. Last time you mentioned that many people commented on the second DOE review. Are some letters to the editor, on that subject, going to be published? If so then when? Strange things seem to be happening in the cold fusion field; I continue observing them. After the retirement it became my full time occupation. I am considering going to the 12th international cold fusion conference near Tokyo. Too bad that my chances of sharing what I know and think can not be shared in Physics Today. I guess it has to do with factors that neither you nor I can control. Cold fusion is going to be remembered as a strange episode in the history of science. Best regards, Ludwik Kowalski" Will this letter, e-mailed more that two weeks ago, be answered?

My two questions were based on the reply I received on April 12, 2005, after asking about the status of my letter to the editor. At that time Dr. Hanna wrote: "Your letter, along with approximately 60 others that I currently have in house, is still in review. As soon as our review is completed, I will notify you of the outcome. Your continued patience is appreciated." I was happy that sixty readers of Physics Today had commented on the DOE report and expect to see some of them in print. I suppose that most authors were APS members with high credentials. Why did editors decide to block timely contributions? Those interested in other records of rejection should look at units # 154. I think that rejections of CF papers are worth recording, for benefits of future generations. Cold fusion will be viewed as an interesting event in the history of science, regardless of verdicts about validity of numerous CF claims.

My Open Letter to the DOE was posted in unit #196. In unit #211 I commented on what Charles Beaudette wrote about that review. His paper can now be downloaded from the Library at <http://www.lenr-canr.org>: I think it is worth reading and thinking about. Was Beaudette one of those whose letters to the editor were rejected by Physics Today? I do not know.

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244) Coulomb barriers etc.

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A discussion list, CANA, has been created for members of ISCMNS (International Society of Condensed Matter Nuclear Science) four days ago. The purpose is to discuss scientific and technical aspects of cold fusion. Several interesting messages have already been posted. What follows is my own message. I hope that other nuclear physicists will comment on it. Will this generate some food for thoughts? I hope so. And I will write about this here, accordingly.

I read the paper recommended by Dennis with great interest. The phrase -- "the effective target area" -- made me think about the coulomb barrier. Why about the coulomb barrier when the bombarding particles are neutral? I will explain this shortly. But before going ahead let us look at something else. The 1 / v law, mentioned by the authors, implies that the apparent size of a nucleus depends on the projectiles with which a nucleus is bombarded. This fact has been known since the beginning of nuclear era. The unit of cross section "barn" was introduced to recognize unexpectedly large cross sections. In the first nuclear reactor (Fermi's pile, 1942) blocks of pure graphite were used to slow down neutrons to benefit from large cross sections at small v. Yes, most CANA members know this; but being a retired teacher I can not miss an opportunity for lecturing.

If the term "effective target area" is accepted then the term "effective coulomb barrier" should also be accepted. I am saying this because the height of the coulomb barrier and the size of a nucleus are intimately related. To justify this one must go back to the well known definition of the coulomb barrier, CB. Coulomb barrier is the value of potential energy (of two interacting positive particles) at a certain distance between their centers. The potential energy, V, consists of the sum of two terms, V1 and V2. The positive term, V1, called coulomb potential, is due to electric repulsion, the negative term V2, called nuclear potential, is due to attractive nuclear forces. The dependence of V1 on d (distance between the centers of two particles) is of the 1/d type, according to Coulomb's law. The dependence of V2 on d is usually much more complicated. In textbooks that dependence is often represented by deep potential wells.

Let me take a crude rectangular well (rather than a more realistic well introduced by Yukawa, by Saxon and Wood, etc.). In that model V2=0 everywhere outside a volume of radius R. At d<R the absolute value of V2 suddenly becomes much larger than the absolute value of V1. In other words, CB is the value of V1 when D=R. For many of you this hint is sufficient to see why CB would go down rapidly if R (the effective radius) were allowed to become larger. But let me illustrate this numerically for a two body system -- a Gd nucleus and an alpha particle. For a gadolinium R is close to 7.5 F. The potential energy of the system, at d>R, can be calculated on the basis of Coulomb's law (where k stands the familiar 1/4*Pi*epsilon-zero):

$$V = V1 = k*Z1*Z2*e^2 / d = 9*10^9 *2*64*2.56*10^{-28} / d$$

where d is in meters and V is in joules. In practical units (F and MeV) one has:

$$V \text{ (in MeV)} = 1.44*Z1*Z2 / d \text{ (in F)}$$

Replacing d by R=7.5 F, and knowing that Z1*Z2 is 128, one finds that CB = 24.5 MeV. A more realistic potential would give a lower CB, perhaps slightly below 20 MeV, but that is not important in the context of this message.

What would happen to CB if R were doubled? In that case the coulomb barrier would be reduced by the factor of two. The paper mentioned by Dennis refers to much larger changes in the "effective R" (several orders of magnitude). That, according to the above speculation, would nearly eliminate the coulomb barrier. Yes, I know that the speculation is silly; the value of R (beyond the region occupied by neutrons and protons) is determined by the range of nuclear forces. The large effective R, as far as I know, does not correspond to unusually large range of strong nuclear forces. Cross sections of nuclear reactions induced by charged particles, even for gadolinium, are never as large as for reactions induced by slow neutrons.

So what is the point of all this? In my mind this speculation is connected with the 1/ v law. That law describes cross sections induced by slow neutrons in the non-resonance regions of v. The common explanation -- "effects of interactions become stronger at small v because times of interactions become longer" -- does not satisfy me. Why not? Because i know that alpha particles approaching a nucleus, in central collisions, also have very small v. In fact, v becomes zero when the distance of minimum approach is reached (alpha particle starts bouncing back). That kind of slowing down does not result in high probabilities of fusion. Why do slower neutrons fuse much more frequently than faster neutrons? Why slowing down of charged particles (for example, at d<10*R) does not increase probabilities of fusion with charged particles? These two questions must be related. "Spending more time near the nucleus" should have the same effect on all projectiles, charged and uncharged.

I would be happy to know how to solve the puzzle without leaning on theoretical concepts beyond classical physics. Please, be aware, that I am not addressing the issue of tunneling effect; nobody ever tried to explain this effect, as far as I know, by using classical physics. But the explanation of the 1/v law, for example in the article chosen by Dennis, appears to be classical. That is my main point. Let me end with a quote extracted from the <http://focus.aps.org/story/v4/st29> article. "A slow-moving neutron spends more time near a target nucleus than a fast-moving neutron, so it is more likely to interact. That makes the cross section -- the nucleus's effective 'target area'--larger for slower neutrons." Thanks for providing this URL, Dennis.

A saying "one picture is often worth more than many words" is applicable. But CANA messages cannot contain pictures; they are for text only. To create a picture follow these steps:

a) Using the last formula above (and several values of d between 2 F and 20 F), plot positive V1 (in MeV) versus d.

b) On the same graph plot negative V2 versus d, according to a rectangular well potential. In doing this assume that R=7.5 F and that for d<R the absolute value of V2 is "moving toward infinity."

c) Calculate V = V1 + V2, for several values of d (also between 2 and 20 F), and plot V versus d, for example, in red.

The red curve will have a peak at d=R. The height of that peak is called coulomb barrier, CB. It is not difficult to see what effect a change in R should have on CB. For much smaller R, the CB would be much higher; for much larger R, the CB would be much smaller. Coulomb barriers are often compared with gravitational barriers associated with mountains and hills (as illustrated in Figure 1 of unit # 40, on this website).

Appended on 8/xx/2005:

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245) What can be done to avoid a global disaster?

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Discussing energy-related issues (on phys-L, a website for physics teachers) one contributor asked three questions. The replies posted by another teacher (see below) are interesting and worth showing here.

1) By how much would the emission of CO_2 etc. be reduced if 100% of US electricity were from nuclear reactors?

Not likely to happen. Currently we get over 60% of our electricity from coal and about 15% from nuclear. We would need on the order of 300 new nuclear plants to take over the coal burden. It is also the case that our energy demand for electricity is increasing (as a percentage of total energy use) even though the electrical usage is only on the order of 20% of total usage [actual values depend on whether you look at gross or net usage.] Even with the incentives of the new energy plan, it could be 10 years before the first new nuclear plant would be built.

2) By how much would the emission of CO_2 etc. be reduced if 100% of US vehicles were powered from rechargeable electric batteries (using nuclear electricity)?

Not likely because of the lack of progress in battery technology. Fuel cells (run on hydrogen separated by electricity from water) are a possibility, but current prototype vehicles run hundreds of thousands of dollars. There is the whole infra-structure, chicken & egg thing here too--you can't sell hydrogren powered cars (combustion or fuel cell) until there is a wide spread network of hydrogen 'filling stations' and who will build those if there are no hydrogen powered cars to buy the fuel.

3) By how much would the emission of CO_2 etc. be reduced if we used trains and busses instead of automobiles?

The infra-structure cost of mass transit (other than busses) is just too prohibitive to expect to see much happening without some kind of major federal program feeding billions of dollars into such a project. [New urban rail project go for 50-500 million per mile.] Getting people to ride busses is not going to be easy either. However, one CAN reduce gasoline usage by 30-40% just through car-pooling [70% of our driving is done commuting to work]. Agressive programs could do a lot with this without any infrastructure costs. However, there will be GREAT opposition to any reduction in 'driving freedoms.' We've built an entire culture (and a large chunk of our economy) based on the automobile, and moving away from such will be very difficult and disruptive to the economy (short term at least).

Conclusions:
For actual numbers the web. <http://www.eia.doe.gov/> is the place to find all the information you would need. The bottom line to all of this is that there won't be any quick and drastic cuts in CO_2 emmissions from the U.S. or from the world for that matter. Weaning our energy systems off of the fossile fuels (currently 90% of supply) will take 50 year at best and more likely will take most of this Century to accomplish. However, it is by no means certain that we can

actually totally replace our fossil fuel energy with 'green technologies' and even less certain that we could do this for 9 billion people all living at a 'western' standard of living. There are money, land usage, and materials problems with scaling up any of the current 'clean' energy technologies to really take over from coal, oil, and natural gas. . . .

Try one of my newer Energy Management Simulators to get a feel for the problem--see web site below.

****
R.W.Tarara
Professor of Physics
Saint Mary's College
Notre Dame, Indiana

Free Physics Instructional Software
Windows & Mac

New Energy Management Simulation software just posted.

www.saintmarys.edu/~rtarara/software.html
****

Appended on 8/3/05:

The discussion is going on. Teacher #3 wrote:

Some discussion of energy reserves and the exhaustion thereof can be found at:

http://www.av8n.com/physics/fossil-resources.htm

which is based on a calculation that can be found at

http://www.av8n.com/physics/energy-reserves.html

This concludes that "switching to coal" is not much of an "alternative". Extrapolating current usage suggests that coal will last longer than oil, but not much longer .... and switching doesn't solve the main problem, namely exhaustion of the *total* fossil energy reserves. U235 doesn't change the story very much, either, because there's not enough of it.

IMHO this is a methodical and dispassionate analysis. If you disagree with the raw data, please provide better data. If you disagree with the method of analysis, please present a more-meticulous analysis. I've presented my evidence in detail. I see no reason to consider it a paranoid fairy tale. If you have evidence to the contrary, let's see the evidence.

And here is what Richard Tarara wrote to me in private, after I asked him if it is OK to show his name and affilation on the CF website.

You can use my name and affiliation AND my web page (see signature below). Actually, what my simulators and my class project seem to show is that we CAN change over to green tech on a 100-125 year schedule, at least some countries can. It will be expensive, but according to an economist (Kevin Laws--Pricilla's son) who frequents the PhysSoc list, not unreasonably so. The big problem for some will be land--Japan can afford the technology but has no place to put it. Western Europe can afford it, Eastern Europe has the land -- seems like a marriage proposal to me. Russia and India can't afford it.

The model is primarily one without a Deus Ex Machina--no cheap fusion (hot or cold)--no other rabbit from the hat [recognizing Nuclear as the only new energy technology of the 20th Century suggests the strong possibility that we are stuck with what we've got.] The scenario is to eliminate oil and natural gas in 100-150 years. Coal and Nuclear can play a role, but ultimately should be phased out as well. That leaves Hydro (minor contribution), Geothermal (likewise), Biomass (land intensive), Wind and Solar. The latter two must carry most of the load, but neither are energy on demand resources. In our model, besides the direct use of electricity from each, additional wind and solar sites are used to produce hydrogen which is piped (a $5 trillion infrastructure upgrade) throughout the country to be

used in place of natural gas, as a portable fuel (combustion or fuel cell), and perhaps to power traditional HIGH DENSITY thermal power plants. The latter would be very convenient in say the East Coast corridor with the high population density, but suffers from ending up at about 25% efficiency considering the initial output of the wind generators or solar cells then taken through the hydrolysis process, piped hydrogen to the east, then the 33% efficiency of a typical thermal-electric plant.

The bottom line for the U.S. is it will cost $200-400 billion per year and end up using maybe 20% of our land area to provide the current per capita energy levels (with corrections for efficiency and conservation changes) for a population of 450-500 million people. All that can be done with the will to do it. The model works for other areas as well--Europe for example--but fails in places like Japan, Indian, and Russia.

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246) Manipulating nuclear decay rates

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Introduction:
The major controversy of cold fusion is whether or not nuclear processes can be influenced by processes taking place at the atomic and molecular level. The prevailing view, based on reproducible experimental data, is that what happens in atomic nuclei (containing at least 4 protons) cannot be influenced by chemical changes. The half life of ^{137}Cs, for example, is the same in chemically pure cesium, in all compounds of cesium, in atoms located in air, in the vacuum, in digestive systems of living organisms, or in sources located near crystal screens. It is probably the same for strongly ionized cesium atoms in our sun and other stars. That point of view has been challenged when the discovery of cold fusion was announced in 1989. According to Fleischmann and Pons, nuclear processes of some kind can be triggered by chemical processes taking place in some electrolytic cells. Other cases, contradicting the prevailing view, have been reported since that time. Two such cases are described below.

Microbial destruction of a radioactive cesium
Members of the International Society of Condensed Matter Nuclear Science (ISCMNS) now have a restricted Internet discussion list named CMNS. At the 11th International Conference on Cold Fusion (ICCF11, September 2004) a Ukrainian scientist, V.I. Vysotskii, presented a paper on microbial decomposition of radioactive cesium. This morning I referred to that paper in a reply to a message from another member. I wrote:

Thanks for the second message about the BT (bacterial transmutations). I have some questions and comments.

1) How do recognized authorities in microbiology react to BT reports? Do they dismiss them as pseudoscience or do they take them seriously?

2) I suspect that most of us here are not competent enough to either validate or criticize experiments to which you refer. One has to know the metabolism of various living organisms and be a good analytical chemist.

3) I had some difficulties with what you wrote; probably because I am neither a biologist nor chemist. For example, in one place you write: “With the germinating wheat he reported [Hg] -200%; for oats [Hg] -2000%, [Zr] -700%, [Pd] -300%; both were analyzed by ICP-MS.” How to interpret such percentages? What is ICP? Should I assume that MS stands for a mass spectrometer?

4) In the reply to your first message I referred to the ICCF11 paper of V. Vysotskii et al. He reported that radioactive Cs-137 (half-life of about 30 years) can be turned into something non-radioactive in several days. Most CMNS subscribers are probably aware that rapid deactivation of Cs-137 and Sr-90 would simplify radioactive waste disposal problems (dealing with spent reactor fuel rods) by at least one order of magnitude. Many objections against nuclear electricity would no longer be valid.

5) Vysotskii report on bacterial decomposition of Cs-137 is listed in our <www.lenr-canr.org> library. Unfortunate, unlike his other ICCF11 report, it is not downloadable. Was it published in a biological journal? The paper was based on experiments which many physicists (working with biologists) can perform by using a widely available NaI gamma spectrometer. The microbial transmutation would be recognized as real if radioactivity was destroyed rather than

transferred, for example as a metabolically produced gas escaping a culture flask. Another potential error would be to ignore a drastic redistribution of Cs within the flask (leading to a very significant change in counting geometry). V. Vysotskii, who supervised the biological experiments, seems to be fully aware of such potential traps. I was highly impressed by precautions taken by the team to avoid trivial errors.

If my memory can be trusted they had about ten identical experiments (and ten controls) occurring at the same time? Rapid decrease of radioactivity (in days instead of 30 years) was observed in each tube. That was about one year ago. What is the situation today?

6) Suppose you find a microbiologist willing to replicate Vysotskii's experiment. The first question would be "where do I get the Cs-137?" A science supplies catalog for teachers will show you where a Cs-137 source of ~ 1 microcuries can be ordered. No license is needed for a source of such activity. Other option is to use dry wild mushrooms. Let me tell you a story about this.

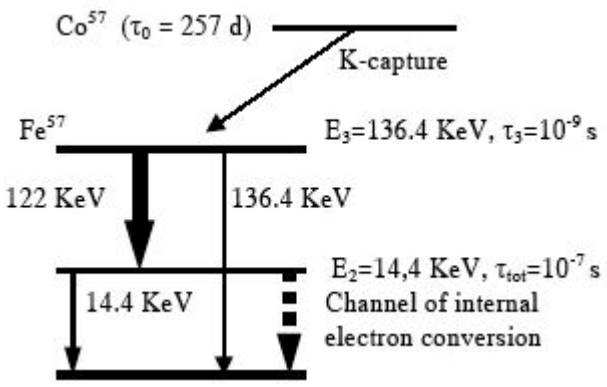
7) More that ten years ago a biology teacher from my university visited his family in Poland and brought back dried mushrooms. Knowing about Chernobyl I suggested that we put about 200 grams of them next the NaI spectrometer. The peak at 660 keV became clearly recognizable after about one or two minutes. But we wanted to be sure that this was due to Chernobyl. So we obtained similar mushrooms gathered in the USA and repeated the experiment. To our surprise we discovered the same 660 keV peak, but the activity (per gram of dry mushrooms) was about 3 times lower. Cs-137, mostly from nuclear explosions in 1960's, is all over the world. Our findings were published in a biological journal. Wild mushrooms are likely to be a good source of Cs-137 for your experiments.

Nobody responded to my reply, so far. This, however, does not surprises me, people who I expected to comment might be on vacation at this time of the year. I will append good comments, if they materialize. Please revisit this unit next month.

A sheet of mica near a radioactive source changes the gamma decay probability

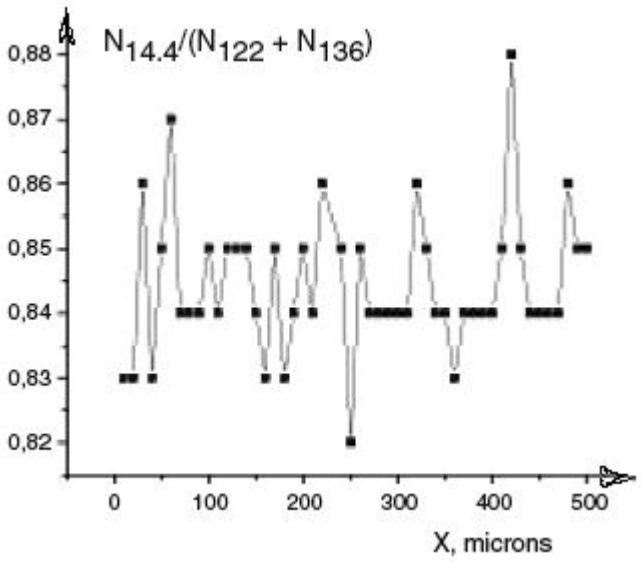
A year before announcing the microbial effect on ^{137}Cs, Vysotskii and his colleagues (from Moscow State University) made another announcement. That was at the 10th International Conference on Cold Fusion (ICCF10, August 2003). Their paper, entitled “The theory and experimental investigation of controlled spontaneous conversion nuclear decay of radioactive isotopes,” can be downloaded from the library at <www.lenr-canr.org>. Let me summarize the experimental part of that interesting paper.

A radioactive source -- ^{57}Co -- (T=257 days decaying into ^{57}Fe by K-capture) -- was placed in front of a detector. Gamma rays of energies of 136.4 keV, 122 keV and 14.4 keV, emitted from the ^{57}Fe nuclei (T=1 nsec) were recorded. There is nothing new about this; the energy diagram of the decay process is shown below.



I can easily imagine three gamma ray peaks in a multichannel analyzer. What is new and interesting is the effect thin mica sheets on relative intensities of the peaks. The authors discovered that the ratios of peak intensities can be

changed by introducing a 50-microns-thin mica sheet into the region between the source and the detector. Labeling the areas below the peaks as N14, N122 and N136 they characterized the effect of mica by the ratio R, defines as N14.4/(N122+N136). By changing the distance X, between the source and the mica sheet, they discovered that, R depends on X, as illustrated below.



Unfortunately, no bars of errors were assigned to individual data points and nothing was stated about reproducibility of results. For example, is R always equal to 0.82 when X=250 microns? And is R always equal to 0.88 when X=420 microns? I will assume that observations were reproducible and that the error bars were "too small to be shown." To give the authors all benefits of my doubt, I will also assume that control experiments were performed to show that equivalent screens made from other materials had negligible effect on the values of R at different X.

Taking these assumptions for granted I tentatively accept the main claim of the paper: "In these experiments we discovered an inhibition of the conversion channel for nuclear decay by 7–10%, and a change (increase) of the total lifetime for the radioactive $^{57}Fe^*$ isotope by 6–9%, at the optimal size *X* of the slot, in relation to spontaneous decay in free space without the thin mica crystal."

P.S.
This mica screening effect on ^{57}Fe is not as strong as the bacterial effect on ^{137}Cs. But each of these effects, if confirmed by other researchers, will show that the prevailing point of view has only a limited validity. Emission of gamma rays is a nuclear effect and ability of influencing it by screening the source with a thin sheet of mica (a mono-crystal) is not consistent with the prevailing point of view. How can a crystal, situated hundreds of microns from the atomic nuclei of the source influence what happens in the nuclei? To answer this question one should be able to understand the theoretical part of the paper. Unfortunately, i do not understand it, due to my very limited background in theoretical physics. But I would very much like to know what theoretical physicists think about the paper. By skimming the first part of the paper I notice that the explanation is based, among other things, on the concept of "zero-energy." The authors claim that experimental results confirm their theory.

Why tracers have not been used to study transmutations? (Appended on 8/8/05)
Focusing on transmutations involving radioactive isotopes has a great advantage over focusing on stable isotopes. Most people here are well aware that analytical techniques used to study stable isotopes are much less sensitive than analytical techniques used to study radioactive substances. Let me illustrate this numerically. I will arbitrarily assume that the limit of detectibiity, of cesium, by a traditional analytical method is one microgram. This is $4.5*10^{15}$ atoms. Yes, I know that the limit is different for different elements; this is probably a good representative number. Let me compare it with the limit of detectivity of ^{137}Cs, for example, in mushrooms. Suppose I am using an NaI crystal in a

situation in which a 660 keV peak can easily be identified when the corresponding counting rate is 1 per second. I will assume that the counting efficiency, including the geometry, is 1%. It means that I am able to easily recognize ^{137}Cs in a source whose activity is 100 disintegrations per second.

How many atoms are there in that source? The half-live of 30 years implies that the decay constant, lambda, is equal to $6.75*10^{-10}$ sec^{-1}. If the activity is 100 then the number of atoms is $1.48*10^{11}$. That is about 30,000 times less of the above-chosen level of detectibility by a non-nuclear analytical method. I selected cesium because bacterial transmutation of that material is being studied in Ukraine. And I have no doubt that even 10^{10} atoms of ^{137}Cs can be identified with a carefully designed high efficiency detection system.

If I were to study nuclear transmutations involving non-radioactive elements I would think about using tracing techniques. Suppose generation, or destruction, of common ^{133}Cs is suspected in the electrolyte of a Naudin's "reactor." How can this be tested? By deliberately injecting a tiny amount of radioactive ^{137}Cs, perhaps a fraction of a milligram, into the liquid before the experiment. The rest should be obvious to a nuclear scientist.

a) Take several cc of the fresh electrolyte and dry it to create a spot source. Then measure the radioactivity, A1, of that source.

b) Conduct the experiment.

c) Take several cc of the used electrolyteand dry it to create another spot source. Then measure the radioactivity, A2, of that source, under identical conditions.

If A1 and A2 turn out to be significantly different then you have an indication that the suspected transmutation might have occurred. Note that only the ratio is needed, systematic errors in A1 and A2 are likely to be identical and they should cancel each other when the ratio is calculated. That is only a general idea. Reality is likely to be less simple. Fortunately, practical difficulties can usually be resolved by anticipating various complicating factors. One should be certain, for example, that a difference between A1 and A2 is not due to the redistribution of cesium, say from the liquid to a metal, or to the surrounding air, during the experiment. Designing a convincing experiment is always a challenge.

To study transmutations involving silver the radioactive tracer could be the beta and gamma radioactive ^{110}Ag (T=253 days). To study transmutations involving zinc the tag could by ^{65}Zn (T=245 days), to study transmutations of platinum the tag could be ^{188}Pt (T=10 days), etc. Shorter half-lives are better, in principle, because they offer higher sensitivities. On the other hand, working with a rapidly decaying tracer would call for corrections based on duration of experiments. A radioactive tracer whose half-life is about ten times longer than the duration of an experiment seems to be ideal, provided its activity can easily be measured, with an available detector, at reasonably high efficiency. Yes, I know that obtaining necessary tracers can be a problem, unless one has access to an experimental nuclear reactor, or to beams of accelerated particles.

P.S.

This note has been inspired by G. Milley's ICCF10 review of transmutations. His paper is downloadable from our library at <www.lenr-canr.org>.

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247) Magnetic forces and coulomb barrier penetration

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It is well known that electrically charged objects acquire magnetic poles (N and S) when they rotate. The N and S poles, situated on the axis of rotation, become stronger when the angular momentum increases. Note that I am saying "rotating" rather than "spinning" because it is not a quantum mechanical consideration. Yes, I know that laws of classical physics do not always apply at the subatomic level. The concept of magnetic poles is used because I believe in its pedagogical usefulness. Those who played with bar magnets, or with compass needles, knows that "unlike poles attract each other while like poles repel each other." That is all I need to show how rapid rotation can help charged particles to penetrate coulomb barriers.

Deuterons are positively charged. Suppose that two deuterons are rotating. Also suppose that the angular momenta are anti-parallel. By this I mean that the axes of rotation are parallel but one direction is clockwise while another is counterclockwise, when seen from the same side. In other words, if the N pole of one deuteron is up then the N pole of the other is down, and vice versa. Under such condition the particles would not only repel each other (due to positive electric charges); they would also attract each other (like two anti-parallel bar magnets). The net force between them would be reduced in comparison with what it would be without rotation. By increasing the angular momentum one would reduce the net repulsive force. The probability of fusion (at a low energy of relative motion) is thus expected to increase with angular momenta.

How large should the rotational angular momenta be to significantly increase the probability of fusion? And what kind of NAE (nuclear active environment) would produces rotating nuclei? I asked these two questions on the CMNS list. I would be happy to post the replies (anonymously), if they materialize. Please revisit this unit in a couple of days.

Appended on 8/11/05:
1) Here is one comment, sent to me in private: "In a particle physics this is called 'interaction between polarized deuterons'. I do not remember exact numbers. But I do remember that the cross section of the DD reaction does not change a lot (factor 2 maximum for some energy range) for flip and non flip states of polarized deuterons in a wide energy range of interacting deuterons." I would prefer this to be posted on the list, to see what others have to say. But private messages are welcome. I suppose that the above comment refers to free deuterons. Deuterons studied by cold fusion researches are usually not free; they are trapped in solids. Some CF researchers say that one should not extrapolate from free deuterons to deuterons in solids.

2) What I posted was inspired by a message on the CMNS list. This message is worth showing here, I think. "I agree, reproducibility is important, but it is not the main problem. The main problem is to understand the environment and mechanism that results in nuclear reactions. Without this knowledge, replication remains difficult. In addition, when replication is achieved, it is rejected because success looks like random chance rather than intention. Also, as long as replication involves measuring energy using a calorimeter, the result will be rejected simply because orthodox physics does not trust [low wattage] calorimeters. In fact, I don't trust most calorimeters. Painful experience has shown me that too many ways exist to screw up. Besides, a standard calorimeter does not exist, like a standard radiation detector. As a result, an ordinary physicist has to master each new calorimeter in order to trust the data. I suggest the only calorimeter type that is sufficiently simple for physicists to understand is the Seebeck-type, which is seldom used.

So, what is the solution? First of all, I think everyone needs to acknowledge that CANR does not occur in ordinary materials under ordinary conditions. Replication is difficult because this special material is not normally present and needs to be made before the effect can be produced. Second, any theory that is based on ordinary material, like PdD, or on special and unproven mechanisms, like ZPE extraction, must be viewed with skepticism. In fact, skeptics reject the claims simply because such theories are used to explain the observations.

If we want orthodox physics to take the field seriously and achieve reproducibility, we need to address the real world, not the limited and imaginary world proposed by many in the field. CANR has to be recognized as a normal process that is part of normal science, but one that occurs only because a special atomic lattice is available. The challenge is to understand what makes this lattice unique and what mechanisms are only initiated within this nuclear-active-environment. In addition, all of the novel observations need to be explain, not just a few that are easy for a particular model to address. After all, how many independent mechanisms and environments do you think are operating? OK, I've thrown down the challenge - what do you all think of this approach?”

Let me mention that a low wattage calorimeter was used , by Pierre Curie in 1903, to discover the unexplained heat produced in his radium source. Likewise, the theoretical discovery of neutrinos (by Pauli) was an attempt to make sense out of calorimetric data on heat from a beta radioactive source. I was under the impression that all scientists have great respect for calorimetry. But calorimetry at low wattage is far from being trivial; the person who wrote the above is one of the top experts in that field.

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248) A proposed set of names

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As mentioned in another unit, I belong to the International Society of Condense Matter Nuclear Science (ISCMNS). That society has a restricted discussion list, called CMNS, for its members. Let me share a message I just posted. It was a reply to XX, a well known scientist. Sorry for not showing names, when quoting from that list. I will append interesting replies, if they materialize. Please revisit.

On Friday, Aug 12, 2005, XX wrote:

> . . . Over time, as results come in, one's views and interests
> change. At least mine do! I have a more open mind regarding
> various "cold fusion" claims than I did years ago. That does
> not mean that I accept or even fully understand all the claims.
> Does anyone? However, I do try to listen. For example, I'm
> amazed at the results showing transmutation in deuterated
> metals -- who would have suspected this back in 1989? . . .

1) Just imagine how different would the history be if R. Gajewski, working for the DOE, did not confuse cold fusion projects of Steven Jones, already supported by the DOE, with an excess heat proposal submitted to the DOE (in 1988?) by Fleischmann and Pons. Why did Gajewski select Jones to be a reviewer of the new proposal? The essence of the P&F proposal was excess heat unexplainable by chemical reactions. Why was such proposal not sent to chemists experienced in calorimetry? If it were sent to chemists then subsequent developments would not evolve into a highly undesirable controversy.

The naming of the P&F phenomenon "cold fusion" was one of the most tragic mistakes in the history of modern science. It created a situation in which the coulomb barrier, and the absence of expected neutrons, became convincing arguments against the new field of investigation. Naming things inappropriately is undesirable, especially in science. Suggestions to stop using the term "cold fusion" inappropriately have often been made.

2) I would very much like to have a non-confusing set of names, for what we are studying, at least on this list. My message is a step in that direction. Keep in mind that it is only a proposal. Perhaps we can discuss it, modify it, if necessary, and establish a set of acceptable names.

3) The name of the entire field should be CANA (chemically assisted nuclear anomalies). The field consist of three distinct sub fields: CF (cold fusion), CT (cold transmutation) and EE (excess energy). Additional sub fields can be identified, when needs develop. Yes, I know that the name CANR would also be appropriate. But it is less easy to pronounce than cana. And the word "anomalies" is probably better than the word "reactions," at this stage. The name CMNS would certainly be appropriate if it were easier to pronounce. I suspect that speakers of many languages would pronounce CANA in nearly the same way.

4) The cold fusion sub field, CF, consists of phenomena in which fusion of hydrogen nuclei, such as D and D, takes place at ordinary temperatures. In the case of D + D the dominant output channels are n + 3He and p + 3H, as in hot fusion. Output channels would be different for the T+ D, T + H, D + H, or H + H. Several phenomena studied by Jones belong to the sub field of CF.

5) The cold transmutation sub field, CT, consists of nuclear processes in which chemical elements, heavier than hydrogen, are synthesized or decomposed, at ordinary temperatures. Transmutations produced by common neutrons, or by accelerated charged particles, are not anomalies.

6) The excess energy sub field, EE, consists of nuclear processes in which unaccounted-for energy is generated. The term "unaccounted-for'" is used to exclude energy from exothermic chemical processes. Should we add that the EE sub field deals with the CF and CT phenomena only? I keep changing my mind about this.

Please comment. Please revisit this unit; I plan to append items.

Appended on 8/15/05:

1) On Sunday, Aug 14, 2005, YY wrote:

". . . I was always convinced that calling anything ANOMALOUS is in some way contraproductive, even suicidal. . . "
Here is my reply:

If other people feel the same way then let us say that CANA stands for Chemically Assisted Nuclear Activities (not Nuclear Anomalies). But CANA is the least important part of my proposal. The CMNS name, for the entire field, is fine with me. (I preferred CANA because it is easier to pronounce.)

2) The important part of my proposal is to stop using "cold fusion" instead of CMNS. Cold fusion (CF) is only a subfield of CMNS. That is what should be recognized as troublesome. Other recognizable subfields are CT (cold transmutation) and EE (energy excess). Benefits of not referring to all CMNS phenomena as cold fusion have been recognized by many on this list, long before me.

3) OK, my attempts to initiate a discussion a discussion of the proposal failed. [YY was the only person who responded to several appeals, posted last week.] But I intuitively feel that many on this list agree that consistent terminology is desirable. Facing the unexplained resistance to discuss the "alphabet soup" of acronyms, I decided to start using new names consistently, at least for a while. Please do the same and we will see how it goes.

a) The name of the entire field is CMNS.
b) Subfield CF -- cold fusion of hydrogen nuclei -- at temperatures below 10,000 C.
c) Subfield EE -- excess energy (hopefully, not only heat).
d) Subfield CT -- cold transmutations -- excluding those due to common neutrons and accelerated particles.

4) In another thread YY wrote: "Terminology is only a part of a greater problem." That is true. But why should this be an excuse for not fixing a small part of the big problem?

To which the immediate reply was:

". . . I said that :'Terminology is only a part of a greater problem.' Because as long as we do not understand the processes, their relationships, the situation, the correlations - how do we know that the terminology has a sound basis? If you don't know that your coming child is a girl or a boy- what name can you give her/him? OK, in the US you have such gender-neutral names as Leslie- but we here have to chose- Ludwika or Ludwik, Petra or Peter."

To which I responded:

But people do refer to CF (cold fusion) as if it included the EE (excess energy) and the CT (cold transmutations). This contributed, and continues to contribute, to undesirable consequences. What harm can possibly result from our consistent use of the proposed set of names? Let us try it, at least on this list. Yes, I know that most people on this list are never confused by inappropriate use of names. That is why they do not want to waste time, I assume, on discussing the suggestion. But context dependent terms are highly undesirable, especially when we talk with others.

YY wrote:

"OK, I think this distinction is OK. Let's use it."
This turned out to be an empty promis. Less then 24 hours later, in an interesting reflection about history of the CMNS field, YY reffered to this entire field as cold fusion. He was mostly writing about EE (Patterson and Case cells, Toyota projects in France, etc.), but the term EE was not used. It is not easy to stop using terminology to which one is attached.

RR wrote:

This is a question of linguistics and language history, which I studied for many semesters 30 years ago. The answers are: this issue does not matter; we have no control over words; we should stop worrying about this; and words often do not mean what they sound like, or what they originally meant. The study of weather is called "meteorology" because people used to think that meteors had something to do with weather. "JPL" stands for Jet Propulsion Laboratory even though that laboratory has always concentrated on rocket propulsion. It was named "Jet" because when it was established rocket propulsion was somewhat disreputable, being associated with science fiction comic books. Whatever name sticks to cold fusion will stick, and no one anywhere has any say about it. Even if cold fusion turns out to be something other than fusion, if it turns out to be several different phenomena, if the name is firmly entrenched it will not change. We still talk about "dialing" a telephone number even though most people have never seen a dial, and I expect we will still refer to movies as films long after the film is replaced with digital bits recorded on hard disks.

One other linguistic note. Euphemisms never work for long. People who are embarrassed or offended by words invent euphemisms, but the stigma quickly follows and attaches to the new euphemism, so they have to invent another and another. Along the same lines, some people have suggested that we coin a new term for cold fusion because the present term -- "cold fusion" -- is associated with so much controversy. We need a fresh start. Alas, even if we could somehow get everyone to call it "LENR" instead, the controversy would follow immediately.

Referring to another message ZZ wrote:

Bill, the concept of chemistry being involved is based on the observation that a solid lattice appears to be a common feature for producing the anomalous nuclear effects associated with CANR. This environment, because it has a high concentration of electrons and a periodic nature, allows a variety of mechanisms to operate that are not present in a plasma. These features are the basis for conventional chemistry, hence allow one to observe that the nuclear reactions are assisted by chemistry. The designation CANR focuses attention on the environment while LENR focuses attention on the mechanism. The choice of which is more accurate goes to the heart of understanding, and obviously has not yet been resolved.

As for the neutron emission observed by Mizuno, we do not yet know whether this is caused by a cold fusion environment. A CF environment seems unlikely because neutrons are normally not produced by cold fusion. It is possible that several different and independent processes are operating, one producing He4, and others producing neutrons and tritium. When evaluating theory, this possibility must be kept in mind.

QQ wrote:

Regarding the definition debate PP wrote some wise lines, and I'm sure he wouldn't veto me reproducing them here: "If we say that the term 'cold fusion' is bad, it will appear that we are attempting to push away the stigma. That we cannot do, it will have to dissolve on its own accord, as a result of the scientific truth and efforts to communicate. In

terms of public relations, to divorce CMNS completely from the term CF is not appropriate and will look weak."

Ludwik Kowalski wrote:

Let me repeat it; calling the entire CMNS field cold fusion is bad. The term CF is not bad but it should be used to describe only some CMNS phenomena. Why was the term "divorce" introduced by Steven K? It was not a correct description of my proposal.

I think that referring to the entire CMNS field as CF is harmful, especially when one tries to convince scientifically educated people. Many of them associate the CF term with the coulomb barrier (Gamow factor etc.). What do they usually say when one tries to convince them that excess heat is real, in such or such experiment? The moment they hear the term CF they ask about the coulomb barrier. That is not good; I have much more confidence in reality of excess heat than in validity of existing speculations about its origin. Yes, coulomb barrier is a decisive aspect for cold fusion. But why should one think about it in discussing excess heat? It is too early to approach the excess heat subject from the theoretical side. Unfortunately, that is what often happened when I referred to excess heat as cold fusion. The discussion with scientifically educated people should focus on such things accuracy (systematic errors) and precision (random errors). And we should be open about the absence of understanding.

Confusing excess heat with cold fusion (of two hydrogen ions) was harmful in 1989 and is harmful today. How often do you hear the old argument that "if CF were real then P&F would receive deadly doses of radiation, due to neutrons." I hear this very often. We do need the term CF but it should NOT be used to describe the EE (excess energy) or CT (cold transmutation) phenomena. People will understand that what we know today is very different from what Fleischmann and Pons suspected in 1989. That what we should tell them to explain the need for the CMNS term.

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249) An interesting theory of Akito Takahashi

Ludwik Kowalski (6/18/05)
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As mentioned in another unit, I belong to the International Society of Condense Matter Nuclear Science (ISCMNS). That society has a restricted discussion list, called CMNS, for its members. A recently published theoretical paper of a well known Japanese researcher is now available for downloading at

http://newenergytimes.com/Library/2005TakahashiA-CondensedMatterNuclearEffects.pdf

That is what was announced on that list yesterday. After reading this paper I posted this message:

Thanks for making this paper of A. Takahashi available to all. I sense that it is an important contribution. But my background is not sufficient to understand the theory. What I would like to achieve is a simplified (intuitively appealing) description of basis assumptions and essential conclusions. I am probably not the only one who would appreciate help from somebody who already grasped the main idea. Let me try to write down what comes to my mind as I read the paper. Please correct where I am wrong.

a) The so cold "hot fusion" involves collisions between two D ions. The coulomb barrier is high because the nearest electrons are too far away to help lowering the barrier.

b) And what about two neutral D atoms forming a neutral covalent molecule? Here the average distance between positive nuclei is 0.07 nm. Thermal fluctuations around this internuclear distance do not lead to fusion because orbitals in which electrons reside are too far away. In other words, the coulomb barrier is still too high to make cold fusion observable.

c) Takahashi invents a neutral quaziparticle TSC (Tetrahedral Symmetric Condensate). It is a squeezed quazi-molecule made from two neutral molecules in tetrahedral configuration. One way to visualize this configuration is to take two identical tooth picks and imagine four positive deuterons at their four ends. Make a symmetrical cross (two sticks on top of each other) and pull one stick away from another. The distance between the sticks should be such that distances between any pair of deuterons is the same. In Figure 5 (on page 7 of A.T. paper) the four deuterons are blue sphere. Each deuteron has an electron associated with it; electrons are represented by yellow spheres.

d) I do not know what causes such system of eight charged particles to collapse. And I believe the author that orbitals in which electrons reside are such that the coulomb barrier is lowered very significantly. The main point is that fusion of four deuterons into ^{8} Be is much more probable than fusion of two deuterons into ^{4}H (from a neutral molecule). Once formed, the ^{8}Be nucleus at once breaks into two alpha particle. instability of ^{8}Be with respect to fission into two helium nuclei is a well known process.

e) Another important point is that the TSC, approaching another nucleus (such Al, Ni, Pd or U) acts as a nearly neutral particle. This happens because negative electrons reside very close to positive deuterons. (Do not ask me why.) For that reason fusion of TSC is no longer inhibited by coulomb barrier. The probability of fusion with heavier nuclei, in

this model, depends essentially on the probability of formation of TSC in condensed matter. The same is true for the probability of producing helium from TSC.

5) Is this a fair description of the main idea? Numerical conclusions are summarized in Figure 6. I see an obvious font substitution error here; what is labeled Rambda should probably be lambda. Is lambda the usual decay constant in sec^-1? I have other questions about that essential figure. But this can wait.

Please comment, please correct, please add. Please share your own attempt to understand. Thanks in advance.

That was about 24 hours ago. Nobody answered so far. Actually it is wrong; I did receive an interesting essay from Russia but I do not think it explains Takahashi's theory. Today I downloaded -- from the library at http://lenr-cmnr.org -- the paper that A. Takahashi presented at the last International Conference of Cold Fusion (ICCF11). It is also about tiny clusters of more than two hydrogen atoms. The author states that a TSC does not have to be a DDDD. In a mixture of heavy and light water a TSC can also be a DDDH, DHDH, DHHH or HHHH. Pure DDDD produces ^{8}Be that decays into ^{4}He +^{4}He liberating 47.6 Mev of nuclear energy. The DDDH, on the other hand, produces ^{7}Be which decays into ^{3}He+^{4}He liberating 29.3 MeV of energy. Likewise DHDH produced ^{6}Be which decays into ^{3}He+^{3}He. Both ^{4}He and ^{3}He are expected when an electrolyte contains a mixture of heavy and ordinary water. Instead of fissioning a TSC quaziparticle, created near the surface of a large nucleus, can fuse with that nucleus.

In other words, the variety of output reaction channels is much larger than it would be if all TSCs were DDDD. Takahashi performed an experimentally verifiable (in principle) relation between the percentage of light water in heavy water and the expected ^{4} He / ^{3}He ratios. I like when theoretical results are presented in terms of relations between measurable quantities. But I still have a bunch of questions. What leads to formation of TSC clusters? Why are these clusters so small? I hope that someone will answer such questions. I guess the answers are hidden somewhere in earlier theoretical publications of Takahashi.

Small clusters make me think about DD molecules, made from two hydrinos. They are also said to be tiny. Occasional cold fusion of two hydrinos residing in a DD molecule would produce ^{4}He while ^{3}He can be produced from fusion of hydrinos in the HD molecules. Likewise, very small DD and HD clusters, approaching heavier nuclei, would produce compound nuclei because coulomb barriers are nearly eliminated. In other words, cold fusion (CF) and cold transmutation (CT) can be explained either by hydrinos (introduced by Mills) or by larger clusters (introduced by Takahashi). What is not clear is why such clusters formed in the first place. Was their existence postulated to explain experimental data or was it derived from preexisting theories?

PLEASE REVISIT THIS UNIT; I PLAN TO APPEND REPLIES TO MY MESSAGE HERE: IF THEY MATERIALIZE.

Appended later on 8/17/05:
I was composing a reply to Kozima when the follwing message from Takahashi arrived.

Thank you for your comments to my models. I think, you are hitting the essence of my models. Could you please read my two papers and see ppt slides available at SIENA2005 of http://www.iscmns.org/ site, which treats some details, together with my paper for ASTI5 at the same site.

I have now no time to respond your questions to write detail explanation, but I hope you kindly read my older papers for ICCF9, 10, 11 and JCF4, 5, which have treated elaboration of EQPET/TSC models. For fission products distribution for A<200 nuclei, please read my paper Jpn. J. Appl. Phys., 41(2001)7031-7046.

To improve my models, I need great helps from other researchers to formulate for example TSC formation rate at some sites (we need modeling) at/near fractal surface, and for other key issues.

PS: In the last 6 days, I was busy due to the funeral of my father who died with 95 years old.

Appended later on 8/18/05:
1) The following comment was posted by Hideo Kozima, a theoretically inclined CMNS researcher.

It is necessary to understand clearly what is a theory, what is a model and what is an assumption. There are too many assumptions pretending [to be] theories. Generally speaking, following definitions will be applicable. **A theory is** based on principles commonly accepted in scientific world. **A model is** a system of assumptions based on facts but not deduced from principles trying to explain various phases of facts. **An assumption is** individual explanation of facts not forming a system of explanation for facts. We have to distinguish them especially in research of the cold fusion phenomenon. **P.S. In my opinion,** "Akito's theory" is an assumption to explain "Iwamura's data" and some others.

My reply, composed last night, was essentially "philosophy" of science not science itself. It looked childish today and I am glad it was not posted on the list. Let me replace what I wrote with what Frank Wilczek wrote recently, in Physics Today.

"A popular class of problems specifies a force and asks about the motion, or vice versa. These problems look like physics, but they are exercises in differential equations and geometry, thinly disguised. To make contact with physical reality, we have to make assertions about the forces that actually occur in the world. All kinds of assumptions get snuck in, often tacitly."

I see nothing wrong with making reasonable assumptions in attempts to match limited sets of experimental data. After being successful one can go further and make theoretical predictions. Such predictions are either confirmed of contradicted by experimental data. ..... Everybody knows how basic laws of nature were discovered. But experiments must be reproducible to rely on that kind methodology. Experimental scientists are blind without theories and theoretical scientists are paralyzed without reproducible data. That is our tragedy in CMNS.

Added on 8/19/05:
Yes assumptions must be reasonable. Some assumptions are not reasonable. Such assumptions can be called wishes . For example, I can assume that, under certain unspecified conditions, an electron can enter a duteron an remain in its center. A deuteron containing an electron (De) is not repelled by another hydrogen nucleus and fusion can take place at low temperatures. The De clusters are also not repelled by heavier nuclei. Thus they can lead to all sort of cold transmutations. Why are cold fusion (CF) and cold transmutations (CT) so rare? Because the probability of capturing an electron by a deuteron is very low. And why are CF and CT not reproducible? Because we still do not know how to control conditions under which De clusters are formed. My assumption is not reasanable; it cannot be justified in terms of what I know about nuclear forces (either strong or weak).

I suppose that Akito's assumption about neutral DDDD clusters, named TSCs, is not wishful thinking. But I do not know how it can be justified in terms of what is known about distributions of electrons in quazi-molecules. A DDDD is essentially a neutral precursor of ^{8}Be.

Last night a teoretically inclined colleague made an interesting comment about theoretical assumptions that are not reasonable. He said that an assumption should not be rejected on the basis of absence of reasonabliness. The only valid reason for rejecting an assumption should a discrepancy between logical (mathematical) consequences of using it and experimental data. The well known assumptions on Niels Bohr were that (a) electrons do not radiate electromarnetic waves due to centripetal acceleration, and (b) only certain orbits are allowed. These two ad hoc assumpions were in conflict with laws of macroscopic physics. They were certainly not based on anything known before 1912. His hypothetical theory became valid after its true predictions (facts that were not known in advance) were confirmed.

One true prediction, in Takahashi's paper presented at ICCF11, is a graph showing how the isotopic 3He/4He ratio should depend on the percentage of light water in the $D_2O + H_2O$ electrolyte. Will experimenta data confirm this graph? This remains to be seen. The unreasonable assumptions, when they lead to accepted theories are said to be beginnings of new paradigms. In the periods of normal development (between scientific revolutions) assumptions should not conflict with accepted theories. To what extent are Takahashi's assumptions reasonable and to what extent are they ad hoc?

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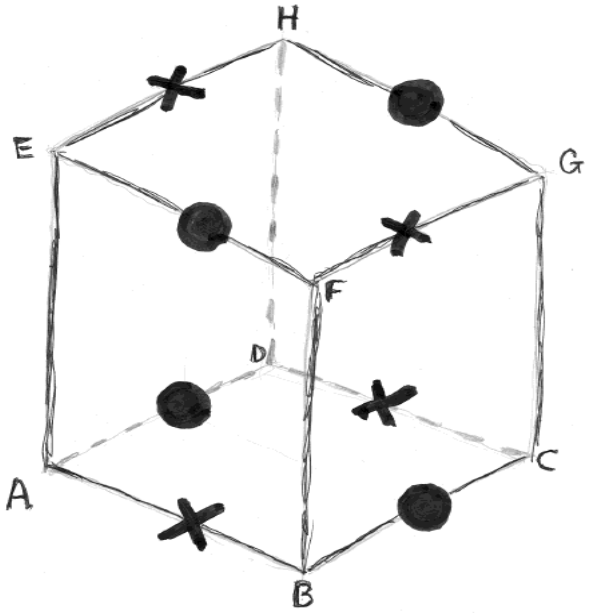
Responding to:

> I liked your comment: "Let the perfect not be the enemy of
> the good". This is a simple explanation of what Dr. Talbot
> Chubb has been saying to the Cold Fusion community for
> years. "If a quantum mechanical explaination is a useful
> model of cold fusion, then use it to predict your experiments".
> (Even if it is not perfect in some peoples view) It could
> be quite GOOD!

I just posted this reply :

Two deuterium atoms, separated by a distance 0.07 nm, form a covalently bound molecule. What kind of bounding keeps four deterium atoms together in a neutral TSC quazi-molecule?

Hopefully somebody familiar with quantum mechanics, and who followed the evolution of the Takahashi's model, will explain what the DDDD clusters are. Meanwhile I drew this schematic picture of a TSC.



Large black dots are atomic nuclei of four deuterium atoms while crosses represent centers of orbitals where electrons reside. If necessary, individual dots and individual crosses can be indentified by the edges at which they are located, for example, the deuteron BC. Note that only 8 edges are occupied. And I am ignoring that this TSC cluster is surrounded by other atoms. These other atoms are probably playing an important role. But this is only a guess. In my mind EFGH is one unusually small molecule and ABCD is another. They are orthogonal with respect of each other. But I have no idea why the molecules are much samller than common molecules and what keeps them together.

Added on 8/20/05:
A theoretical physicist XX posted an interesting summary of a paper that was submitted, but not accepted at a conference this year. But that was not what I expected. An hour later I received a very brief private message from Takahashi. He is going to address the issue at the ICCF12 (next International Conference on Cold Fusion, starting on November 28, 2005). Then he wrote " Please look my ppt slides for IMFP2005 (International Meeting on Frontiers of Physics), especially #58-62 slides, according to transient electron bonding for TSC." The CC of Akito's message was sent to XX.

Slides that were shown at IMFP2005 (Malaysia, July 2005) can be downloaded by going to:

http://www.iscmns.org/siena05/program.htm

and scrolling to "Saturday 14 May." The third item contains two links, one to Akito's slides and another to his talk in Siena. The slides show that the issue of formation of TSC has been addressed. One slide, for example, has this title: "Orthogonal coupling of two molecules makes a Miracle!" Another slide has long equations describing wave functions; presumably indicating where components of the "miracle" (four deuterons and four electrons in a TSC) are located. Unfortunately, my familiarity with QM is very limited; I never had a chance to go beyond what I learned about 40 years ago. For example, I accepted reality of attractive covalent forces (as between two hydrogen atoms in common H2 molecules) without studying QM machinery. What people like me need are popular versions of serious papers. Theoretically inclined physicists often overestimate their audience.

In the Siena paper I see a section about formation of TSC. Here an interesting quote: "We know surface of metal is complex and fractal with ad-atoms, dimers and corner-holes, for example, as illustrated in Fig.2. Somewhere, for instance in corner holes, incident D2 molecules are trapped by dangling bonds. Free D2 molecule has freedom of rotation and vibration. Trapped D2 would lose freedom of rotation, but can vibrate for changing distance between pairing two deuterons, and waiting for incoming D2 molecule. When incoming D2 molecule meets near to trapped D2, incoming D2 rotates with 90 degrees maximum against waiting D2 molecule to neutralize charge (minimize Coulomb repulsion energy) and form an orthogonally coupled two D2 molecules when there meets coherence in vibration modes and electron-spins are anti-parallel for counter part electrons. In this way, TSC may be formed on surface. Since the scenario is still very speculative, we need further substantiating studies."

What is missing, as far as I am concerned, is an explanation of causes of such behavior of two molecules, for example, during the electrolysis, or when hydrogen diffuses through palladium.

Appended on 8/21/05:
1) Here is the message I posted yesterday: "The slides show that the issue of formation of TSC has been addressed. One slide, for example, has this title: "Orthogonal coupling of two molecules makes a Miracle!" Another slide has long equations describing wave functions; presumably indicating where components of the "miracle" (four deuterons and four electrons) are located. Unfortunately, my familiarity with QM is very limited; I never had a chance to go beyond what I learned about 40 years ago. For example, I accepted reality of attractive covalent forces (as between two hydrogen atoms in a common H_2 molecule) without studying QM machinery. People like me need popular versions of serious papers. A set of assumptions and a set of conclusions is usually sufficient. Theoretically inclined physicists often overestimate their audience. "

2) Why am I writing all this? To dramatize situations that are probably more common than theoretically sophisticated people believe. What is the best possible way to guide experimental researchers? I do not know. Experimentalists must try to learn the vocabulary, if not the language, of those who guide them. But theoretically inclined leaders should try not to overwhelm experimentalists with details. Likewise, experimental people should not overwhelm theoreticians with practical details. What is essential for one group is not essential for another.

3) Takahashi sent me the most recent version of his slide show; it should already be downloadable from the library at <http://www.lenr-canr.org>. The beginning of formation of TSC is shown on slide 27. All four electrons, represented by crosses at my illustration above, are simultaneously pushed toward the center. The center is called site T and the process is referred to as squeezing. Squeezing forces are represented by red arrows; they originate from four Pd ions. These ions also form a tetrahedron; they are situated further away from the center than electrons. I suppose that squeezing forces are electrostatic; what else can they be? I am puzzled by directions of these forces on slide 27. Positive Pd ions should attract and not repel electrons.

4) Slide 28 introduced new notation "Quadruplet e*(4,4)." On slide 29 I see that e*(4,4) is called a single particle at site T. The size of that site (~0.01nm) is seven times smaller that the average distance between D and D in an ordinary molecule. The lifetime of that particle is calculated as 2.3 fs. Then, if I understand this correctly, it becomes TSC, whose lifetime is 60 fs. Should I assume that the TCC, in the slide title, is a typing error (it should be TSC)? Slide 31

is not clear because I do not know, for, example, what probability = 10% refers to. I also do not know the difference between dde* and dde*e*. Unfamiliar notation is the most frequent inhibitor, as far as i am concerned. Slide 32 is a reminder that the probability to penetrate a coulomb barrier depends not only on the height of the barrier but on its width as well.

5) Should I assume that 60 fs (on slide 29) is the expected time for the cold fusion to occur? If so then this time determines the outcome of competition between cold fusion (CF) and cold transmutation (CT). The probability of CT would increase if that time interval were longer.

Appended on 8/22/05:
Last night I received a private message from Dr. Takahashi; he started addressing questions and comments that were appended yesterday morning (see above). I am glad that XX, who is a theoretical physicist, also received that private message. He will certainly understand Akito's papers much better that I can. My suggestion is that he cooperates with Takahashi and that together they develop a tutorial. In my opinion a popularized version of Takahashi's model is going to help many physicists, both inside and outside the CMNS community. To trigger this potentially desirable project I am going to send this paragraph to both XX and Takahashi. I am nearly certain that XX would not object if his identity were revealed. I will do this next time, unless he prefers to remain known as XX.

2) Water at low temperatures is a crystal. At higher temperatures it becomes a liquid and than a vapor. In a liquid molecules often cluster into attached groups. Clustering is possible because water molecules are polar and because they are close to each other. Hydrogen molecules, such as DD, are not polar. Therefore the existence of a tetrahedral cluster (see the drawing above) must be explained by something else that electric attraction. Likewise, something else than electric force must explain the squeezing of a DD-DD cluster into a TSC. According to Takahashi phonon excitations are responsible for processes leading to formation of TSC. I guess I must learn something about phonons to understand this.

STAY TUNED; REPLIES WILL BE POSTED.

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250) Another confirmation of nuclear anomalies

Ludwik Kowalski (8/16/05)
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I am reading a recently published paper. It is a short note for electrochemists interested in cold transmutations (CT). I am not even a chemist; this disqualifies me from being an intermediary between scientists and laymen. But I will try, nevertheless, to share some observations. The title is: “Evidence of nuclear reactions in the Pd lattice” and the authors are Stanislaw Szpak, Pamela A Mosier Boss, Charles Young and Frank E Gordon. Submitted in May 2005 the note was published (Naturwissenschaften (2005) 00: 1–4) in July 2005.

The instrument used was an electrolytic cell with heavy water solutions of LiCl and PdCl. The anode was a platinum grid while the cathode was a gold plate coated with Pd and D. These two materials were deposited on the cathode electrolytically by using a very small current (1 mA) in 24 hours. Then the current was increased to about 40 mA (for 2-3 hours) to make sure that the distribution of deposited material is uniform. In the main experiment, lasting 48 hours, with the current was about 100 mA. During the main experiment the cell was in the electric field of a parallel plate capacitor (field of ~ 2000 V/cm). The role of the external electric field is not clear to me; the authors say it was imposed to created conditions favoring nuclear reactions. This reminded me of an experiment (D. Letts et al.) in which a laser beam was used to stimulate the cathode.

After the main experiment the electrodes were removed and examined with a scanning electron microscope. Structural changes on the cathode (localized spots which could only be formed at the above the melting point temperatures) were recognized. One of the authors told me, a telephone conversation, that highly localized flashes (indicators of high local temperatures) were observed during the electrolysis when the cathode was observed with an infrared camera. What kind of reactions were responsible for profound structural changes in the cathode and for the infrared bursts during the electrolysis?

To answer this question the authors submitted the cathode to a different kind of examination. Their scanning electron microscope was equipped with a device able to recognize chemical elements at selected spots of the surface. The name of that device is EDX (electron dispersive X-rays analysis). I never saw such device but its principle of operation is clear to me. Any selected spot can be bombarded with a beam of electrons to generate X-rays. Distinct chemical elements can be recognized by characteristic peaks when X-rays are analyzed with a high resolution spectrometer. This kind of elemental analysis was used by other researchers, for example, by Mizuno in Japan, Miley in the USA and Savvatimova in Russia. All of them reported formation of elements that were not originally present (see units #60, #85, #159 and #206, at this website).

The most prominent elements identified by Szpak et al. were: Al, Mg, Ca, Si, and Zn.
What is the origin of these elements? The authors believe that they are products of nuclear transmutations. Before reaching this tentative conclusion they consider a possibility of chemical contamination. Such conclusion, they claim, would not be consistent with known amounts of trace elements in the electrodes, and in the electrolyte. Furthermore, elements discovered in hot spots were not found in other cathode locations. Structural materials of the cell were examined before the experiment (also with the DEX device) and elements discovered later were not found on their surfaces. Such arguments against a possibility of contamination, are compelling, especially when they are presented by recognized and experienced electrochemists.

But what should I think about their tentative claim that the elements like A, Mg, Ca, etc., are products of nuclear transmutations? Similar claims have been made by other highly qualified researchers. On that basis I take a possibility of nuclear transmutation very seriously. I am puzzled, however, by the absence of radioactive CT products. Being a nuclear physicist I know that both radioactive and not radioactive isotopes are produced when transmutations are induced by accelerated particles, or by neutrons. Why are radioactive isotopes absent among the reaction products? I am certainly not the first person to ask this question. As far as I know, that question has not been answered to satisfy all researchers.

Appended on 8/18/05:
The article that I described, and another recent article published by the same team, can be downloaded from the library at <html://www.lenr-canr.org>. The second article was published in J. Electroanal. Chem., (2005. **580**: p. 284-290) and its title is "The effect of an external electric field on surface morphology of co-deposited Pd/D films."

In the second article the authors explain that the external electric field was applied to increase the capillary forces at the cathode surface. These forces are said to be responsible for some minor deformations occuring during the electrolysis. More drastic, and highly localized, deformations (hot spots), not observed in light water cells, were also observed. They are said to be indicative of local melting, presumably due to highly localized violent nuclear processes. New elements, as reported in the first paper, were observed mostly at hot spots.

One effect that should be expected, when an electrolyte is placed into a strong electrostatic field, is preferential orientation of water molecules (known to be dipoles). The number of molecules whose hydrogen sides are in contact with the cathode is probably much larger than when the electric field is off. What effect does this have on the "double layer?" The local electric field is likely to increase near the cathode surface. Is it conceivable that local electric breakdowns, in the double layer, are somehow responsible for hot spots, and for other surface abnormalities?

Appended on 8/20/05:
It is regrettable that isotopic analysis could not be performed. I am reading the 1996 paper of Miley and Patterson in which a large number of transmutation products was reported. Two methods were used to perform isotopic analysis: SIMS (secondary ions mass spectroscopy) and NAA (nuclear activation analysis). The NAA is a technique with which I am familiar; that is why I find it more convincing. Abnormal isotopic ratios are very convincing indicators that contamination was not responsible for new elements found in the Ni cathode after the electrolysis. One of the new elements was copper. The natural abundances of its two stable isotopes, ^{63}Cu and ^{65}Cu, are 69% and 31%, respectively, as reported in column 3.

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251) Crack fusion

Ludwik Kowalski (8/24/05)
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As mentioned in another unit, I belong to the International Society of Condense Matter Nuclear Science (ISCMNS). That society has a restricted discussion list, called CMNS, for its members. What follows is an interresting discussion of the so-called fracto-fusion. It evolved from the dabate about names of various CMNS phenomena. The sugested names, as described in unit #248, were: cold fusion (CF), cold transmutation (CT) and energy excess (EE). Somebody mentioned that fusion of two deuterium nuclei dominated by the $n+^3He$ and $p+^3H$ channels, as opposed to the ^4He channel, does not belong to CF.

Edmund Storms wrote:

Bill, the concept of chemistry being involved is based on the observation that a solid lattice appears to be a common feature for producing the anomalous nuclear effects associated with CANR. This environment, because it has a high concentration of electrons and a periodic nature, allows a variety of mechanisms to operate that are not present in a plasma. These features are the basis for conventional chemistry, hence allow one to observe that the nuclear reactions are assisted by chemistry. The designation CANR focuses attention on the environment while LENR focuses attention on the mechanism. The choice of which is more accurate goes to the heart of understanding, and obviously has not yet been resolved.

As for the neutron emission observed by Mizuno, we do not yet know whether this is caused by a cold fusion environment. A CF environment seems unlikely because neutrons are normally not produced by cold fusion. It is possible that several different and independent processes are operating, one producing ^4He, and others producing neutrons and tritium. When evaluating theory, this possibility must be kept in mind.

Ludwik Kowalski (addressing Ed) wrote:

1) According to what was reported at ICCF10 by Jones Steven (nearly two years ago) neutrons and protons are signatures of cold fusion. Does the NAE, to which you refer, have anything to do with the phenomena described by him?

2) Would you object if I characterized your current efforts to understand CMNS phenomena as, mostly, studying the EE (excess energy). I know that you were also one of the pioneers in the investigation of CF; I have the accumulation of tritium in mind.

Ed Storms wrote:

1) Personally, I do not think he [Jones] is studying "conventional" cold fusion, but I do not make a big point of this because any replication of an anomalous nuclear reaction is part of the general field and useful to obtain attention. In his case, I suggest the NAE is TiD that forms cracks where a "normal" hot fusion reaction occurs.

2) I produced tritium when at LANL. but I have no way to test for its presence in my present laboratory. This is one of

the basic problems with the field, i.e. we do not have the tools to determine just what is happening. However, I have good calorimeters and can measure heat with confidence.

Ludwik Kowalski wrote:

It seems that Ed is saying that only D + D --> ^{4}He should be called cold fusion; events leading to n+^{3}He and p + ^{3}H (tritium) channels do not belong to CMNS phenomena. If one accepts this point of view then generation of ^{4}He is just another case of transmutation and CF is no longer a subfield of CMNS. That would be ironic.

The answer to the first question is interesting. Ed thinks that electric potential differences (in microscopic cracks) accelerate deuterium ions to energies at which fusion becomes practically possible. The phrase “to obtain attention” might be an indication of something else. The following message from Stven Jones confirms this.

Steven Jones wrote:

Ed wrote: "Personally, I do not think he [Jones] is studying "conventional" cold fusion, but I do not make a big point of this because any replication of an anomalous nuclear reaction is part of the general field and **useful to obtain attention."**

Makes me feel used. My research is not just to “obtain attention” for you, Ed or for the excess-heat claims -- what an amazing, unscientific attitude you portray here.

PS – when will you return the BYU equipment I loaned to you years ago, during your visit to BYU?

The emphasis (bolding) is not mine; it was in the posted message. I was nor aware of personal conflicts within the CMNS community till last week. But that deserves a separate units.

Ed Storms wrote:

Sorry to hurt your feelings, Steve. As you well know, an understanding of CF is still in the future. While I have my opinions, these are not shared by everyone. My opinion that you are not observing what I believe to be cold fusion is not as important as the fact that you are seeing something anomalous, which I acknowledge brings attention to the field. I fail to understand why you think an acknowledgment of such attention is unscientific. Conventional physicists do pay attention to what your work, which helps the field. However, it remains to be determined how your work relates to CANR. On the other hand, should I get my feelings hurt because you don't think my published excess heat measurements are important, least of all to demonstrate CANR?

As for the power supply you loaned to me, I will return it when you want it back. It has been very useful and I'm grateful for its use when I had nothing.

Ludwik Kowalski:

It seems that Ed is saying that only D + D --> ^{4}He should be called cold fusion; events leading to n+^{3}He and p + ^{3}H (tritium) channels do not belong to CMNS phenomena. If one accepts this point of view then generation of ^{4}He is just another case of transmutation and CF is no longer a subfield of CMNS. That would be ironic.

Steven Jones:

Look, Ed, I do not think your published excess heat results are unimportant -- my personal interest remains however in the nuclear particle measurements as a better window (IMO) on what is going on sub-microscopically. I'm glad to hear the power supply has been useful; you may continue to use it for your research. Hope all is going well for you in New Mexico -- I haven't been there for a long time, but love the place! Hopefully I'll bring my wife back to Sante Fe sometime.

Ludwik Kowalski:

If I understood him correctly Ed thinks that fusion events leading to n+^{3}He and p + ^{3}H channels are due to static charges in the microscopic surface cracks. Electric fields created by these charges accelerate deuterium ions and this leads to fusion. This calls for some questions:

1) Is Ed's "fusion in cracks" an assumption or is it a fact?
2) If it is a fact then what is the experimental basis for it?
3) What evidence does Steven have to rule out a possibility of cracks fusion?

Ed Storms wrote:

You have the essence of my opinion, Ludwik. However, the effect can be initiated by additional mechanisms other than acceleration produced by static charge. This mechanism has been explored in the past and claimed to be insufficient, at least using conventional theory. The other mechanism, which has not been explored, is the effect of unbalanced charge in the surface that can produce large local voltage gradients. This mechanism has also been applied to the surface of cathodes in F-P cells. Even though the mechanism is still not understood, people who report low level neutron and energetic particle emission have conditions that would be expected to produce cracks. In addition, these emissions are at least 12 orders of magnitude smaller than the nuclear reaction rates associated with heat production and cold fusion. This large difference, along with production of n and t rather than He, suggests to me that two different mechanisms are operating.

[In other words, the effect is only assumed. How can it be assumed if it "has been explored in the past and claimed to be insufficient" to produce the observed events? What kind of theory was used? My guess is that it was the coulomb penetration formula for the highest possible differenece of potential. That is what Steven will probably write, if he decides to reply. I wish the reply from Ed was more informative. Perhaps somebody else will explains the "unbalanced charge in the surface." I also do not know the meaning of the phrase: the "mechanism has been applied to the surface of cathodes? "]

Steven Jones wrote:

Ludwik and Ed raised again the possibility of "crack" fusion to explain observations of protons, tritons, neutrons we have observed coming from deuterided metals. In the early days, this idea was referred to as "fracto-fusion." However:

1. The metal is still an excellent electrical conductor, so it is very difficult to see how the required large electric accelerating fields could be produced.

2. We actually followed up on this hypothesis several times, by grinding, bending, crushing metal-deuterides, hoping to produce "fracto-fusion" conditions. NO enhancement of particle emission was observed, at BYU or (later) at LANL in joint BYU-Los Alamos experiments.

3. The current experiments which are most successful in this area of energetic charged-particle production are those involving low-energy deuteron beams impinging on metal foils, as described briefly in my previous posts here. I do not see any connection of those experiments to fracto-fusion ideas. The explanation in terms of screening by electrons is most popular at this time, IMO, although it too seems to be insufficient to account for the large effects seen.

Ludwik Kowalski wrote:

1) I do not understand crack fusion. Ed wrote that "the Casmir effect and van der Waals forces are available to initiate nuclear reactions." Can you elaborate, please.

2) Every physics textbook tells us that metallic surfaces are equipotential. If the piece is grounded then the total charge on its surface is zero and the electric field is zero, everywhere.

3) If the piece is not grounded then a net electric charge can be introduced through its manipulation, especially if air is

dry. But that electric field is always distributed is such a way that the vector E is perpendicular to the surface, anywhere. A cavity (crack) is essentially a Faraday cage and the surface charge density in the cage is essentially zero. The only places where concentration of static charges can be high are very sharp points. Thus the term "needle fusion" would probably be more appropriate as "crack fusion" or "fracto fusion."

4) Steven Jones wrote: "We actually followed up on this hypothesis several times, by grinding, bending, crushing metal-deuterides, hoping to produce “fracto-fusion” conditions. NO enhancement of particle emission was observed . . . " . Is this not the best possible experimental proof that the postulated phenomenon does not exist? I would like to know about reproducible experiments demonstrating existence of crack fusion. Is there at least one person on this list who performed a crack fusion experiment? Please share your results.

5) Textbooks also tell us about potential differences due to dissimilar metals. For example, a particle of Zn deposited on the surface of copper can generate a difference of potential of one or two volts (I do not recall the exact number). But that is several orders of magnitude below what is needed to accelerate a deuterium ion to a kinetic energy at which the DD fusion is practically possible.

6) What is experimental evidence that rare events observed by Steven are due to crack fusion?

7) What is experimental evidence that rare events observed by Steven are due to Casimir effect?

8) What is experimental evidence that rare events observed by Steven are due to van der Waals forces?

Edmund Storms wrote:

1) Yes [in the early days, this idea was referred to as “fracto-fusion] and a large literature describing this process is available on www.LENR-CANR.org.

In addition to short lived voltage gradients [see item 1 in Steven’s message above], the Casmir effect and van der Waals forces are available to initiate nuclear reactions at low levels.

2) This [see item 2 in Steven’s message above] has been done by Russian workers. I don't have the citations just now but will find them.

3) [Referring to item 3 in Steven’s message Ed added:] Obviously something is being overlooked.

Ed Storms wrote:

1) The nuclear action occurs only as the crack forms. As it forms, unequal number of electrons and ions are separated at the two surfaces. For a brief time, this produces a charge separation, which can cause acceleration of ions from one surface to the other. This is the usual explanation. However, the crack is so small that Casmir effects can be expected. These might introduce energy into the ions from the vacuum energy. Also, the surfaces contain ions with unbalanced charge, as is the case with all surfaces, so called van de Waals forces. In this case, the surface is pure and free of absorbed ions, except deuterium. This will strongly distort the electron

charge on the D atom or D2 molecule.

2) This [equipotentiality and neutrality of a grounded metallic sphere] is only true in a gross sense. Local charge still exists.

3) Charge on a point [needle] usually exists because a gross charge is concentrated there or because electrons are being added as would be the case in an electrolytic cell or during gas discharge.

4) [Concerning "reproducible experiments demonstrating existence of crack fusion:] It is well known that if LiD crystals are hit with a hammer, neutrons will be emitted. Such work has been published.

5) Concerning Zn on Cu:] Differences in Fermi levels can create a few volts difference between metals, whether they are in contact or not. This is measured by comparing work function values.

6) "What is experimental evidence that rare events observed by Steven are due to crack fusion?" There is none. The expectation is that the very low level of emission is associated with experimental conditions used during the study that are expected to produce cracks. Also, the nuclear products are characteristic of a high energy reaction rather than the low energy reaction associated with cold fusion.

7) "What is experimental evidence that rare events observed by Steven are due to Casimir effect?" None, just a suggestion.

8) "What is experimental evidence that rare events observed by Steven are due to van der Waals forces?" None, just a suggestion.

Ludwik Kowalski wrote:
Thanks for answering all my questions, Ed. I commented: "Every physics textbook tells us that metallic surfaces are equipotential. If the piece is grounded then the total charge on its surface is zero and the electric field

is zero, everywhere.'" To which you replied: "This is only true in a gross sense. Local charge still exists." How can this (violations of Gauss law) be demonstrated?

Denns Cravens wrote:
So if I insert two positively charged D's into a metal where is the charge on the nucleus or on the metal surface?

Ludwik Kowalski wrote:
Good answer, Dennis. Gauss' law does not refer to situations in which a net charge consists of a pair of single particles. But does anybody think that a pair of duterons (or thousand such pairs) would produce observable CF (cold fusion) events? Even a million would not be sufficient to observe particles (such as n and p) generated via CF events. I was not thinking about a single pair of ions.

Suppose an insulated metallic sphere was bombarded with alpha particles. This produced a net charge of one nC. How is that charge distributed? I think that transformation of He ions into neutral atoms is practically instant. The total number of free electrons is reduced by 2*k, where k is the number of intercepted alpha particles. Is there any reason to think that the distribution of free electrons will not be uniform after the bombardment?

Ludwik Kowalski wrote:
Edmund Storms wrote: "It is well known that if LiD crystals are hit with a hammer, neutrons will be emitted. Such work has been published."

1) How well is it known? What was the reported emission rate? Who published papers describing such observations? Did someone on this list observe such remarkable CF phenomenon? My inclination is to think that neutrons were due to cold fusion, unless reality of crack fusion (or Casimir fusion, or van der Waals fusion) is independently confirmed.

2) Unless published papers are already at our lenr-canr library, I would appreciate if someone could send me a copy of the most recent paper. Write to me in private and I will send you my postal address. The phenomenon would be worth verifying, for example, by observing 3 MeV protons with CR-39. I am thinking about cracking crystals with pliers above the CR-39.

3) Steven Jones wrote: "We actually followed up on this hypothesis several times, by grinding, bending, crushing metal-deuterides, hoping to produce 'fracto-fusion' conditions. NO enhancement of particle emission was observed, at BYU or (later) at LANL in joint BYU-Los Alamos experiments." Why were no excess neutrons observed in these experiments? Who should I believe? Are the authors "hammer fusion" papers more experienced in detecting neutrons than LANL people?

4) Those with no experience with detection of neutrons, or with no access to necessary instruments, must rely on credential of researchers reporting experimental results. What else can we do?

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252) A student-oriented project

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Introduction:
1) Suppose that an immersion heater is inserted into a beaker with water. How should thermal energy generated depend on the amount of electric energy used? The answer to this question was provided by James Joule -- one joule of heat for one joule of electric energy. In a well insulated container (to minimize losses due to convection) the temperature would rise linearly if the electric power remained constant, provided no boiling takes place.

2) And, at the boiling temperature, one would expect water to be evaporated at the rate proportional to the wattage of the heater. The latent heat of water evaporation, under normal atmospheric pressure, is 2260 J per gram. The same is expected to be true when a small amount of salt is dissolved in distilled water, to increase its electric conductivity. Salty water, by the way, is called electrolyte. Thus the maximum possible amount of water one can evaporate in 5 minutes, when the heating rate is 450 W, is 59.73 grams. In reality the amount of evaporated water would always be smaller, as illustrated in Figure 1 below, because some thermal energy is always lost via conduction, convection and radiation. That figure, was copied from a paper sent to me recently by Pierre Clauzon, a French scientist from the Laboratoire d'Electrochimie Industrielle at Conservatoire National des Arts et Métiers, Paris.

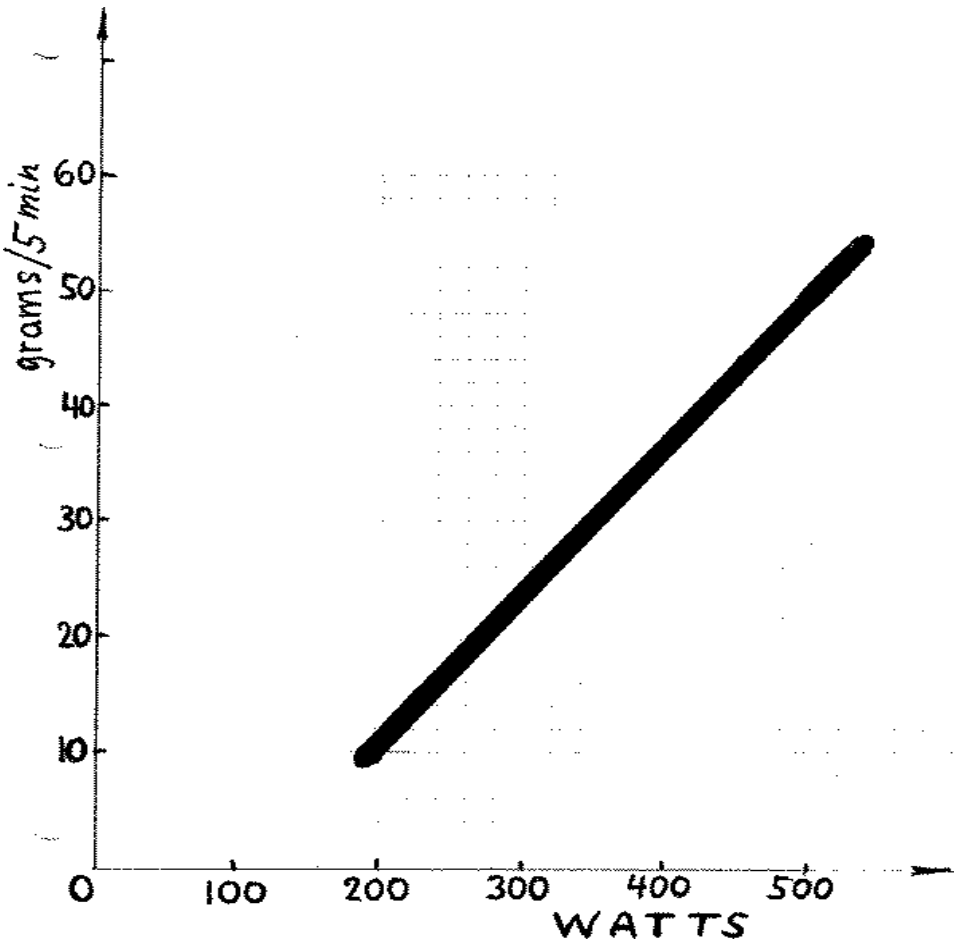

Figure 1

3) There is nothing unusual or abnormal about the black region (thick line) on that figure; it shows where a large number of experimental data points were located. The line thickness gives some idea about uncertainties of individual measurements. But the title of the paper was “Abnormal excess heat observed during Mizuno-type experiment.” Abnormalities were observed when the electrolyte was heated by a different method. Instead of passing the electric current through an ohmic heater the authors of the paper passed it through the electrolyte itself. By doing this they discovered, for example, that to evaporate 38 grams of water in 5 minutes with an ohmic resistor required the heating power of 410 W while only 300 W was required to evaporate the same amount of water in 5 minutes when the current is flowing through the electrolyte. The data point (38,300), as you can verify, is far above the black region in Figure 1. Abnormal experimental data are said to be highly reproducible.

4) How can it be? The amount of evaporated water should be the same no matter how a given amount of thermal energy is delivered to it, provided the same fraction of thermal energy is used to evaporate water. That is the dilemma. And that is why I think that attempts to replicate the experiment are worth the effort. The purpose of this note is to start a cooperative project in which several teachers would be performing essentially the same experiment at the same time. Then we can try to publish a paper that either confirms or contradicts the claimed results. If we can finish this before November 20 then I would be able to deliver this collective paper at an international conference. Schools are likely to be proud of such contributions, especially when their students are also involved.

5) Yes, I know that some might consider this suggestion ridiculous. Why bother if we already know that the claim makes no sense? Let me answer this question. By performing this experiment, preferably with a group of students, we will have a chance to practice scientific methodology. How often do we find a situation in which students can deal, in a meaningful way, with a real controversy? Here we have an opportunity to: (a) promote critical thinking, (b) focus on careful experimentation, propagation of errors, etc. and (c) possibly, if not probably, confirm a scientific discovery. The French paper has not yet been published but the authors gave me permission to write about its content, and to initiate collective students-oriented attempts to replicate the results.

6) My interpretation of such results was that some chemical reactions, taking place in the cell with the electrolyte, are producing heat in addition to electrically delivered thermal energy. In fact, I know that the tungsten rod, through which electrons enter into the solution, is partially consumed during the electrolysis. In an e-mail message to Pierre I wrote that production of heat through chemical reactions would not be an abnormal process. Here is his reply: "I know that my friends at EDF (Electricité De France) have carefully studied these problems and their answers are negative. You cannot explain the abnormal excess energy by chemical reactions with the tungsten rod. I will ask my friend Olivier to maybe elaborate more in this subject. He is a real chemist, not me !" Well, for the time being I will accept this answer; I asked for a reference to a published or unpublished report. I will insert the reference here, as soon as it arrives.

Experimental setups

7) Participants in this research will receive the French paper; do not be scare, it is in English. My anticipated setup is shown in Figure 2 below. It will be a graduated beaker (capacity close to 2 liters) containing a solution of the potassium carbonate salt, K_2CO_3, in distilled water. The concentration of the electrolyte will be 20 grams per liter (0.2 M). The electrodes (tungsten cathode and platinum anode) will have to be somehow mounted on the beaker. An ohmic heater, not shown in Figure 2, will also be mounted on the scale.

8) The first step would be to collect data with the ohmic heater. This will show where the normal data points are located (see Figure 1) for my experimental setup. Each experimental setup will probably produce a slightly different line, depending on the size of the beaker, on the lab temperature, etc. But once established, this line becomes a reference for the so-called "abnormal" results. Naturally, one has to calibrate the ohmic heater connected, for example, to a variac. Likewise one will need a high voltage power supply able to deliver up to 2 A and 600V. A sensitive scale, to support the setup, as in Figure 2, will also be needed. Note that the W cathode is placed in a glass tube; only about 1.5 cm of the cathode is below the lower end of that tube.

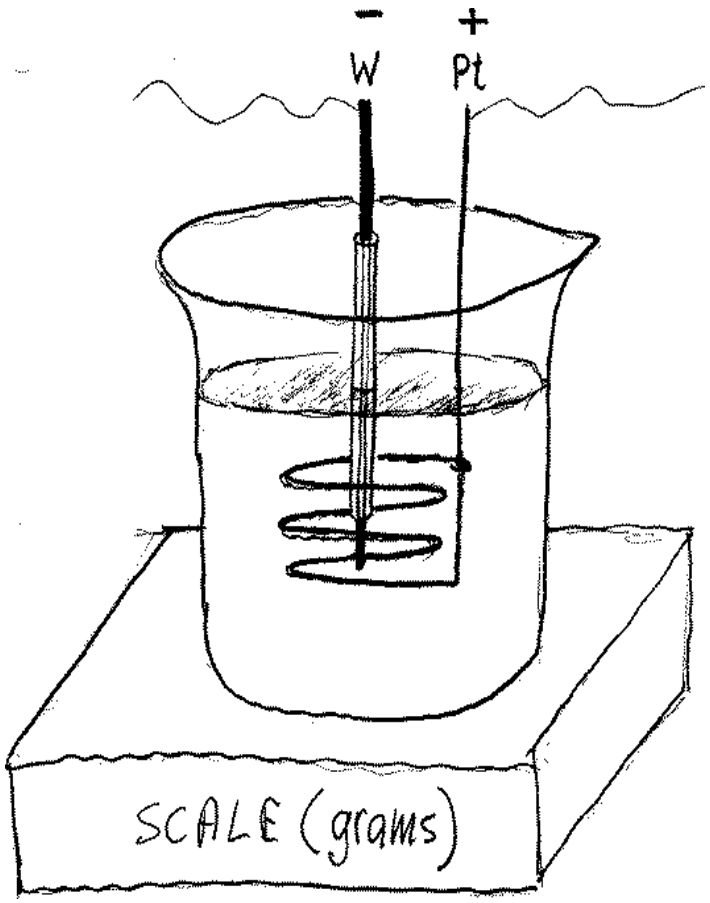

Figure 2

9) The Pt anode will be a cylinder-like spiral wire situated 4 cm from the cathode, as in French experiments. The cathode will be a commercially available tungsten rod used for welding. The water level would have to be the same at the beginning of each five-minute-long test. I do not think that replacing evaporated water during a test (from a container mounted on the scale) is worth the effort because changes in the level of the electrolyte are relatively small. On the other hand, I anticipate problems with water condensation on the walls of the beaker, and with possible splashing, abobe 200 W. Each of these two effects, if not accounted for, can contribute to an apparent excess heat.

10) I know that spectacular underwater glow discharges will be seen around the cathode. That will be associated with significant fluctuations of electric current. But the glow discharge in water is not going to be the object of this study. The purpose will be to determine the relation between the average water evaporation rate on the average electric heating power during the electrolysis. We want to either confirm or to contradict that experimental points are located above the reference line established with the ohmic heater. How will power measurements be made? I have to think about this. Rapidly fluctuating electric current, and arcing in the liquid, are likely to create problems. Please write to me, if you have some suggestions.

11) Be aware that experimenting with electric equipment, especially with a high voltage (and high power) supply, can be fatal if safety precautions are not taken. I will assume you have the necessary equipment and experience. Those who are interested should contact me in private. Let me end this invitation with a message posted by James Mackey, in another context. According to Bridgeman "[The scientist] cannot permit himself any preconception as to what sort of results he will get, nor must he allow himself to be influenced by wishful thinking or any personal bias" This seems wrong. How can one devise an experiment without having some preconceptions about the results expected. How does one decide what to measure without preconceptions about the results. The problem would arise when one allows "wishful thinking" to bias his conclusions based on the experimental results. Everyone has preconceptions about his experiments, so saying that a scientists cannot permit himself any preconception is wrong.

12) My prediction -- call it a preconception or an educated guess, if you wish -- is that the experimental data of Pierre and his colleagues will be confirmed. I also predict that the excessive heat will eventually be explained as due to well known chemical reactions. On the other hand, I remain open-minded toward other explanations. Japanese scientists, like Mizuno, whose experiments were replicated in Paris, are highly qualified chemists. They also think that excess heat released during the electrolysis is too large to be due to chemical reactions. Furthermore, they presented evidence of transmutation of chemical elements. That seems to point toward nuclear origin of excess heat. The controversy about this is not new. Do not miss an opportunity to expose students to meaningful research. They will benefit from working on this excess energy project no matter what the final verdict will be.

Appended on 8/31/05:
What I would like to do, if at least four or five teachers make tentative commitments, is to create a dedicated private discussion list. Informing each other about progress, and addressing problems, should be easier on a discussion list than via regular e-mail messages. I know that many on this list have the necessary experience but no time to perform experiments. Such people are also invited; we will need advisers. Please indicate, in a private message to me, what role do you want to play -- that of a researcher or that of an advisor?

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253) Mizuno-type high voltage electrolysis

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1) In unit #252 I described experiments of Pierre Crauzon and his colleagues. It was based on their recent, still unpublished paper, and on my correspondence with Pierre. In this unit I want summarize what I known about other experiments of that type.

a) Experiments of Mizuno et al. (1997)
b) Experiments of Little et al. (1998)
c) Experiments of Naudin et al. (2003)

Results of Clauzon (excess energy is about 30% of the input energy) seem to be more or less consistent with what was reported in several Naudin's experiments.
Results of Little, however, indicated that the excess heat, if any, is less that about 3% of the input energy. Mizuno, on the other hand, originally reported that excess heat was 1000% of the input energy. In fact, the purpose of nine experiments undertaken by Little was to replicate Mizuno's experiments. All researchers wrote that their results were highly reproducible. That is certainly a strange situation. I also looked at the report that Mizuno presented at the 10th International Conference on Cold Fusion (ICCF10 in 2003). The main point of that paper was the discovery that the excess heat is only a fraction of excess energy; a significant part of excess energy is chemical -- production of hydrogen fuel. The amount of that fuel turned out to be much higher than what one would expect on the basis Faraday Law of electrolysis.

2) I do not have the original Mizuno paper; it was presented at the ICCF7. But his ICCF10 paper is downloadable from the library at <www.lenr-canr.org>. From that paper I see that two independent methods were used to measure excess heat: flow calorimetry and isoperibolic calorimetry. The first has already been introduced in units # 116 - 119. It is highly reliable when the cell is well isolated (no significant excess heat is lost through the surrounding wall. The second method is also quite simple; its reliability depends on stability of calibration. The calibration consists of placing an immersion heater, of known wattage, into the electrolyte and waiting till the temperature stops rising. That gives the first calibration point, for example, 50 W and 47 C. The the heating power is increased, for example to 100 W, and new stable temperature is measured. That the second calibration point. In that way a relation between the heating rate and equilibrium temperature can be established. The curve is plotted and used in subsequent electrolytic experiments.

3) In my opinion the credibility of results reported by Clauzon would go up if the flow calorimeter were used. On the other hand, I see nothing wrong in calculating the average heating rate from the average rate of evaporation. Simplicity is a great advantage, especially in educational context. Naudin also used that simple method. It is hard to believe that large discrepancies of conclusions are due to systematic errors in measuring excess heat produced at the rates higher that 50 W. Measuring rates at which electric energy is supplied to an electrolytic cell is more tricky. But I do not believe that a discrepancy of several orders of magnitude, between Mizuno and Little, is due to a systematic error in measuring electric energy.

4) So what can possibly cause the discrepancy? That is a very difficult question. As usual, one is inclined to think that

an essential success-enhancing ingredient was present in the W cathode used by Mizuno but absent in the cathode used by Little. Ed Storms call the mysterious ingredient NAE (nuclear active environment). He believes that CMNS phenomena will become reproducible everywhere after the nature of the NAE is recognized. But in the experiments #7 and #8 Little used the cathode sent to him by Mizuno. And the results were the same as in other experiments -- the average electric input power and the average thermal power were essentially identical. The controversy remains unresolved.

The only way to make progress, in my opinion, would be to bring Mizuno to the laboratory of Little (to repeat Little's experiment together) and then to bring Little to the laboratory of Mizuno (to repeat Mizuno's experiment together). Would they discover an error in at least one experiment? If not then a tentative conclusion will be that the NAE is linked to geography. But that would lead to other contradictions; many successful attempts to generate excess heat were made outside Japan, for example, in China, Russia, Ukraine, Israel, Italy, France, Canada and the USA.

Let me finish this unit by mentioning that Naudin's experiments were reproduced by other researchers, as illustrated at <http://jlnlabs.online.fr/cfr/>. They are not very different from the experiment of Clauzon and his colleagues. What is remarkable is that no expensive platinum was used in many of his devices. Very large amounts of excess heat were reported with cells whose both electrodes were identical, for example both tungsten or both stainless steel. In other words, two knives or two spoons can be used. Two tungsten rods for arc welding, he told me last fall, are excellent electrodes. It is not difficult to find suppliers of such rods by googling. Let us hope that a massive assault (many teachers, students and other qualified people working on the same kind of experiment at the same time) will help to solve the puzzle.

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254) An interesting theoretical paper

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As mentioned in another unit, I belong to the International Society of Condense Matter Nuclear Science (ISCMNS). That society has a restricted discussion list, called CMNS, for its members. In unit #249 I wrote about one interesting theoretical interpretation of CMNS phenomena. Here I would like to mention another theoretical interpretation. A link to the following webpage was shared, this afternoon, by one of the contributors.

http://arxiv.org/abs/cond-mat/0505026

It points to an interesting paper, dated Mat 5, 2005, of A. Windom and L. Larsen. I am not a theoretical physicist but I have an urge to share what I think can be understood in terms of elementary nuclear physics concepts. Let me begin by mentioning the familiar form of radioactive decay in which an orbital atomic electron is captured by a nucleus producing a transformation of a proton into a neutron. A well know example of such process is the decay of ^{51}Cr into ^{51}V (T=27.8 days). Can a hydrogen ion be transformed into a neutron by capturing an electron? That question is answered positively by Windom and Larsen.

First they remind us that transformation of protons into neutrons has been observed when negative particles captured are negative muons rather electrons. The mass of a muon is sufficiently large to overcome the mass excess of a neutron over a proton. That makes the reaction energetically possible. Transformation of protons into neutrons, by capturing electrons, is energetically impossible; the mass of a free electron is much smaller than the mass difference between neutrons and protons. But it would be energetically possible if the mass of an electron were 2.53 times larger. The authors observe that “the electron mass in condensed matter can be modified by local electromagnetic field fluctuation” on metallic hydride surfaces.

The paper addresses details of this issue in terms of concepts with which I am not familiar. That is why I would very much like to know what quantum electrodynamicists think about the paper of Windom and Larsen. For the time being I will tentatively assume that anticipated mass enhancements of electrons are realistic and that heavy electrons do produce neutrons, occasionally in metals, such as palladium, loaded with hydrogen. The author anticipate that neutrons produced from protons will have very low energies. At those energies capture cross sections are very large. For that reason, they speculate, only a very small fraction of neutrons should be experimentally observable.

One can argue with this. Most neutrons would indeed be absorbed (via nuclear reactions) if they were generated in a thick metallic sheet. But that expectation does not seem to be consistent with the idea that most neutrons are produced at metallic surfaces. The explanation of helium production, via equations (26), makes sense to me but production of ^{6}Li, via equations (28), is much less likely. This has to do with the extremely short half-life ($2*10^{-21}$ s) of ^{5}He. That nucleus will emit a neutron long before it is hit by a second neutrons produced from a proton. And I do not know how to consolidate the suggested mechanism with frequently reported absence of gamma rays. Pd isotopes, for example, absorbing neutrons, would produce several gamma radioactive isotopes of silver. Likewise ^{181}Ta and ^{58}Ni would produce gamma radioactive ^{181}W and ^{58}Cu, respectively.

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254) Excess energy; progress report

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1) In this unit I plan to write about reports, comments and reflections related to the project described in the unit #252. Two discussion lists were used to announce the initiative: PHYS-L (~200 subscribers, mostly physics teachers) and CMNS (~ 150 subscribers, members of the International Society of Condensed Matter Nuclear Science). Most members of that society either conduct or conducted research in three areas of CMNS: cold fusion (CF), cold transmutations (CT) and excess energy (EE). Except for myself (LK), I will refer to authors of comments as either T (teachers), S (students) or R (CMNS researchers).

2) Here is the first message on PHYS-L; it was posted by me yesterday.
Dear colleagues: Please read what has just been posted at my website as:

http://csam.montclair.edu/~kowalski/cf/252clauzon.html

It is an invitation to participate in a collective student-oriented project. The idea of turning what I wanted to do into a collective Internet project was prompted by the ongoing debate about scientific methodologies. Give your students a chance to participate in a research project they can easily understand.

3) Here is the first message on CMNS; it was also posted by me yesterday.
The following has just been posted on a discussion list for science teachers (PHYS-L): . . . [see item 2 above]. I would also like to reach people who are not on the PHYS-L list. Some of you might know teachers who are qualified to participate in this research project. If so then share the above URL, and my e-mail address, with them. By the way, it would be great if you could assist them. Please comment on the idea of organizing easy-to-set-up Internet-based CMNS projects.

4) So far I had only one person commitment. A physics professor T1 wrote:
"We would like to participate. One or more students from my experimental physics class (senior physics majors) will set up and perform the experiment. Thanks for the opportunity."

5) Several interesting comments were made on the CMNS list. Here are extracts from their messages:

R1: ". . . I do not think experiments of this nature will produce definitive evidence, and I doubt it will change the minds of any of the professional skeptics. I expect this work will be like the experiments conducted by high school students working with John Dash: interesting, worthwhile, thought-provoking, but not fully convincing. The students are smart cookies and they deserve an A for effort. The experiments are excellent publicity for cold fusion. They attract young people, which the field desperately needs. But most researchers I have spoken with feel that the instruments are too crude to be convincing. .....I hope that some of the leading researchers in the field, such as McKubre, Storms, Dash and Mizuno, will lend a hand and offer advice directly to the participants."

R2: (who was not able to confirm excess heat in similar experiments): " .....I applaud the idea of making the

experiments public via the Internet. I think it will go a long way towards getting to the bottom of this particular phenomenon. As you can see from our 'Experiments" pages [several links were provided] .....we have tried a variety of closely-related experiments and never observed the excess heat reported by other investigators. ..... Since the heat output is relatively easy to measure in these experiments (especially in the Clauzon embodiment), this means there is a high probability that the other investigators are either getting real excess heat or mis-measuring the electrical input power. Due to the highly erratic nature of the current flowing in this experiment, the latter is a very real possibility.

I therefore urge all participants in this experiment to focus on the accuracy of their electrical input power measurement. In lieu of purchasing an expensive power analyzer (such as our Clarke-Hess 2330), experimenters should consider using an ordinary residential watt-hour meter (readily available on Ebay for ~$25). The smaller of these meters have low Kh values (watt-hours per revolution of the dial) and finely graduated dials so they can be used to measure pretty low energy values accurately. For example, half a revolution on a Kh=1.8 meter, would be 0.9 watt-hours or 3240 joules, which is the energy delivered by a 324 watt source in 10 seconds -- this is well within the range of interest in this experiment.

I look forward to seeing the results of these experiments. Because of our history with this experiment we will wait and see how things turn out....for now. If several groups obtain positive excess heat results and double-check their electrical measurements, naturally we will drop everything and join the party immediately."

R3: ". The discrepancy between Mizuno's results and Little's are a mystery. I do not think progress toward resolving this mystery will be made until Little and Mizuno spend several weeks working together, perhaps using some of both sets of instruments. Mizuno reportedly has a great deal more space these days in his new lab, so it should be easier to work with him. "

R4:" LK asked: 'Can a chemist tell me how much thermal energy is released when one gram of tungsten is totally oxidized. A sentence or two explaining the calculation would also be appreciated. Quick and dirty reply, W has an atomic mass of about 184 so 1 gram is 1/184 moles or about 0.0054 moles. Assuming it goes to WO2 with O2 that is about 140 Kcals /mole. That is about 0.76Kcals."

LK: "Thanks. That translates into 3181 joules. Dividing this by 300 s (for a 5 min. test) one gets only a 10 W contribution to excess power. According to Clauzon, experimentally measured excess powers are often higher than 100 W. To explain 100 W one would have to assume that 10 grams of tungsten "fuel" is consumed in 5 minutes. That is about 0.5 cc. I do not know by how much the mass of the cathode was reduced in experiments in which excess power was 100 W. I suspect it is no more than 2 or 3 grams; but that is only a guess based on what I know."

R4: "If I understand correctly, tungsten is the cathode where hydrogen is produced, not oxygen. Therefore, the tungsten can not oxidize. Loss of material from the cathode is caused by other reactions including vaporization when a plasma is used."

R3: "Yes, but LK asked about one gram of tungsten being totally oxidized. It may or may not be in an electrolytic system. The W may or may not be a cathode since some of these are run with AC. "

R4: "True, but this is an important fact anyway. In addition, if W is run as the anode, it quickly oxidizes and disappears. Use of AC gives some protection because the oxygen activity at the anode does not get as high as would when using DC."

LK: "R4 makes a good point that oxidation of tungsten is probably not the main cause of its gradual consumption. I was thinking about the worst possible scenario. Even with that scenario the reported excess power is too high to be due to chemistry..... "

R2: " . . . The formation of tungstic acid is exothermic and yields 270.5 kilocalories/mole...BUT, the dissociation of 4 moles of H2O into the H2 and O2 needed for the formation of H2WO4 is endothermic and requires 4 x 68.3 = 273 kilocalories.... an almost perfect cancellation."

R4: “T2, I suggest the H2WO4 was not made by electrolysis but by hot W reacting with steam in the bubbles. Of course, this would produce energy and extra hydrogen, but not from the electrolytic process. The electrolytic process is too reducing at the cathode for H2WO4 to form - a small point that might help clarify thinking about the process.”

R2: “I don't see the "produce energy" part. It looks like a slightly endothermic reaction to me 270.5 kC/mole for the formation of H2WO4 but it costs 273.2 kC to get the 4H2O dissociated into H2 and O2. Net heat of formation under these circumstances: -2.8 kC/mole (here the minus sign signifies the reaction consumes this energy. not releases this energy. Yes, I know that's contrary to conventional heat of formation sign conventions but I'm a physicist not a chemist.).”

R4: “I don't have the numbers handy, R2, but this reaction would only go above about 1000° C. Are your heat values for this temperature. Such a temperature would be expected to occur for brief times on the surface of the tungsten in contact with the plasma. In any case, such a reaction would not go unless it is exothermic.”

6) I see three very distinct aspects in this project. First is to ether confirm or contradict experimental facts reported in the French paper. Is it true that less electric power is used to evaporate a given amount of water in five minutes via high voltage electrolysis than via ohmic heating? In other words, is it true that the experimental data points (rate evaporation versus electric power, as in Figure 1) from high voltage electrolysis can be located above the data points from ohmic heating? The second aspect is to interpret experimental results, after they are recognized as valid. Can the difference between the two sets of experimental data be explained in terms of well known chemical processes or not?

The first task is relatively simple; both physicists and chemists are trained to deal with it. The second task seems to be much more complicated because it calls for the expertise in analytical chemistry. All major chemical reactions, taking place during the high voltage electrolysis, must be recognized and their thermal outputs must be calculated from enthalpy tables. If the existence of excess energy is confirmed, and if that energy cannot be attributed to known chemical reactions, then the situation is really abnormal and worth of additional investigations. The third task would consist of numerous projects designed to identify the abnormal phenomenon, to explain it in terms what is known, and to explore possibilities of practical applications. One particular “abnormal” phenomenon comes to my mind -- I am thinking about the first transformer. An electric current was discovered (Faraday, 1830’s) in a circuit not connected to a battery.

Appended on 9/1/05:

7) This is my reply, posted on the CMNS list, to what R2 wrote yesterday, about kWh measurements. “Commercial kWh meters are probably designed to operate when volts and amps change sinusoidally. The high voltage electrolysis cell is an arcing and sparking load. Therefore the instrument must be calibrated; otherwise honest skeptics will have good reason not to take the reported excess heat seriously. Please comment on the following way of calibration (or suggest a better way).

A small cell with a cold electrolyte is immersed into a high school calorimeter (a double-wall aluminum cup) and the high voltage is applied to electrodes. The electric energy, taken from the a.c. 110 V outlet, flows through the kWh meter to the HV power supply and then to the cell. That is the anticipated setup. At time zero the switch is turned on to start arcing and sparking. The switch is turned off when the temperature of the electrolyte becomes, for example, 60 C, as monitored with a thermister. Then the kWh are recorded. The amount of heat received by the calorimeter is calculated (from the change of its temperature, the amount of water in it, etc.) The kWh meter can probably be trusted to measure the energy needed to operate the power supply when the load is disconnected.

Suppose that the electric energy needed in two minutes, when the load is disconnected, is 1 kJ. Also suppose that during two minutes of arcing and sparking the measured electric energy was 36 kJ (10 kWh). That would indicate the net electric energy of 35 kWh. Suppose that the calorimeter shows that ~35 kJ of thermal energy was delivered to the cell at the same time. That would mean that the kWh recorded by the electric instrument are reliable. On the other hand, suppose that the thermal energy delivered to the calorimeter was found to be 20 kJ. That would indicate a correction factor of 35/20=1.75. That factor would have to be used to convert the kWh read by the electric instrument into true input energy. The accuracy of measuring thermal energy with a high school calorimeter is not great (~5%) if

one is careful. But such instruments are probably sufficient to avoid larger calibration errors."
P.S.
As I read my own message, thinking critically, I see need to specify that electrodes used to calibrate the kWh meter should be nonmetallic, for example, carbon rods. Why is this important? Because a metallic cathode, especially Pd, is believed to promote releases of thermal energy from nuclear reactions. My anticipated method of calibration is based on the assumption that thermal energy and electric energy are equal (during a given time interval). Is this an acceptable assumption when carbon electrodes are used? If not then a better way of calibration is needed. Do you agree that the recalibration of a commercial energy-meter is essential when the load is an arc or glow discharge?

P.P.S.
This is not the whole story, unless the cell was sealed (to be totally immersed inside the calorimeter), and contained a recombiner of hydrogen and oxygen. Without the recombiner chemical energy produced (formation of hydrogen and oxygen from water during the electrolysis), would have to be added to the reading of the calorimeter. Like heat, chemical energy, produced during the calibration, comes from electric energy.

How much chemical energy is produced in 5 minutes when the electric current is 2 A? The total charge is 2*5*60=600 C. According to Faraday Law 96500 C will produce one mole of atomic hydrogen. This in turn becomes 1/2 mole of molecular hydrogen (one gram of H_2). The heat of combustion of hydrogen is 143 kJ/gram. In other words, production 1 gram of H2 fuel, via electrolysis, will use the electric charge of 96500 C to store 143,000 J of energy. This translates into 1.48 joules of excess energy per coulomb. Mizuno called it "heat of formation." For I=2 A and t=5 min the charge is 2*5*60=600 C. The corresponding chemical energy is 1.48*600=889 J, or nearly 0.9kJ. This is about 1% of the excess energy reported by Clauzon and his colleagues. In other words, excess heat would not increase significantly if production of chemical energy was taken under consideration. But the total immersion of the calibration cell, in the high school calorimeter, is important, as in bomb calorimeters used to rate bread, butter, sugar, potatoes, etc.

R2: "I suggest we use a residential watt-hour meter for one of the instruments and a pair of DMM's for the other instrument. One DMM [digital multi meter] can measure the voltage across the cell and the other, perhaps with some filtering, can register the average current through the cell. Since the average cell current often changes significantly throughout a run (due to erosion of the cathode), the experimentalist will have to discipline themselves to record voltage and current at regular intervals throughout the run and then add up the total energy delivered. BTW, residential watt-hour meters can handle significantly non-sinusoidal waveforms. They must do so in order to accurately meter the electrical energy consumed by motors, light dimmers, etc.

R4: "Carbon is not good, LK, because CO is made at the anode and CH4 is formed at the cathode, thereby contributing extra chemical energy. Clean Pt is the best material for both electrodes."

LK: "Pt is in the same group as Ni and Pd. But I will take your advice, R4. By the way, those who are very skeptical about CMNS phenomena will not object to the calibration on the basis that "nuclear energy might possibly be released on the metallic surface." They think that this is impossible. To convince myself (that calibration is correct) is much more difficult. I agree with R2 that the level of confidence will go up when several methods of calibration give nearly the same result.....Something has to be trusted as one goes from what is known to what in not known."

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256) On measuring electric energy

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1) What can be more simple than measuring electric energy delivered to a resistor at constant current? Measure the current, measure the voltage, multiply amps by volts (to get power in watts) and multiply by time in seconds. This gives energy in joules. The same is true of common alternating current, for example, at 60 Hz. Most voltmeters and ammeters show so-called effective values and that is what is needed to calculate the power in joules correctly. Electric power meters, calibrated in watts, can also be used in d.c. and a.c. circuits. Furthermore, electric energy meters can be used. Nearly every house has an energy meter; cost of electricity is usually calculated on the basis of such instruments. (They display energy in kWh; 1 kWh=3.600.00 J).

2) But what about a case in which an electric current is highly irregular? I am referring to an experiment described in unit #252. Two days ago I received a message from a German engineer, XX, who read that unit. He suspects that at least some researchers did not measure electric energy properly. He wrote: “

“. . . Some comments in #254 shows that I'm not the only one which sees the measurement of electrical energy consumption as one of the keys to this experiment. On the other side there seems to be some believe that if one take two multimeters and take the average I and U one could easily measure the electrical power of the experiment. At that point I want to stress that doing so could easily generate errors of nearly any magnitude. I will give you and example. Take the case of a pulsed waveform. Let us assume a pulse width of 1/10s and a pulse voltage of 1V. The current which is forced by the pulse of 1V should be 1A . The pulse should be repeated every second. So how can I calculate the energy used in 100s? It is really simple. Is it?

Method ONE (I'm sure the correct one): The energy of one pulse is 1A*1V*1/10s = 1/10Ws. In 100 second we have 100 pulses so we have a consumption of 100*1/10Ws=10Ws= 10J.

Method TWO (I'm sure the erroneous one): Let us calculate the energy taking the values of our two multimeters. The voltmeter in our case shows us a value of 1/10V and the ampere meter shows us and value of 1/10A. If we now calculate the energy simply by using U*I*t we get 1/10V*1/10A*100s = 1Ws= 1J. We have on error of factor 10 (exactly the inverse of the 1/10 duty factor of our case).

Now you can think what's the point of this discussion? The point is that I have found some experiments I assume exact this kind of error has happened. One of them is in an experiment from Kanarev you mentioned in #172. Here is the link to the description http://www.newpowers.org/heat_generator_test.html. At the end of the paper the calculation of consumed energy is discussed and Kanarev insist on the calculation like method two. Because duty factor in that experiment is small he get huge excess power. Also some people from SITIS (?? obviously some guys he cooperate with) made him aware of the wrong calculations he argued that his method is correct. I have to admit that I'm a little bit surprised at this. Everybody can make errors but a professor should realize such an error at least after he was point the error. The other one I found was on the homepage of Naudin you mentioned also in 172 and 174 (I want to note that I like his homepage) in his experiments with MAHG. There I assume the same error happens. He calculates the energy consumption of his pulsed system with values reading from his two RMS multimeters. At the least he should have been skeptical after looking on his own results http://jlnlabs.imars.com/mahg/tests/maghwr.htm. The COP factor

scales nicely with the inverse of the duty factor. It is exactly the error one get if using method TWO.

I write this message not to accuse somebody to spread false claims or do a bad job I do not even totally exclude the chance that my thoughts in this email are based on an error of myself also I strongly belief that I'm right. I write this long message only to make interested people aware of the fact that using the values of two multimeters (even if they are true RMS multimeters) to calculate the energy consumption of an experiment could result in huge errors. The size of this error depends strongly on the waveform used during the experiments. Please do not hesitate to comment my email even if the comments should be critical.

Replying to the above I wrote that arcing and sparking produces highly irregular current. This makes energy measurement even more difficult that for the pulsed waveforms described by XX. Fortunately a simple calorimetric method can often be used to calibrate the system. Here is an illustration. Produce an irregular current by inserting a vibrating contact (like in a Tesla coil, or in a door bell) into a circuit containing your resistive heater. Immerse this heater into a calorimeter and turn the power on, for example, for 300 seconds. Suppose the heat capacity of your calorimeter (including the cup and the heater) is 500 cal/C = 2093 J/C. Suppose the temperature goes up by 30 C in 5 minutes. Then the average power (rate of heating) is 2093*30/300 = 209.3 J/s. Your electric power meter is reliable if the average power is close to 209.3 W. The calorimeter tells us that 209.3*300 = 62,790 J of thermal energy was delivered and that should agree with what is measured electrically.

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257) Questioning a statistical protocol

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Units 240 and 241 were devoted to a paper of Robert Bass. Two days ago he sent me a new version of that paper; that version will appear in the ICCF10 proceedings. I see that the second author, Michael McKubre, was added. I also see my name mentioned at the end; I am glad than my critical comments were useful. That encourages me to revisit the long-forgotten topic and to comment on the new version. Here is the message sent last night to Robert Bass:

= =

1) Thanks for sending me Your ICCF10 paper, and for mentioning my name. I read the protocol and the description was clear to me. In the example of 5 data points (plus blank that is always 0,0) you found that rho was between 1/3 and 1/2. The 95.4% level of confidence was assigned to this experiment (to this set of 5 data points) on that basis.

I am familiar with the concept of level of confidence when samples of limited size are drawn from large populations. For example, suppose that heights of 100 of randomly selected students, in a large university, were measured and the average was 167 cm. A statistician might say that, at the level of 68% confidence, the true average height, for the entire population, is between 163 and 171 cm. Or she might say that, at the level of 95% confidence, the true average height is between 159 and 175 cm. To validate the protocol I could, at least in principle, collect 1000 random samples (from the same population) and count how many samples are consistent with a prediction. For the 68% prediction to be valid, about 680 random samples must yield means between 163 and 171 cm. The protocol (or the assumed randomness of selections) would be shown to be wrong, for example, if only 250 mean values were in the predicted range.

But your 95.4% level of confidence applies to the statement that "the relation is linear." It does not apply to a number that is confined to a specified range. Suppose the experiment is repeated 1000 times (producing 1000 sets of five slightly different data points). What should happen to confirm your statement? What should happen to contradict it? In other words, what does the 95.4% level of confidence refer to?

P.S.
You can DEFINE a rectangle whose sides are 2 cm and 3 cm. Then you can say that the area is 6 cm^2, with 100% confidence. Or, after MEASURING two sides, you can say that the area does not differ from 6 cm^2 by more that 1%, at the level of 90% confidence. But saying that the MEASURED area is 6 cm^2, at the 90% confidence, for example, is meaningless. Why is it so? Because it can not be verified by performing a finite number of experiments. Suppose the mean value, after 1000 measurements, becomes 6.09. That would contradict the 1% range. But the range was not specified in the last statement. Therefore results like 6.09 or 5.9999 neither confirm nor contradict it. As you know, statements that are not falsifiable are not acceptable in experimental science. That is why, I am questioning the statement about linearity, in your modified ICCF10 paper.

= =

2) The reply will be appended here, after it arrives. For the time being I will assume a range of values about which the confidence level was 95.4% will be specified. This will offer me a possibility to test the protocol by simulating

experimental data with a Monte Carlo code.

= =

3) Bass and McKubre wrote:
"It is universally accepted, even by nonscientists, that if the measured output from a physical system is double, triple, or quadruple that obtained when the measured stimulus/input is doubled, tripled, or quadrupled then there is a "cause and effect relationship" between the input and output (e.g. total energy input versus excess energy [or nuclear ash] output in a cold fusion experiment)."

Deciding what is stimulus/input for a given output is not always easy. Two quantities measured in a given experiment my be proportional to each other without being part of a "cause and effect relation." Suppose that temperature and pressure of atmospheric air are measured for several hours. A correlation, between p and T, even linear, is discovered. Which variable is the input and which variable is the output? It is not at all obvious that one variable affects the other; both might be influenced by something else. The same is true in the case of burning fuel. Is the energy released caused by ash or is accumulated ash caused by released energy? Both are byproducts of a complex process. The same can be said about the weight and height of a growing child. I think that the reference to the "cause and effect" is totally unnecessary; the main purpose of the paper is to offer a tool for testing linearity between two experimentally measure quantities.

4) The simplest, well known, approach consists of plotting the data points with error bars. Then one can see how a straight line fits the data. Also well known is the linear regression analysis of a scatter plot. It offers the "best possible" straight line and the correlation coefficient. That coefficient gives a general idea about how the actual data points are scattered on both sides of the best line. The authors of the paper, however, want to go beyond this. They want the "level of confidence." I already wrote that this is not possible unless a region in which the straight line is expected to be located is also specified. Without this their rho parameter is just another useful indicator of the "goodness of fit."

5) But can experimental data in the area of CMNS be processed in the same way as in an area in which experimental data are reproducible? In my opinion the CMNS field is not ready for the "level of confidence" analysis. But I will ignore this fact and try to evaluate the protocol presented by Bass and McKubre. That protocol is straight forward. Given N-1 data points, and an additional (0,0) point, called "blank," one calculates a single parameter, rho (Greek symbol in the paper) and uses it to determine the level of confidence. More specifically, the level of confidence is 68.3%, 95.4% and 99.7% when rho is less than 1, less than 1/2 and less than 1/3, respectively. It would be useful to have a table in which the range of levels of confidence were broader (for example, 50% to 100%) and steps in rho were smaller (for example 0.05). What surprizes me is that the outcome does not depend on the error bars assigned to individual data points. To test the protocol I will use the following set of six data points:

(0,0), (1,3), (2,4), (3.6), (4,11.9) and (5,13.8).

For these points, according to Figure 1, rho is between 1/3 and 1/2 and the level of confidence is supposed to be 95.4%. I will take this statement for granted and use the Monte Carlo method to determine its validity. In doing this I will assume that the first variable, in each pair, has a negligible random error. The second variable, however, in each data point, will be assigned an error bar, as explained below. The confidence level of 95.4% implies that in 10000 Monte Carlo trials (based on the above data points) at least nearly 9540 should produce regression lines confined to the specified region. Agreements with expectations will be labeled as YES. For example, if YES is 3500 then only 35% of experiments produce regression lines in the specified region.

. . . . How many YES out of 10,000 ?
Case 1: vertical error bars (sigma = 0.5%) ....>
Case 2: vertical error bars (sigma = 5%) ....>
Case 3: vertical error bars (sigma = 10%..................... >
Case 4: vertical error bars (sigma = 20%..................... >

Added on 9/17/05:

6) My Monte Carlo code is ready but Robert did not reply. Perhaps he is away, perhaps his computer is not working. I hope he is not sick. I suspect the value of rho is linked, somehow, with the "width" of the straight line; smaller values of rho refer to wider lines. I can not test his protocol without knowing how to distinguish YES from NO. But I can do the following (just to illustrate the working of my code). Let me arbitrarily assume that the calculated rho defines the width of the region as 5% of the mean expected value. The expected values are those found from linear regression fits. If the regression line, based on a set of six simulated data points is confined to the expected region then YES is incremented. Otherwise it is not incremented.

With such artificial choice the results were as follows:

. . . . . How many YES out of 10,000 ?
Case 1: vertical error bars (sigma = 0.5%) .....> 10,000
Case 2: vertical error bars (sigma = 5%) .....> 2,958
Case 3: vertical error bars (sigma = 10%..... > 335
Case 4: vertical error bars (sigma = 20%..... > 28

The fact that the vertical error bar has a decisive influence on the number of YESes is not surprising. This is likely to happen no matter what criterium is used to identify YESes. Validity of the protocol will always depend on vertical "width" of the regression lines (region of confinement) and on the vertical error bars, as intuitively expected. The weakness of the protocol is that it does not take into account bars of error and that it does not define what is and what is not a linear relation between two experimentally measured quantities.

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258) Sampling errors are not likely to occur

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An interesting discussion developed between people planning to replicate the experiment described in unit #252. The voltage applied to the cell, v(t), will be essentially constant but the current, i(t), will be highly irregular. They are interested in measuring electric energy delivered to the cell during a test of a known duration. Consecutive values of i(t) will be determined by an instrument performing digital sampling. That energy will be calculated as a sum of consecutive dE = i(t)*v(t)*dt, where dt is the time interval between samplings. The frequency distribution of i(t) is likely to be very wide. Suppose that the sampling frequency is much lower than some frequencies contributing to the shape of the waveform. Can this lead to a systematic error in an attempt to determine the electric energy delivered to the cell? That question is answered below.

Consider a current waveform shown in Figure 1. It has been generated by using random numbers (from a uniform distribution between 0 and 2). The time intervals between consecutive current changes were 0.1 ms, as indicated. It is clear that the spectral composition of that waveform contains frequencies higher than 10 kHz. The mean current, over a long period of time, is 1 A, by design.

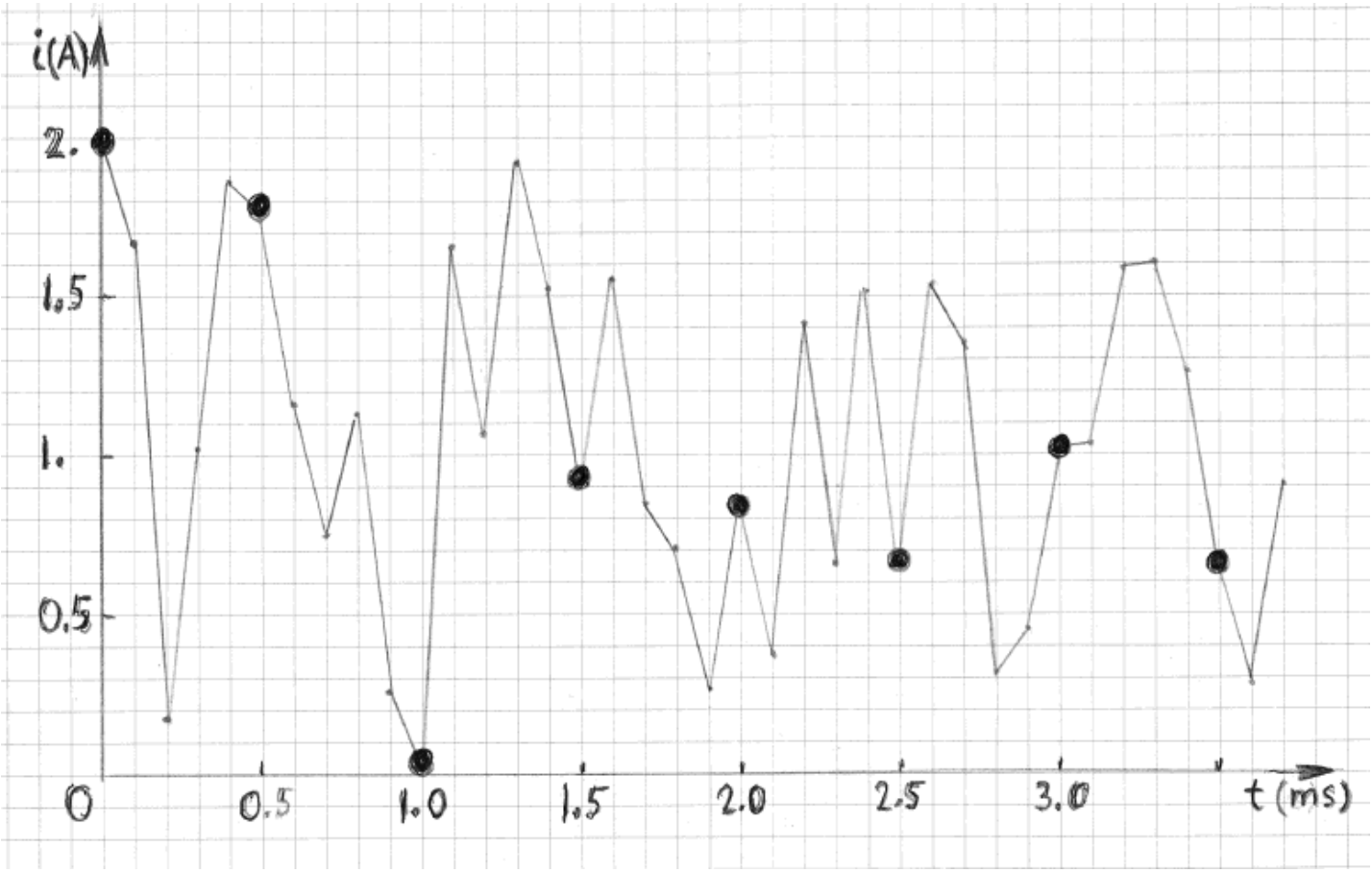

FIGURE 1

Suppose that the shape of this waveform must be determined by sampling. The rate of sampling would have to be considerably higher than 10 kHz , for example, at least two or three measurements during every 0.1 ms. The dark circles show the waveform one would obtain if the rate of sampling were only 2 kHz. This is certainly not a correct representation of the real shape of the

waveform. But the situation becomes dramatically different when the purpose of sampling is to determine the mean current over a long time interval, for example between 0 an 20 seconds. In that case the mean value from the true waveform and the mean value from the circled waveform would be practically identical. This fact was numerically verified by using a computer code. The six mean currents, obtained by executing the code six times were: 1.000, 0.999, 0.993, 1.001, 0.992 and 1.004 A.

In other words, the sampling frequency necessary to determine the mean current can be much lower than what would be necessary to determine the shape of the signal. The only requirement, to obtain a reliable mean value, is to gather a sufficiently large number of samples during the experiment. This conclusion is intuitively obvious for any random waveform. The situation, however, can be different for a periodic waveform, as illustrated in Figure 2. In that example the sampling interval -- 1 ms -- is the same as the period of repetition. The apparent mean current would thus be 0.2 A; that is several times smaller than the true mean current.

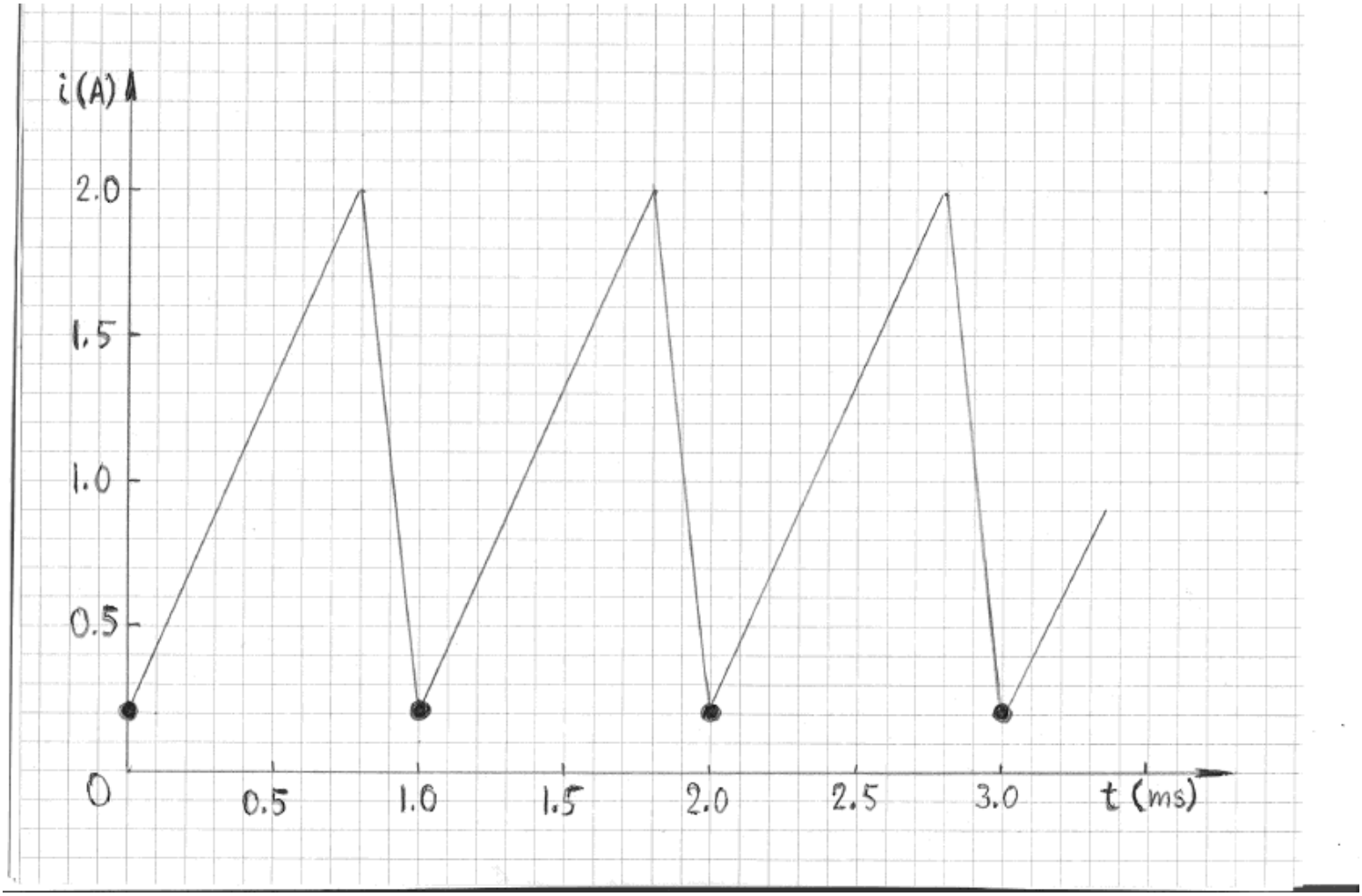

FIGURE 2

Note that the apparent mean current of 0.2 A would occur only at sampling frequencies of 1 kHz, 0.5 kHz, 0.25 kHz, 0.125 kHz, etc. At all other sampling frequencies the apparent and the true means would be nearly identical, provided the sampling interval is negligibly small in comparison with the duration of the waveform. In other words, an error due to a poor choice of sampling frequency, for a periodic waveform, is possible but not very likely. To eliminate possible systematic errors periodic waveforms must be sampled at randomly distributed intervals (rather than at equal intervals, as in Figure 2). using consecutive sampling at two different frequencies, such as 13 kHz and 17 kHz, and showing that the means are nearly identical, would be a good indication that a sampling error did not occur. Another way to gain confidence would be to connect two ammeters (sampling at different frequencies) in series. Nearly identical readings would indicate that an error due to sampling did not occur.

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259) What is this experiment about?

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Next week I will be working with Scott Little and Gregory Luce an ETI (Earth Tech International, Inc.) in Austin. The purpose will be to replicate the experiment described in unit #252. In the past Scott tried to confirm excess heat in similar experiments (performed by Mizuno and then by Naudin) but all results were negative. He thinks that this attempt to observe excess heat will also be negative. On the other hand, in a message sent two days ago, he wrote: "If we confirm the excess heat effect, Ludwik, I guarantee you we will try EVERYTHING to see what makes it tick. ..!!!! " At the end he added: "Let vision of fame and glory not be the enemy of accurate measurements." I like this kind of attitude. Each of us would be happy to confirm excess energy but we want to be objective.

To describe our experiment to a person familiar with high school physics I would begin by asking a question. What is the maximum possible amount of water that can boil out of a beaker in 5 minutes when an electric heater of 400 W is immersed in it? Then we would calculate the electric energy received by water, E=400*5*60=120000 joules. Dividing this by 2260 the answer would be 53.1 grams. That is because 2260 J/g is the well known latent heat of evaporation. In reality less water would be evaporated, perhaps only 40 grams, because not all heat is lost via evaporation; some of it is lost via conduction, convection and radiation.

Suppose that somebody performed the described experiment and reported that the amount of water evaporated was actually 70 grams. "What would you think about such report?" I would ask. Then we would discuss two possibilities: a hidden experimental error and a hidden source of thermal energy. The third alternative, that the law of conservation of energy is not valid, would not even be discussed in our context. Such topic should be discussed at much higher levels.

An experimental error, either on electrical or on thermal side of the procedure, would certainly be the first thing to suspect in an experiment performed by a student. But in this case the anomaly (more water evaporated than allowed by the law of observation of energy) is reported by top scientists. P. Clauzon is a retired nuclear physicist who worked on the French breeder reactor for at least two decades, J.F. Fauvarque is a recognized international authority in the area of electrochemistry. We are trying to replicate experiments they described in a paper to be published. Furthermore, Fauvarque et al. were not the first to report the anomaly. Similar reports, from a large number of similar experiments, were published on Naudin website. And Naudin was only confirming the claim made by Japanese researchers, such as T. Mizuno and T. Ohmori. It is not likely that scientists of such caliber, working in different laboratories, could make large experimental errors.

That is why my inclination is to suspect a hidden source of energy. Is it possible that chemical reactions are responsible for excess energy? That was a question I sent to Fauvarque. His reply was a categorical no. The only possible source would be oxidation of tungsten. But the maximum possible amount of chemical energy from tungsten, he wrote, is much smaller than what is needed to evaporate the reported amount of water. I have to take such statements for granted because I am not a chemist. For the time being the only option I have is to replicate the experiment as carefully as possible. That is exactly what we will do next week.

So what has to be measured in each test? Electric energy deposited in water, EL, the mass of the evaporated water, MS, and the amount of non-evaporative thermal losses, TH. Ignoring a hidden source of energy we expect

EL = MS*L + TH.....(1)

where L=2260 J/g, is the well known latent heat of evaporation. What can be more simple than to verify that the left side of the equation is approximately equal to the right side? We want to measure excess heat, EH, defined as:

EH = MS*L + TH - EL..... (2)

According to Fauvaque et al., that quantity can exceed 30 kJ in five minutes (100 W). If this is true then the determination of MS, TH and EL should not be too difficult. Our methods of measuring these quantities are going to be theirs. The MS will be measured from the readings of the digital scale supporting the entire setup. The precision of measurements is expected to be better than 0.1%. To determine TH we will multiply the rates of non evaporative cooling, at 100 C, by the test duration, such as 300 seconds. The rates of cooling, in joules per second (watt), will be measured at several temperatures between 90 and 98 C. The rate of cooling at 100 C will then be obtained through the linear extrapolation. The expected uncertainty in the value of TH, approximately 2%, will be due mostly to fluctuations in room temperature.

The expected uncertainty in the third quantity, EL, will also be close to 2%. Potential sources of error have already been discussed in unit #256. I think that our method of measuring the average current should be calibrated by using a simple high school calorimeter. That method has been described in unit #255 (in the piece appended on 9/1/05). Scott knows, on the basis of previous experience, that working at voltages exceeding 300 V might introduce difficulties not encountered at lower voltages. From what Scott wrote I inferred that problems he encountered might be due to the effect of emitted HF on the data gathering component. If this is true than grounding the anode, which forms a cylindrical screen around the cathode, might be helpful. he has some other “tricks” to deal with that issue.

And in the worse possible case we can stop collecting data with the computer. Fauvarque et al., reported 49 kJ of excess, in 5 minutes, at 350 V. That is 41% of EL, on the average. And it corresponds to the production rate of unexplained energy equal to 164 W. Even very crude voltmeters and ammeters could be used to detect such rates of production. The strategy , however, should be to do as much as possible at 250 V and at 300 V there the rates of production of EH was reported as 8% and 15%, respectively. Splashing of the electrolyte will probaly also be more intense at high voltages. Than can lead to an overestimation of MS, and consequently to an underestimation of EH. Condensation of vapor on the wall of the beaker, and return of water dropps to the electrolyte, can have the opposite effect of EH. I suspect that these will be our most serious difficulties.

My last comment has to do with L. How do we know that L does not becomes samller than 2260 J/g when vapors are formed in the presence of intensive arcing and sparking? Using an inflated value of L would certainly generate an illusion of excess heat, as one can see from the last equation above. How can one distinguish a true excess heat from an apparent excess heat (due to unaccounted lowering of L)? By using a method of measurement of EH that does not depend on L. How can all thermal energy generated in 5 minutes be measured? What comes to my mind is a closed steel container with very strong walls and some space above the electrolyte. That space would contan the vapor while temperature and pressure are going up rise during a test. Then

EH = (FC + TH) - EL

where FC is the energy removed by the flow calorimeter and TH, as before, is the heat lost by conduction, convection and radiation. TH can be minimized by surrounding the cell with a thick layer of styrofoam, as Scott did in trying to replicate Mizuno’s experiments. TH depends only on the cell’s geometry; it can thus be determined, at various temperatures, from the cooling curve for the empty container. To avoid excessive temperatures and pressures the volume available to vapor should be sufficiently large.

P.S. HERE IS A MESSAGE I POSTED TODAY ABOUT L ON THE CMNS LIST.

“Naudin, and others, confirmed large excess heat in Mizuno Ohmori type experiments. The most recent confirmation is described in the paper of Fauvarque et al. That paper is now downloadable from the library at <www.lenr-canr.org> . It shows that excess power increases with applied voltage (from nearly nothing below 200 V to ~35% at 350 V. The first author is an electrochemist and I asked him about a possibility that excess heat might come from chemical

reactions. He does not think so. The only possible fuel, he wrote, would be tungsten and not enough of it was consumed to account for the measured excess heat.

So what is the source of excess heat? The name of the device is CFR (where R stands for the reactor). That indicates one possibility. But to confirm nuclear origin of excess heat one must show that reaction products accumulate (in a high voltage cell) at a reasonable rate. I am thinking about another interpretation of experimental results. The underlying assumption is that intense electric arcing, the cause of evaporation, has no effect on the latent heat of evaporation, L. But what evidence do we have for the validity of that assumption? The value of L was actually measured by Fauvorque et al. and the result came out to be very close to the known value, 2260 J/g. That shows that no significant errors were made by performing thermal and electrical measurements. But the expected L was found by using an ohmic resistor.

Let me speculate by assuming that L decreases (perhaps by ~35%) when the cell voltage changes from 200 V to 350 V. Not being aware of this, and using 2260 J/g, one would underestimate the expected amount of water to be evaporated. Expecting 40 grams, for example, and observing 50 grams, would then be interpreted as excess heat. But that would be an apparent excess heat, not real excess heat. Was this possibility discussed at some conferences?

Latent heat of evaporation depends on binding energy of water molecules (surface tension, if you prefer). I am speculating that presence of a strong electric field might enhance evaporation. What is wrong with this speculation? “

Appended on 9/23/05:
XX wrote:
“I believe that Conservation of Energy tells us that your speculation cannot possibly be correct, Ludwik. Assume for the moment that we already have a strong electric field in existence (between capacitor plates, for example). Now bring some water into this field. I think we can do this for free (energy-wise) provided the water is already at the correct potential. We just "slide" the water in between the capacitor plates crossing all the E field lines orthogonally at the correct potential point. Now assume for a moment that your speculation is correct. With the water in the strong E field we can evaporate it using less than the usual amount of energy. Now we convey the water vapor back out of the field along the same path and condense it in a low field region, recovering the usual heat of vaporization. The result: free energy. ..just from playing games with ordinary water. My conclusion: Conservation of Energy tells us that the heat of vaporization of water will not be dependent upon the ambient electrical field strength.”

My reply was as follows:
I recognize this "reductio ad absurdum" rebuttal. The absurdum is a possibility, at least in principle, of constructing a perpetual motion engine. This kind of criticism is very powerful and convincing. Let me rephrase the speculation; perhaps this will make it less vulnerable to the rebuttal.

Suppose there are two ways of turning water into a vapor (not two ways of decomposing it, as mentioned by Dennis Letts). The first is direct, requiring 2260 J/g, and the second is indirect, requiring only 1000 J/g. The products, ejected from the liquid, are not the same in these two processes. The first process creates water molecules in air and these are able to recombine and generate 2260 J of heat per gram of water. The second process creates something that can not recombine, unless an additional 1260 J/g is used OUTSIDE THE CELL, to turn them into common H2O molecules. Do not ask me what the intermediate products are, or through what kind of oxidation are they transformed into H2O outside the cell. This is only an abstract speculation invented to avoid Scott's rebuttal. Can a perpetum mobile be constructed on the basis of these two processes? I do not think so.

How can these two processes be used to explain the excess heat reported by Fauvarque et al.? Suppose that the second process does not take place unless the applied voltage is higher than 200 V. Above that threshold the second process becomes more and more probable when the voltage is increased. At 350 V the second process is responsible for 50% of the lost liquid. Thus one half of the liquid water is lost at the cost of 2260 J/g while another half is being lost at the cost of 1000 J/g. The effective L is considerably less than 2260 J/g and this is responsible for the apparent excess heat. The only thing I did was to postulate that the effective L might be voltage-dependent. What right do we have to assume that any process through which liquid water can be removed from the cell must have the same energy cost?

Yes, I know that all this will remain highly speculative, until the second process is identified and experimentally confirmed. But this observation is also applicable to other kinds of speculation, including the idea of nuclear reactions, hydrinos, Casimire effect, monopoles, bacteria, etc. etc. The Mizuno Ohmori effect seems to be real and we must understand it..... The whole point of this discussion is to consider a possibility that not all liquid water is lost at the same energy cost of 2260 J/g. Can water lost via splashing be treated as if it were lost via evaporation? I do not think so. How to avoid splashing (or how to account for it)? According to Pierre Clauzon (private communication) splashing becomes more and more pronounced as the wattage goes up at higher voltages. Practical advice on that point would be highly appreciated.

P.P.S.
Large bubbles seen in boiling grow from tiny bubbles spontaneously formed in water at any temperature. Boiling occurs when vapor pressure inside tiny bubbles exceeds the outside pressure (at t=100 C, when the outside P=1 atm). Under such conditions bubbles formed in the liquid do not collapse; they grow and rise to the surface. This is not the same kind of evaporation as at lower temperatures, when vapors are formed at the surface. It turns out, however, that the values of L, measured at the boiling temperature (2260 J/g) are nearly the same as the value of L measured below the boiling point. So much for normal boiling and evaporation.

Is the glowing volume of plasma, surrounding the cathode inside the electrolyte, made of ionized water vapor? If so then one might think that plasma is an electrically formed vapor made of ions. Why should the energy needed to form one gram of ionized vapor be the same as to form one gram of neutral vapor?

Appended on 9/24/05:
YY wrote: "The correct answer to Ludwik's question is that L (change in enthalpy for vaporization) is a STATE function, hence it can only depend on the initial and final states-it is independent of path. Thus, the change from D_2O(liq) to D_2O(vap) at standard conditions will always yield 45.401 kJ/mol or 2266.9 J/g. The cell voltage used cannot change this value. If this were not true, you could construct a cyclic process that returns the system to the initial state and produces energy, i.e. a perpetual motion machine in violation of the First Law of Thermodynamics."

And here is my reply: "Thanks for your comment, YY. XX made a similar rebuttal last night. That forced me to modify the speculation. In the subsequent message I introduced two kinds of vaporization and two kinds of products being ejected from the liquid. Do you think that the subsequent speculation is also in conflict with the first law? In other word. IS MY SPECULATION, INVENTED TO EXPLAIN EXPERIMENTAL DATA OF THOSE WHO CONFIRMED EXCESS HEAT IN HIGH VOLTAGE ELECTROLYSIS, IN CONFLICT WITH THE FIRST LAW?

In the normal evaporation (path #1) neutral molecules are ejected from the liquid electrolyte at the cost of 2267 J/g. In the competitive process (path #2) what is ejected, at the cost of 1000 J/g is not an already made water molecule. It is something that would need an additional 1267 J/g, outside the cell, if it had to become a water molecule. That additional energy would have to be taken from somewhere (for example, a chemical reaction) outside the cell. In the context of the experiment I want to interpret the only factor that counts is the mass lost during each 5-min test. We do not care what happens to the lost mass outside the cell. Is this position acceptable? It is an alternative hypothesis; the other hypothesis is to postulate that an exothermic nuclear process takes place in the cell and that only path #1 is responsible for the lost liquid."

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260) Facts versus interpretations

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Introduction:
As mentioned before, I belong to the International Society of Condense Matter Nuclear Science (ISCMNS). That society has a restricted discussion list, called CMNS, for its members. In units #252 I wrote about an anticipated experiment. The setup has already been assembled at ETI in Austin (by Scott Little and George Luce); I am going to join them tomorrow. Various aspects of that experiment were discussed in units #253, #255, #256, #258 and #259. What I want to show here is the continuation of the discussion that was described in unit #259.

Voices from the CMNS list:
1) Responding to ZZ, YY wrote:
"Yes, I agree that the energy needed for vaporization is the same. However the source of that energy could be other than purely heat applied. Some of that energy could be derived from dielectric insertion. It is also an energy source. There is energy released by insertion of a dielectric into a constant potential capacitor. That is: the 2267J/g can come in part from heat and part from other sources. The thing that must be kept in mind is that even if you created water vapor between capacitor plates, and find that it takes lest heat than you expect to vaporize it in the region between charged capacitor plates, it would take an addition amount of energy to remove it from that region."

2) Responding to my message (shown at the end of unit #259) YY wrote:
"I think that you are focused on Fauvarque's recent work. The problem, is that he is assuming that the loss of weight is due to water being removed by evaporation. Could it just be that some water is leaving by droplets in the gas flow? The water surface of these things is very active and they did see droplets outside the beaker. I would want more care on that point before I would spend too much time on the possibilities that you are asking about altering the latent heat of vaporization. The same with Mizuno's gas levels of 80 times Faraday numbers - could some of it be CO2 from the electrolyte?"

3) My reply was:
"The first thing Fauvarque et al. did was to measure the latent heat of evaporation with the immersion heater (see Figure 2 in their paper). They measured L at several wattages, up to 600 W. In all cases the measured value was essentially 2260 J/g. Boiling is quite intense at 600 W; if droplets were ejected the value of L would be significantly smaller than the accepted value. This shows that both thermal and electric measurements were accurate. One may argue that droplets are more likely to be ejected when the current flows through an electrolyte, that when it flows through a wire. Oxygen and hydrogen bubbles might carry some invisible droplets, especially when the current is large. I do not know if ohmic heating at 600 W generates more invisible droplets than electrolytic heating at 250 W (at which significant excess heat was reported). What kind of control experiment would you (or others on the list) suggest to demonstrate absence of droplets? Rita permitting, I might be involved in another attempt to replicate the Mizuno-type experiment (next week in Texas). We would be happy to add control experiments designed to make conclusions more acceptable."

4) YY responded:
"I would suggest that you consider the reflux calorimeter method (see my ICCF4 paper in the proceedings). Basically

you put a condenser on top of the cell, and do flow calorimetry on the water flow of the cooling condenser with everything well insulated. You also can put a recombiner at the top of the condenser. The surface of an electrolytic system at several hundred watts can be very "frothy" and very fine droplets can be produced when the bubbles pop. Hydrogen gas in the bubbles "lift" more than just water vapor. I would be very cautious using water loss as a measure of heat. You might could check things by measuring the carbonates remaining in the solution. I would think that the droplets would remove some of the carbonates but evaporation would not. I have not proceeded that way, but it might be a good check."

5) A message from WW (who did observe droplets):
"In the past I did a lot of experiments using a plasma electrolysis system (send to me by E. Mallove) with a condenser which was built on top of the electrolysis cell.(about 1-1.5 meter high). The condensate was collected in a flask. I used a W cathode and a platinum anode during the electrolysis of K2CO3.(in the beginning to carbon electrodes). The main purpose of this experiment was to look for changes in T 1/2 of radioactive elements. First I used radioactive Thallium 201. I put about 5 MBq of 201 Tl into the solution and started electrolysis with a low current. After a few hours the Thallium was adsorbed on the W electrode. I measured the gamma emission with a solid state detector. Then I increased the voltage to 70-80 V and plasma electrolysis started . Then the voltage was increased until 220V. During the experiment I could clearly see a drop in gamma emission from the solution. After electrolysis I noticed that a lot of the radioactivity could be found on the bottom of the reaction vessel, mainly in (WO2?) particles. I concluded that the change in geometry of the distribution of radioactivity caused the decrease in gamma counts. I did not persue further analysis of the particles. Further it was remarkable that about 10- 20% of the radioactivity was found in the condenser solution. I concluded then that droplets with radioactive solution went through the condenser into the condensate. My conclusion was that during my experiment about 10-20% of the solution was not evaporated but was transported as small droplets. This has to be taken into account if one wants to calculate the power balance of the cell. It is possible that during more intense plasma electrolysis at higher voltages,higher percentages of non evaporated solution can be expected."

6) My comment on what YY wrote:
"a) While reading YY's paper I realized that he was also addressing the issue of the path #2 (ejection of liquid water from the electrolyte at the energy cost of less than 2267 J/g). The only difference between his version and my version is the mechanism of the second path. I had no idea what the mechanism might be; Dennis associated it with invisible droplets in the streams of bubbles of hydrogen and oxygen. Each of us is saying that excess heat in high voltage electrolysis, or at least part of that heat, might turn out to be apparent. As far as I know that potential source of error was not discussed by those who published reports on excess heat in Mizuno type cells. Is this a correct assessment? How can this be explained?

b) The way to eliminate the suggested source of error is worth taking seriously. It is probably too late to try D2's old approach in our next-week experiment. But it is something to consider, if the excess heat is observed.

c) The idea of paying attention to the concentration of the K2SO3 in the electrolyte is also attractive. The initial concentration is 20 grams per liter. After only two 5-min tests one is expected to loose about 100 cc of liquid water. Let me tentatively assume that 50% of that is removed in the form of droplets (producing a lot of apparent excess heat) . That would take away 1 gram of the salt. The concentration of the remaining electrolyte, in Fauvarque's experiment, would then change to 19 grams per liter. Such change is probably not very difficult to measure. Here is a suggestion for our next-week experiment. A sample of the initial electrolyte should be preserved in a bottle. The used electrolyte, for example after ten or twenty tests, should also be preserved. A comparison of concentrations seems to be worth making. Why was this not done before? Thanks for the suggestion, D2.
P.S.
d) VV's contribution is extremely valuable. It shows how radioactive tracers can be used to perform very accurate measurements of the relative contribution of the path #2 identified by Dennis. The probability of the path #2 should increase with the voltage because the current (and the associated rates of generation of O2 and H2 bubbles) becomes larger. If that is true then the excess heat should also go up with volts, as reported by Fauvarque et al. But that would be an apparent excess heat."

7) Responding to droplets mentioned by YY, VV wrote:

“(a) I do not know how he confirmed this. Ohmori confirmed it by placing paper towels around the cell, and weighing them after the run.
(b) No. The gas goes through a condenser and then a sample is sent through a mass spectrometer to measure composition.
See: <http://www.lenr-canr.org/acrobat/MizunoThydrogenev.pdf>.”

8) YY responded:
Thank you; I had not seen that paper. I am quite taken by his figure 10. This is quit comforting. The jump in output between 70 and 75 is about what I had noticed in many other CF type systems. In fact I now (since 95) run most all my "good" CF cells above 72 C and many right at boiling. I would say that although Mizuno runs similar electrodes (W,W/Th. ...). I normally run with sulfamic acid (yes, that is sulfamic not sulfuric) at higher current densities.

As an aside- mine glow blue instead of yellow/orange. See picture on my home page <http://www.netmdc.com/~physics/>.

My work right now is going after the hydrogen stream. I am also trying to see if I can get some additional photo-dissociation within the cells. (Compare to work of using Rh ions to mimic photosynthesis and creating the ions in situ from the electrolysis by adding rare earth ions to the electrolyte). As I said in an earlier thread, recovering H for part of the "excess" energy makes the engineering breakeven easier since you only get about 30% of the heat back (for an electrical input loop)-if you are very good- because of Carnot, but you can get 60 to 90% back in the H stream via a fuel cell to use for input electrical energy. I hope to get some funding to be able to burn it "in" a Stirling engine. I have a small one right (about 1 Watt)now but it is hard to match to the high power levels. I have not yet been able to scale down these sparkly cells to levels below about 70 or so watts. It is much easier to get them to "work" at higher levels. You need the high current densities and if you scale it down you have to use a point and those just erode very quickly. Thanks for the info.

9)My message:
According to the 2005 paper of Mizuno et al., <http://www.lenr-canr.org/acrobat/MizunoThydrogenev.pdf> the temperature and pressure in the plasma region are extremely high (up to 9000K and 4000 atm. respectively). I am inclined to think that a sizable fraction of the liquid water lost during each test is vaporized near the cathode. And it is reasonable to thing that the fraction increases with the voltage. (Foe example from ~0% at 200 V to ~36% at 350 V.) That fraction would be "the abnormal" evaporation.

In the Sears and Zemansky's "University Physics" I see this statement : "as we approach the critical point [along the vaporization curve in the pT phase diagram] the differences in physical properties between the liquid and vapor phases become smaller and smaller..... THE HEAT OF VAPORIZATION ALSO BECOMES SMALLER AND SMALLER AS WE APPROACH THE CRITICAL POINT, AND IT TOO BECOMES ZERO AT THE CRITICAL POINT." I suppose that this statement was more than a speculation; it was probably based on some experimental evidence.

Thus associating abnormal evaporation with the lower than usual heat of vaporization is not at all silly. The implied assumption (made not only by Fauvarque et al.) -- that the average latent heat of vaporization is 2267 J/g -- might be not valid. Accepting this point of view one would conclude that the reported excess heat might be apparent. Thus we can list five competing qualitative explanations of experimental data:

a) Excess heat is real; it is due to nuclear reactions.
b) Excess heat is apparent, due to ejection of tiny droplets (experimentally observed by Peter van Noorden).
c) Excess heat is apparent, due to the lowering of the average heat of evaporation.
d) Excess heat is real but its origin remains to be identified.
e) Excess heat is apparent for a reason not connected with (b) and (c).

I would like to know how rapidly L decreases when T is going up. Unfortunately, the water vapor table I consulted does not have a column for L.”

9) I expected to hear from more of those who reported on reality of excess heat on the basis of the amount of water

evaporated. Their contributions to the debate, if any, will be appended here. Please revisit.

9) XX responded:
"I don't think 'c' can be an explanation for the apparent excess heat results. I agree that the heat of vaporization does go to zero as you reach the critical point (where there is no distinction between liquid and gas, hence no such thing as vaporization anymore). But, as Miles told us, 'L (change in enthalpy for vaporization) is a STATE function, hence it can only depend on the initial and final states-it is independent of path.' Thus, the following two processes will require exactly the same energy input:

a) Evaporating 1 gram of water at 100C and 1 atm pressure (i.e. ordinary boiling).

b) Taking 1 gram of water at 100C and 1 atm pressure up to the critical point, evaporating it there for 'free', and then bringing the resulting vapor back down to 100C and 1 atm.

In our experiment, water at 100C and atmospheric pressure moves towards the cathode where it is somehow converted into vapor. The vapor bubbles rise up through the water and burst at the surface releasing water vapor that is essentially at 100C and atmospheric pressure. From an energetic viewpoint, I think we can therefore ignore the details of the vaporization process at the cathode."

10) YY responded:
"You may also want to look at the fugacity of the water at the cathode. But it too is a state function. Just having done this types of items, I would first design the experiment to check b) first."

11) My reply (not posted):
I agree with this, if what "evaporates" at much higher p and T consists of common H_2O molecules. But the first law does not seem to be violated if the substance escaping from the liquid does not consist of H_2O. The escaping matter might become H_2.O, outside of the cell, at the expense of energy of some chemical reactions. I should have phrased the explanation (c) more carefully, as I did before. In any case; we are not discussing water as a fluid in a cyclically operating, and reversible, engine.

12) I expected to hear from more of those who reported on reality of excess heat on the basis of the amount of water evaporated. Their contributions to the debate, if any, will be appended here. Please revisit.

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261) Energy in high voltage electrolysis

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An experiment to be performed was described in unit #252. Results from our work, still in progress, will be presented at the next international conference on cold fusion (ICCF12) in Japan. Facing the deadline I submitted the following abstract.

Searching for excess heat in Mizuno-type plasma electrolysis

Ludwik Kowalski (a), Scott Little (b), and George Luce (b)

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Excess heat generated in the glow discharge plasma electrolysis, first reported by Mizuno and Ohmori (1), has been studied by several researchers, both in Japan (2, 3, 4) and in other countries (5, 6, 7, 8). Most reports, but not all, confirmed generation of excess heat. Facing this situation we decided to replicate the most recent experiment in which excess heat was found to increase with voltage (8). The planning for the design of this project was described in (9). Considerable progress has been made toward the building and testing of a cell able to operate at high power levels. This work, still in progress, should either confirm or contradict the results reported in (8).

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5) Jean-Louis. Naudin et al. Several illustrations and references are downloadable from <http://jlnlabs.imars.com/cfr/index.htm> and from <http://jlnlabs.imars.com/cfr/html/cfrtpwr.htm>

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8) Jean-Francois Fauvarque, Pierre Paul Clauzon and Gerard Jean Michelle Lalleve. *"Abnormal excess heat observed during Mizuno-type experiments;"* 2005. Downloadable from the library at <http://www.lenr-canr.org>

9) Ludwik Kowalski <http://csam.montclair.edu/~kowalski/cf/252clauzon.html

WHAT FOLLOWS IS AN ATTEMPT TO DESCRIBE OUR WORK. THIS IS A DRAFT;
IT WILL GROW IN COMING WEEKS. PLEASE REVISIT. PLEASE COMMENT.
kowalskil@mail.montclair.edu

Introduction:
Suppose that a common immersion heater is inserted into a beaker with water. How should thermal energy released in the container depend on the amount of electric energy supplied? The answer is in the law of conservation energy. In a steady state the amount of electric energy, E, supplied during a time interval, t, must be equal to the amount of thermal energy released, Q, during the same interval.

$E = Q_v + Q_c$ ..... (1)

where Q_v is thermal energy released by vaporization and Q_c is thermal energy released by conduction, convection and radiation. But in the setup described in (8) the sum of the two terms (on the right side of the above equation) was reported to be significantly larger than the electric energy on the left side. The authors believe that some hidden energy, presumably nuclear, is released during the electrolysis. That is the essence of the excess energy claim. To verify it we constructed a similar setup and measured E, Q_v and Q_c. The work is in progress; the results will be added, probably in three weeks or so. The purpose of this unit is to describe the methodology and to share some observations. The electric energy on the left side is the integral of $dE = i(t)*v(t)*dt$, over a test duration.

$E = \text{integral}(dE)$ .....(2)

where i(t) and v(t) are the instantaneous current and voltage between the electrodes, respectively. Instruments used to measure E are described in the appendix. To measure Q_v one must determine the amount of water evaporated, M_p, during the plasma test

$Q_v = M_p*L$..... (3)

(4) where L is the latent heat of evaporation. Two measurements of the evaporated mass were performed, one when the current was flowing through the electrolyte, giving us M_p, and another, when the current was flowing through the ohmic heater immersed in the electrolyte, giving us M_o. The value of Q_c can be calculated as

$Q_c = L*(M_t-M_o)$ .....(4)

where $L*M_t$ is the amount of thermal energy that would be lost if evaporation were the only way of loosing heat. In other words M_t*L is nothing else but E measured at the same time as M_0. One does not have to measure M_t to calculate Q_v. Additional nuances involved in experimental determinations of Q_v and Q_c will be addressed in the next section. The equation (1) is certainly satisfied when an ohmic resistor is used to sustain boiling. But will it also be

satisfied when boiling takes place during the high voltage electrolysis? That is the question we want to answer.

Experimental setup

According to the sketch, shown in unit #252, the setup is conceptually simple. But gathering reproducible data turned out to be difficult. Two differences between the initial sketch and the actually used setup had to be made to match (8) as close as possible. Instead of a spiral Pt anode we used a cylinder made from the platinized titanium mesh. The height of that cylinder was 5 cm and its diameter was 6.3 cm. The anode was a welding tungsten rod containing 2% of thorium. The electrolyte, as in (8), was a 0.2 M solution of K_2CO_3 in distilled water. To keep the volume of the boiling electrolyte constant the evaporated water was continuously replaced by fresh water coming from an auxiliary reservoir. As shown in Figure 1, that reservoir was mounted on the same support as the scale.

The temperature of water in the reservoir, T, typically 25 C, was constantly monitored. If that temperature were 100 C then the value of L, to be used in formulas (3) and (4), would be 2260 J/g. At a lower T, however, the effective latent heat of evaporation is 2260+4.186*(100-T). Thus the effective L is 2594 J/g at 20 C and 2553 at 30 C. Two thermistors were used, one to monitor the temperature of the electrolyte and another to measure T of water. A simple open beaker, turned out to unsuitable above 250 volts, due to splashing of the electrolyte. The beaker was thus replaced by a taller cylindrical vessel made from polycarbonate. The depth and the inner diameter of that vessel were 32.0 cm and 11 cm, respectively. A set of ani-splashing baffles was mounted near the top of the vessel, as illustrated below.

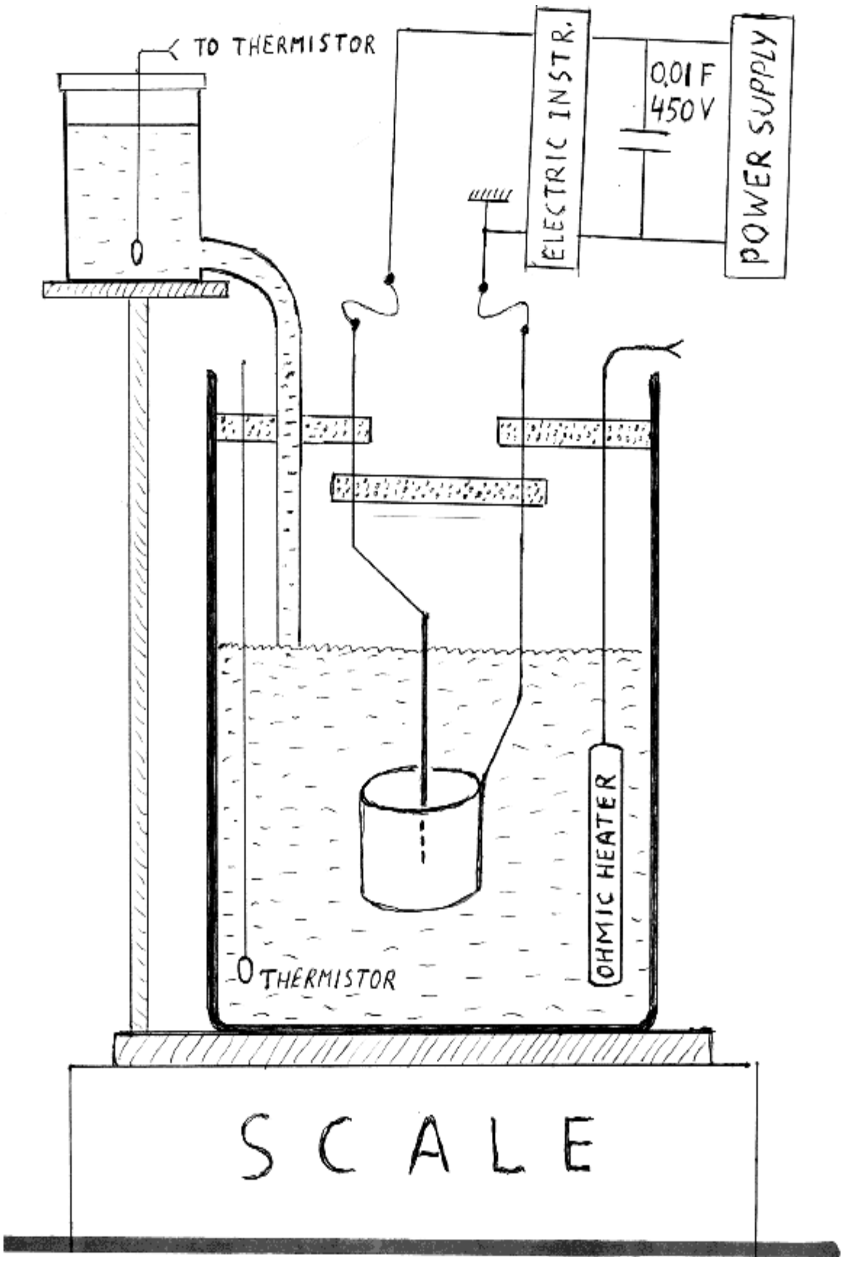

FIGURE 1
Schematic diagram of our first setup. The volume of the electrolyte was 650 cubic centimeters. The dotted baffles, in the upper section of the cell, offer an escape path to the vapor but not to the splashing electrolyte. Vertical rods supporting the electrodes are mounted on these baffles. The "electrical instruments" box, at the upper right corner, stands for the Clark-Hess meter (model 3200). It is essentially a set of three digital instruments: an ammeter, a voltmeter and a wattmeter.
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Masses of water evaporated in consecutive tests (M_p and M_o were determined from the readings of the scale. Light flexible filaments were used to connect electrical components inside the vessel with outside circuits. That arrangement did not prevent us from measuring mass differences (typically 40 grams) with a sufficient accuracy (at least 0.5%). The entire vessel was wrapped in a layer of polyethylene foam. The purpose of that thermal isolation was to keep the walls temperature as high as possible in order to minimize condensation of vapors on the inner surface. Condensed water dripping back into the electrolyte would reduce values of m_1 and M_o . Splashing, on the other hand, would make M_p and M_o larger than what were evaporated.

Note that the intensity of splashing, if not mechanically reduced, would increase with the applied voltage. This could

possibly create an illusion of a voltage-dependent excess heat. The same kind of illusion could occur if the "invisible splashing" was significant. Ideally only water vapor (plus some hydrogen and oxygen) should escape from the electrolyte. Suspecting that tiny droplets of electrolyte might also be escaping from the cell we compared concentrations of the electrolyte before and after some tests. These two concentrations should be the same because evaporated water is replaced by fresh water. But replacing droplets of the electrolyte with water would make the second concentration smaller than the first. A reduction of the concentration was noticeable but not significant to create an illusion of excess heat. Another possibility of a systematic error, could possibly occur if the rising temperature of water in the reservoir were not taken under consideration. The flow of steam escaping from the vessel increases with voltage and affects the temperature in the reservoir. This, in turn, changes the value of L used in formulas 3 and 4. Using L=2595 (correct at 20 C) when in reality it is 2427 (at 40 C), for example, would create an illusion that Q_V and Q_C are 7% higher.

The ohmic heater was used to preheat the electrolyte to the boiling temperature and to measure M_o. After that the ohmic heater was turned off and boiling was maintained by the current flowing through the electrolyte. The evaporated masses m1 were measured after the ohmic heater was turned off and after desired voltages were applied between the anode and the cathode. The Figure 2 shows time dependencies of our two most essential parameters. The line labeled "masses" refers to evaporated water (M_p or M_o in grams along the left-vertical axis). Likewise, the line labeled "energies" refers integrated electric energy (E in kJ along the right-vertical axis). Other measured, or computer-calculated, parameters were also monitored during the experiments but their time dependencies are not shown in the figure below. The mass lost was essentially zero until the boiling temperature was reached. After that the amount of water evaporated started increasing. The segment AB of the mass curve was used to determine M_o. The segments CD and EF of that curve were used to determine M_p at two different voltages. The corresponding segments of the energy curve, A'B', C'D' and E'F' were used to determine the values of the energy received in the matching time intervals.

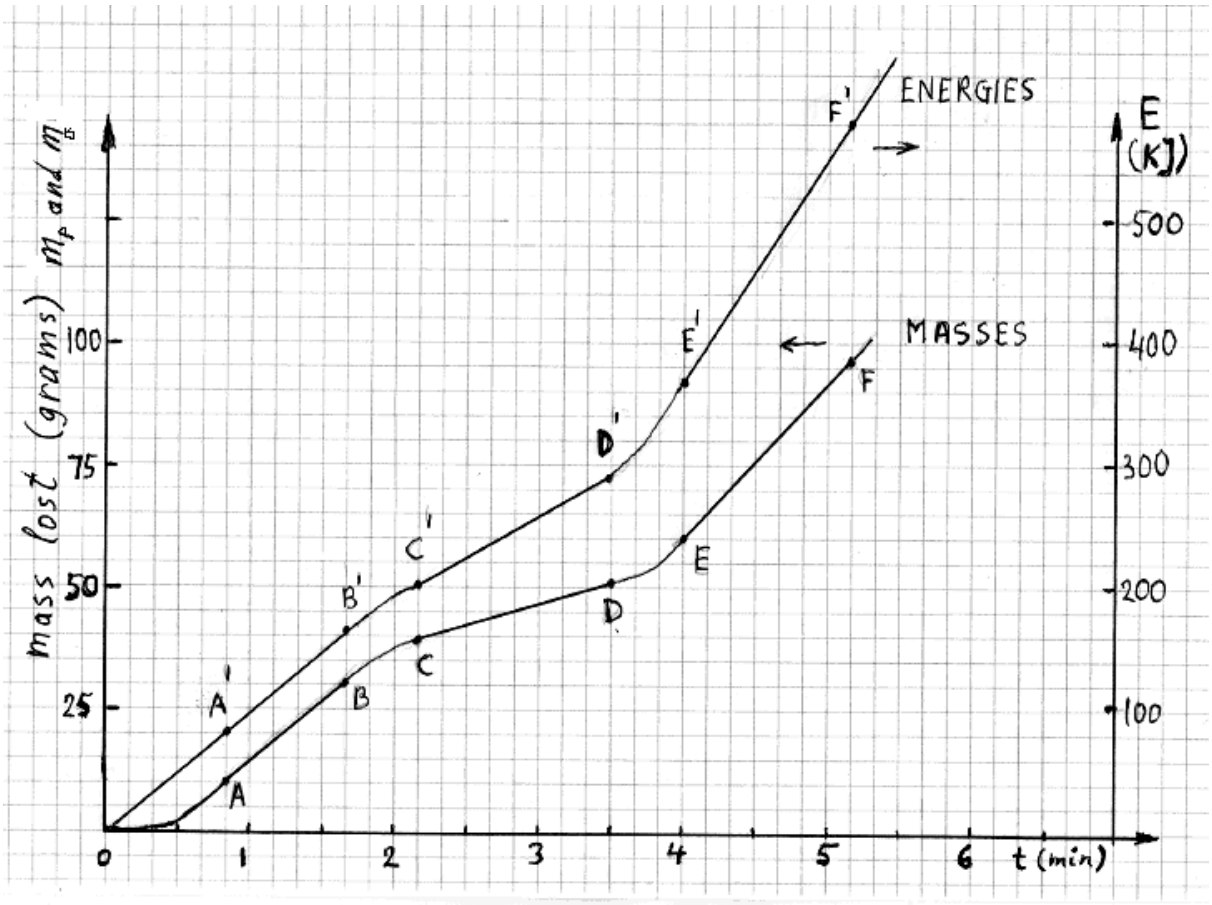

FIGURE 2
Typical time dependencies of M_p (or M_o) and of E for three short tests. Boiling point was reached at t=0.5 min but the first test started (in this hypothetical illustration) at t=0.833 min.

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Due to micro explosions in the electrolyte, especially in tests above 300 volts, the mass curves often displayed wiggles (not shown in figure 2). Wiggles, representing dynamic effects on the scale, did not interfere with determinations of masses of the evaporated water because only initial and final readings were needed. We were always able to stop cell shaking by lowering the voltage for three to five seconds.

Results

Figure 3 shows how the rates of non evaporative losses (measured with the ohmic heater) depended on the electric power. The values of electric power, in watts, are shown along the horizontal axis while the vertical axis shows rates of non evaporative losses (also in watts). The rate of loosing heat by conduction and convection increases with electric power. This is an expected result; the rising column of steam acts as an air pump which drives convection currents more strongly than when generation of steam is less intense. Furthermore, more rapid boiling is equivalent to more intensive mixing of the electrolyte. This too is likely to increase the rate of cooling by conduction and convection.

The values of E, Q_V and Q_O, at different voltages, are shown in the table below. Most cells of this table contains two numbers; the mean value and the standard deviation. For example, a cell at the intersection of the row labeled 300 V and the column labeled E, shows that the . . .

Single numbers, in the last column, labeled COP, are coefficient of productivity at different voltages. By definition:

$COP = (Q_v + Q_c) / E$

The numerical values of COP were calculated from the mean values of Q_V, Q_C and E, at corresponding voltages. The data show that . . .

Please revisit this unit in November to see the results.

APPENDIX 1
Measuring electric energy

The electric energy E, delivered to a cell, is the integral (over a test duration) of dE = i(t)*v(t)*dt. The instrument we used, Clark-Hess model 2330 power analyzer, sampled the v(t) and i(t) about 2000 times per second. As expected, the voltage between the cell electrodes, v(t), was essentially constant but the current i(t) was highly irregular. Digitized samples of v(t) and i(t) were recorded by the computer and the cumulative E was displayed during the data acquisition, as shown in Figure 2. A highly irregular nature of the i(t) waveform guarantees random sampling and the average energy, over a time longer than several seconds, is highly reliable. To verify this we also used an a.c. watt meter at the input of our d.c. power supply. The a.c. energy, E', measured with that instrument was only slightly larger than the d.c. energy, E. The difference, (E'-E), was consistent with what is necessary to operate the power supply when it is not loaded. A high quality digital multimeter, Fluke 87, was also used to measure the mean current. The result was identical with the mean current measured by the Clark-Hess instrument.

APPENDIX 2
Measuring electric energy
A polycarbonate cell whose height and internal diameter were . . . cm and......cm, respectively, replaced the taller cell used in the previous set of experiments. The new cell (see Figure 4) has a lid made from the linen-based phenolic plastic. The lid is firmly attached to the cell with four nylon-tipped screws. The electrodes, the water reservoir, the ohmic heater, and the thermistor are now mounted on the lid. A PVC exhaust pipe, whose inner diameter is 2.54 cm, is also mounted on the lid. Its purpose is to conduct steam escaping into air. A plastic baffle, placed 2 cm below the pipe’s input (inside the cell), is an effective shield against consequences of splashing the electrolyte.

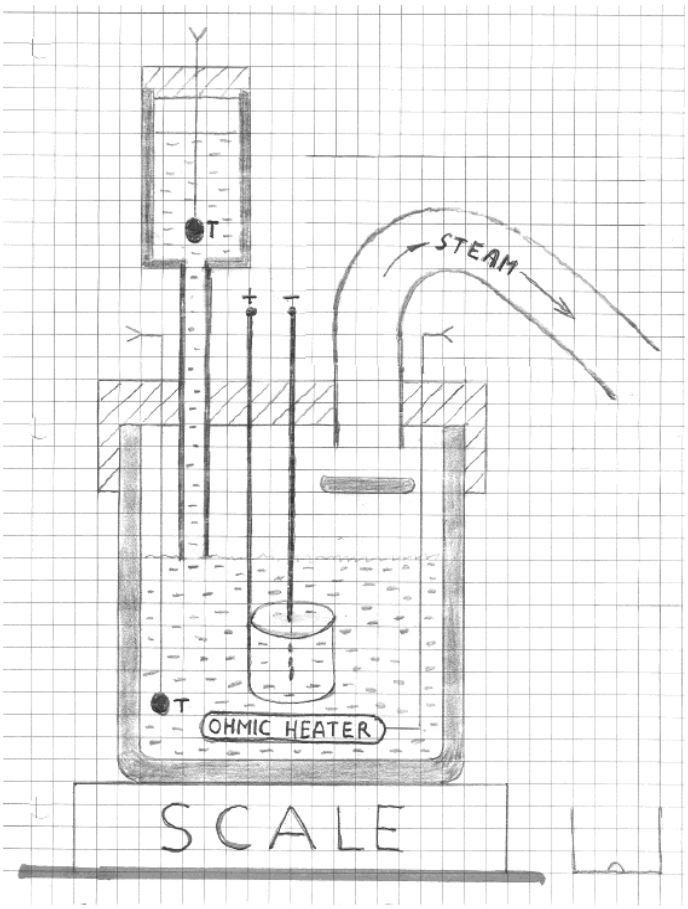

FIGURE 4
Schematic diagram of our second setup.

Some condensation is expected to occur on the inner surface of the exhaust pipe, mostly in the outermost section where the temperature is the lowest. That section was directed downward so that drops of water could drip down to be collected. Note that in a vertical pipe condensed water would return to the electrolyte. Also note that steam ejected from the cell is kept away from the water reservoir and from electrical connections above the lid.

The Pt-coated Ti mesh anode, same as before,is now supported by two titanium rods (instead of one) to minimize shaking. As before, the exposed end of the tungsten cathode (about 1 cm) was located at the center of the cylindrical anode. One of the problems encountered with the previously used cell was arcing between the electrodes in the space above the electrolyte. All metallic components in the new cell are covered with insulation. Hopefully this will reduce the probability of arcing.

NOT TO FORGET:
1) Recognizing that the idea of measuring the concentration of salt in the electrolyte (before versus after a test, in order to investigate a possibility of ejection of tiny droplets) was suggested to us by Dennis Cravens. Also his critical comments on measuring E.
2) Electroplating as a method of calibrating our C-H ammeter, if necessary.
3) Recognizing Clauzon's help in providing details about their setup.
4) Try to understand presence of tungsten in the water reservoir (discovered with the X-ray fluorescence instrument)
5) Recognizing Puthoff's (ETI?) hospitality, contribution and encouragements.
6) ?

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262) H_2O versus D_2O

Ludwik Kowalski (10/5/05)
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As mentioned before, I belong to the International Society of Condense Matter Nuclear Science (ISCMNS). That society has a restricted discussion list, called CMNS, for its members. The purpose of this unit is to record what I learned from recent messages posted on this list. The topic is as old as cold fusion itself. Fleicshmann and Pons announcement was based on experiments with palladium in heavy water. Similar experiments with common water, if I recall correctly, produced negative results, as far as cold fusion was concerned. Later, however, cold fusion phenomena were observed by using other metals and common water. In what follows authors of messages are identified as researchers, R1, R2, R3, etc.

Voices from the CMNS list:

1) R1:
CMNC group - as you may know, I have been asked to discuss "CMN Effects in Light-Water and Hydrogen Systems" at the ICCF-12 short course. I would welcome any suggestions of recent work/results that you think should be included in this discussion. I anticipate positive results, but if you have arguments against this aspect of CMN, I would want to know those also.

2) R2:
To my knowledge, no real test has ever been performed with "pure" light water, which means that it is not possible to rule out the possibility that the residual (.02%) component of heavy water might be causing the Excess Heat. For this reason, I would suggest the term "normal water" be used in the future, not "light water."

3) R3 wrote:
I agree. The term light water should only be used for water with no D or T. To my knowledge no one has used light water. Also, most of the normal water- nickel, normal water- W, Mo, Zr systems I have used always appeared to work better with some D2O added.

4) R4:
I suggest when the argument that the deuterium in normal water is the source of heat in light water experiments, you should take into account that electrolysis concentrates light hydrogen in the cathode. If the electrolyte contains 0.02% D, the cathode will contain only about 0.001% D. This seems like too small a concentration to have much effect.

5) R3:
Yes the concentration will be small at the cathode. However, I still think the term light water should only refer to water devoid of any D or T. If not, we will have a problem later when someone does a true light water experiment.

6) R5:
My colleague and I, have effected some tests with the cell of Mizuno in which we have introduced some quantities of D2O. The amount of anomalies of energy did not change.

7) L.K. (addressing R2):
On the other hand, according to Fauvarque et al. "In any case, the device presented in this paper is a very simple device which can be used to rapidly verify hypotheses without sophisticated means. For example, we have verified that this phenomenon does not seem related with the heavy water which is found at one part in 7,000 in natural water. We increased the concentration of heavy water by a factor of 100 in our electrolyte, i.e. to 1 part in 70. We did not see any perceptible change in our results." This seems to be a good experimental proof that the role of deuterium is not dominant in Mizuno-type generation of excess heat.

8) R4:
I agree with D2 that we should make a distinction between normal water and pure light water. However, it has always concerned me that a little H2O in D2O will kill a heavy-water cell, but it is claimed that a little D2O in H2O will add to the excess energy. Why should it be so?

9) R2:
But if the effect is tied to crystal size (and considerable evidence exists that smaller, nanoscale structures are helpful) and/or magnetic effects, ideas associated with "how much" D in the cathode might not be relevant.

10) R3:
I have not made a study of it directly. However, it always seemed to me that the important part was the D2O purity on initial loading. Once it is loaded it was less sensitive to H2O. There have been a few times when I ran out of D2O and had to add H2O to an existing cell. They seemed to continue for some time even with 10 to 20 % H2O added if they were first loaded in good D2O and still running. - Now the counter to that is that if I had made a mistake in measurements, those errors just were continuing with the H2O addition. I know that X made several runs with H2O added to D2O runs and got reasonable results. In the Old CETI days, most of the "good" runs were those that where first run in D2O. If you added H2O they would continue about 2 or 3 days then "die out". R4- have you done some study at the replacement rate in a loaded cathode if the current is never removed after first loading with D?

11) R6:
If the claims are correct, the result is a function of the cathode material. With Pd, H2O kills the reaction. With Ni, ordinary water works, but increasing the deuterium content enhances the reaction. Apparently, with W glow discharge, the deuterium content makes no difference. This is a summary of what some people have reported. I do not think that these claims have been rigorously tested, except the first: H2O poisons the Pd heat producing reaction. What it does to the reactions that generate tritium and other nuclear effects I cannot say.

12) R4:
I think we need to consider two main points. If the nuclear reaction is simple fusion, the reaction rate will be proportional to the D concentration squared. If clusters are required, such as D4, then the rate will be proportional to the fourth power. This is presumably why the D concentration needs to be so high. Dilution with H would reduce the D concentration even though the total hydrogen concentration would remain high. The second point is that if, as we all believe, the effect occurs in the surface region, exchange with H will be very rapid because the electrolytic process consists of D dissolving in the Pd, diffusing to the nearest crack, and leaving the metal as D2. In other words, a steady flux of D(H) passes through the surface region. The only way D would not be rapidly replaced by H would be for the D to be trapped in a very stable lattice site, presumably by formation of a compound more stable than beta PdD. This is one of the reasons I have been suggesting that the NAE is PdD2 rather than PdD.

13) R7:
R1, you may want to talk about our work with CR39 detector plastic, showing alpha tracks after exposure to electrolysis using ordinary tap water and Li2SO4. The relevant papers can be found in the proceeds of ICCF-11.

14) R8:
If I were to take a long view of this, as Peter Gluck often does so well, I would say that the current body of knowledge and understanding of this field is only a mere fraction of what it will eventually be ... and that the semantics may not be so important.

15) R9:
Hi R4, would you please explain to the group (including me) how you arrived at the numbers above?

16) R2 (responding to R4):
This is one scenario. But there can be (and probably are) many scenario's. In band-like states, comparing concentrations of H vs D is like comparing apples and oranges because the statistics, possible modes of occupation, and forms of interaction are entirely different. In this kind of scenario, the concentration is of band state H's (protons) or band state D's (deuterons, d's). These concentrations can transient and/or (potentially) virtual (i.e., not long-lived). Within this scenario, many possibilities occur. Interfacial effects, however, will be important.

17) R4 (referring to R9's question):
If you look at Fig. 1 in "The Effect of Hydriding on the Physical Structure of Palladium and on the Release of Contained Tritium" you will see how H is substituted for D in Pd at high concentrations of D. In this region the substitution factor is about 2 with a tendency to larger factors as the H/D ratio in the electrolyte increases. My suggestion of 20 for normal water is largely a guess which I suggest to get people thinking about this effect. Obviously, this measurement needs to be made more accurately if a model is based on the amount of D in Pd after electrolyzing in normal water.

18) R4 (addressinging R2):
It does not matter what changes the concentration of D, whether it be dilution by H or distribution between different states. It is the concentration of D in which ever state that allows the nuclear reaction to occur that matters. As long as several D nuclei must be in the same place at the same time for a reaction to occur, the concentration will influence this probability. The greater the number of D that are required too form the final product, the more the reaction rate is increased by increases by the concentration in the active state or environment, i.e. by concentration raised to increased power. Granted, the active state or environment may not have the composition obtained from measurement. If I understand your suggestion, you are proposing that the active state has a composition that is relatively independent of the measured composition and that no matter how much D is present in the electrolyte, the active state is always saturated. If this were so, heat production from the cold fusion should never be sensitive to the amount of H in the heavy water, which is clearly not the case

19) R2 (replying to R4):
Implicit in my comments are subtle points associated with band states and the fact that only a very small band state occupation is required and the fact that in a many situations, assuming such a picture is valid, when the band state occupation becomes to large, the reaction will turn off. Implicit in my comments is also the notion that in a true, many-body situation, many charges are involved, and this can drastically alter the situation in such a way that basic notions about being "localized" at the same place can be replaced by a counter-intuitive notion: being at "same" place simultaneously at many different locations. Below, I am providing specific answers to your questions.

It does not matter what changes the concentration of D, whether it be dilution by H or distribution between different states. It is the concentration of D in which ever state that allows the nuclear reaction to occur that matters.

Not true if some of the deuterons are in in band states and some are not.

As long as several D nuclei must be in the same place at the same time for a reaction to occur, the concentration will influence this probability.

Not true when the deuterons interact with many charges coherently; in this case the d's need not "be" in the same place in order to react. Waves are not localized.

The greater the number of D that are required too form the final product, the more the reaction rate is increased by increases by the concentration in the active state or environment, i.e. by concentration raised to increased power.

Not true when only a small amount of d are required and when the reaction turns off if too many are involved, which is the case when "ion band states" (or even ion band state fluctuations) are involved.

Granted, the active state or environment may not have the composition obtained from measurement.

There need not be a single "active state." In fact, Quantum Mechanics requires that "all" accessible states be included.

If I understand your suggestion, you are proposing that the active state has a composition that is relatively independent of the measured composition and that no matter how much D is present in the electrolyte, the active state is always saturated.

This is partly true. The solid can (and probably does) filter out potentially reactive d's

If this were so, heat production from the cold fusion should never be sensitive to the amount of H in the heavy water, which is clearly not the case

This pre-supposes saturation is required. The key point is that the electronic structure dictates what goes on. The electronic structure dictates the chemical potential, and this determines if the d's (and/or protons) can occupy band states. The point is that the solid (which can have characteristic dimensions as small as 10's of nanometers) plays a key role.

20) R4 (replying to R2):

a) I wrote:
It does not matter what changes the concentration of D, whether it be dilution by H or distribution between different states. It is the concentration of D in which ever state that allows the nuclear reaction to occur that matters.
You replied:
Not true if some of the deuterons are in in band states and some are not.

I see that we are talking past each other. I will try to make my point clearer. You point out that some of the deuterons are in band states and some are not. Those that are not in an suitable band state can not enter into a nuclear reaction, hence are irrelevant to the discussion. This is point I was making. We must only concentrate on those D that are in a state that allows the nuclear reaction to occur. This means that the total measured D concentration is not the main variable we must discuss.

b) I wrote:
As long as several D nuclei must be in the same place at the same time for a reaction to occur, the concentration will influence this probability.
You replied:
Not true when the deuterons interact with many charges coherently; in this case the d's need not "be" in the same place in order to react. Waves are not localized.

Granted, the waves are not localized so that my description does not apply. Nevertheless, a probability exists that describes the interaction of waves. Would not this probability be sensitive to the number of waves present in the active state?

c) I wrote:
The greater the number of D that are required too form the final product, the more the reaction rate is increased by increases by the concentration in the active state or environment, i.e. by concentration raised to increased power.
You replied:
Not true when only a small amount of d are required and when the reaction turns off if too many are involved, which is the case when "ion band states" (or even ion band state fluctuations) are involved.

I do not understand this argument. You seem to be adding additional variables that allow anything to be explained, i.e. the model can never be tested because its failure to predict behavior can be explained by too much D or too little, neither of which can be measured.

d) I wrote:
Granted, the active state or environment may not have the composition obtained from measurement.
You replied:
There need not be a single "active state." In fact, Quantum Mechanics requires that "all" accessible states be included.

Are all accessible states active in initiating nuclear reactions? My point is that only those states that are active are important.

e) I wrote:
If I understand your suggestion, you are proposing that the active state has a composition that is relatively independent of the measured composition and that no matter how much D is present in the electrolyte, the active state is always saturated.
You replied:
This is partly true. The solid can (and probably does) filter out potentially reactive d's

What do you mean by "filter out"? All d are identical until one converts to a wave by being in a band state. Presumably a mechanism exists that converts one d to a wave while another d that is dissolved in Pd does not convert. What determines how many d form waves?

f) I wrote:
If this were so, heat production from the cold fusion should never be sensitive to the amount of H in the heavy water, which is clearly not the case.
You replied:
This pre-supposes saturation is required.

No, this only presupposes that the number of d waves available for interaction determine the reaction rate. If the number of d waves is reduced by the presence of H, the nuclear reaction rate will be reduced.

g) You wrote:
The key point is that the electronic structure dictates what goes on. The electronic structure dictates the chemical potential, and this determines if the d's (and/or protons) can occupy band states. The point is that the solid (which can have characteristic dimensions as small as 10's of nanometers) plays a key role.

This is all true, but does not address the issue.

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21) LK (myself):
OK, I will stop recording this thread. It turned out to be a good sample of interactions between CMNS researchers. It does not seem to differ from interactions between scientists in other areas. So why do people continue to think that the field is pseudoscientific? Why are reports from investigations in that field rejected by mainstream journals? Why do their research proposals not supported by the DOE or NSF grants? Who benefits from this highly abnormal situation?

By the way, as I type this I recall a recent conversation with a mathematician in Austin. He said that the situation cold fusion finds itself in is not unique. Something similar is taking place in astronomy. A group of highly qualified people, rejecting the "big bang" idea and proposing another interpretation of experimental data, is practically excommunicated from the community of astronomers. Their publications, he said, are rejected by mainstream journals and they find it difficult to get access to large telescopes. That is interesting. My impression was that only the CMNS field is treated in that way.

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263) Fraud or not fraud?

Ludwik Kowalski (10/7/05)
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In 1812 Charles Redheffer traveled through Philadelphia and New York, charging a dollar admission (a dollar was a lot of money in these days) to see his perpetual motion machine made up of wheels, gears and pulleys which kept moving continually with no apparent source of energy. He did very well until someone discovered a little man in a back room turning a crank. <www.answers.com/topic/charles-redheffer>

I am probably not the only one who receives many fraudulent proposals over the Internet. Below is one of them; it has nothing to do with cold fusion. But it made me think about a company called iesi (Innovative Energy Solutions, Inc.). So I went to the website of that company <www. iesiusa.com> hoping to find some new information. But I was disappointed; nothing new was added there since my units, #216 and #226, were posted. That was about four months ago. Why is it so? It is also strange that not a single message about Dr. Yang's research has been posted on the CMNS discussion list. Peter Hagelstein, who saw the demonstration five months ago (see unit #237), suggested that I suspend judgment and wait. How long should I wait? According to Dr. Arrison (see the insertion at the end of my unit #229), the impressive prototype was already working in 2004 and "the second plasma device is expected in early 2005." So why didn't a single company scientist come to the 2004 conference in Marseilles to tell us about the impressive cold fusion device? How is it built? How was the 100% reproducibility achieved?

According to <http://peswiki.com/energy/PowerPedia:Cold_fusion> the company is already in the process of commercializing "cold fusion as an energy source." I am a cold fusion researcher trying to keep up with new developments. It puzzles me that the iesiusa website is totally silent. Are they also planning to declare bankruptcy (as Mr. E.B Fasheyiky, see below) after collecting millions from poorly informed investors? That would hurt the reputations of many honest cold fusion researchers, including my own. So far I have not seen any convincing evidence that what the company does is a cold fusion application.

But that is not the view of the editor of Cold Fusion Times. In volume 12, No 12 of this magazine (page 23) I see a report entitled "iESi Technology." The author was probably among those invited, about five months ago, to see the demonstration in Edmonton, Canada. The report states that "the level of power generation was several kW for about 15 minutes" and that the energy gain was "in excess of 5." Assuming that "several kW" stands for 3 kW, the energy generated would be 9 MJ. The gain of 5 implies that 80% of that was due to cold fusion. About 200 grams gasoline would have to be burned to generate the same amount of heat. But what evidence do they have that excess heat comes from cold fusion? How do we know, for example, that it was not due to a chemical fuel, such as alcohol, released into circulating water?

P.S.
Nothing has to be released into the circulating oil because that fluid itself can be used as fuel.

= =

From:Mr.E.B Fasheyiky <fasheyiky1@tiscali.cz> London, U.K

Greetings to you: I am Mr.E.B Fasheyiky and though I reside in the U.K, I am actually a citizen of U.A.E. I am the heir apparent to a family business with its "roots" in Dubai, U.A.E. The importance of my position in my family mandated that I be sent to study for a degree in the U.K. At the conclusion of my degree programme, my family decided to open the first European branch of the company in the U.K and I was put in-charge to run it. This was of course a good beginning for me. At present, we are at the end of another trading year and our auditors have once again brought it to our attention, that the tax rates applicable to our company due to the fact that we the owners are not british is gradually "killing" our business. Unlike the last time I was alerted, I have this time, taken a critical overview of the records from "day one" and the auditors seem very correct. This situation when related to my father has really been disturbing. After much "brain storming", my father has decided to close the UK branch. He has directed that we salvage what is left of our investment and concentrate our efforts on Dubai in the United Arab Emirates.

Now, you must wonder why I have contacted you! I am contacting you now because there is an arangement that I have personally mapped out to ensure that what ever cash remaining in the Company account be transferred to whoever is willing to assist me in this regards. All you will be required to do is to draft an agreement from your end that you will be supplying Medical and Agricultural Equipment to the tune of $18,500,000 (Eighteen Million, Five Hundred Thousand United States Dollars) in my Company's favour. Once this agreement is set up, I will need to see the contents and have a copy submitted to my bankers here so they can immediately begin the transfer processes of the stipulated amount in favour of goods to be supplied (Supposedly) by you.

After the agreement between you and I has been submitted to my bankers, I will file for bankruptcy while the agreement still stands with my bankers. This way, I will be able to leave the U.K back to my Country without having to pay so much taxes on the funds remaining and I will not be loosing much money at the same time. You must understand that I personally have something in it for myself because what I declare to the company in Dubai, is what will be stated and accepted. If you are willing to assist, please respond to this message and bear in mind that your confidentiality is required because if this arrangement fails, I will probably loose everything I own in the U.K. For your assistance, there is a reward. This will be discussed as soon as I get a response from you. Feel free to reach me by email. I will be awaiting your response anxiously. Thank you in view of your anticipated urgent cooperation. Yours Faithfully, Mr.E.B Fasheyiky

Cold fusion researchers I worked with are certainly honest. But are all researchers honest? Probably not. A possibility of honest errors, or self-deception, should also not be ignored. A claim made by an honest researcher is not always valid. Extraordinary scientific claims become valid when other qualified researchers confirm them. Complete openness is essential in science. An invitation to qualified engineers, not associated with the company, to inspect the device, would certainly be desirable at this point.

Inserted on 7/15/07

Scientific people behind the iESiUSA technology are A.I. Koldomasov (from Russia) and H. Yang (from Korea). A set of essays, including my own, about their work can be found at:
http://www.rexresearch.com/koldomsv/koldomsv.htm . This reference was sent to me this morning, by Google

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264) Measuring electric input

Ludwik Kowalski (10/14/05)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

The topic of possible systematic errors, in measuring electric energy supplied to a cold fusion cell, is often discussed on the CMNS list. The purpose of this item is to show interesting extracts from recent messages, to comment on them, and to ask additional questions. Note that item #258, and the appendix in unit #261, were also devoted to this topic. As usual, authors of messages from which I quote are not identified. I refer to them as researchers: R1, R2, R3, etc. I will be happy to substitute these Rs by real names, if requested. Likewise, I will remove a quote if the author asks for this. Please let me know. This issue, by the way, was raised by two young journalists on the CMNS list. They feel that quoting without permission is not consistent with journalistic ethics. I am not a journalist but I understand their concern. Therefore, from now on, items of that kind will not be posted before showing the draft to the list members, and waiting a day of two for possible requests.

Inserted on (10/16/05):
The above paragraph triggered an interesting discussion about privacy. It jad nothing to do with this unit; it had everything to do with ethics. Many old-timers feel that quoting them, even anonymously, is not appropriate. Before showing anything that was posted on the CMNS list to outsiders one must have a permission. I will start followong this rule. But this unit was composed befor the topic of privacy was discussed. That is why I decided to post it. Only one quote was removed because I now know that the author is against anonymous quoting without permission.

R1:
. . . The biggest concern in my view is when folks are simply measuring a voltage and current points and multiplying them, and assuming this represents accurate AC wattage. This is another source of potential systematic error in cold fusion experiments which I've seen time and again. Any good engineer who knows AC power issues will look at the results of that experiment, see the simplistic approach used, and discount the results.

My comment:
In a standard a.c. setup, i.e. when the waveforms are sinusoidal and when the phase factor, cos(phi) is known, the power is I*V*cos(phi), where I and V are effective values read from two instrument. The phase factor can be measured by using an oscilloscope. What R1 has probably in mind is a situation when v(t) and i(t) are not sinusoidal, or when cos(phi) is time dependent. In unit #261 I was addressing a situation in which v(t) remains constant and only i(t) is highly irregular. In that case the approach we used seems to be reliable.

R2 (referring to the approach described in item #261**):**
Note: R2 did not want to be quoted. The essence of the message was that the v(t) and i(t) waveforms contain frequencies excedding the limit of the instrument, 82 MHz. The sample rates must thus be higher than 100 Mhz. He worned us about possible errors associated with the assumption that v(t) was cinstant.

L.K.:
My reply was that we were cautious; voltages were sampled at the rate of 2000 per second and mean values were calculated (and recorded) five times per second. That was like using an oscilloscope. The near constancy of v(t), when i(t) is highly irregular, was possible because a capacitor of 0.01 F was used, as illustrated in Figure 1.The issue of the

sampling rate was discussed in length in item #258. My conclusion was that the frequency of sampling can be as low as one wants, provided the number of random samples is sufficiently high.

R3:
I think all this talk about the difficulty of measuring power only applies to people who are working on a shoestring and do not have enough money to buy a top-of-the-line power meter. I do not think there is any difficulty measuring power when you use professional instruments costing thousands of dollars. With regard to glow discharge experiments, it should be noted that Mizuno measured power with both the Yokogawa and with an ordinary computer-based meter. The results agreed to within 1 percent, and the excess heat was considerably larger than this. The Yokogawa PZ4000 specifications are here: <http://www.yokogawa.com/tm/pdf/bu/pz4000/pz4000spec.pdf>

R4:
It is important when trying to measure power deliver to a varying load that the source impedance of the supply be much lower than the load. The power industry never has to worry about this as their line impedance is zero compared to what we have connected. If not we blow the breaker. We can be fooled by top of the line watt meter if there is not a correct match between the source and the load. If the match is incorrect the watt meter is measuring power that is dissipated in the source and in the load.

In the Mizuno Experiment the load impedance is low and changing rapidly. The impedance of the source must be significantly less than the changing load impedances. If it is not then getting correct power measurement is much more difficult because the impedance of the source must be added to the equations.

R4 (in an earlier message)
I've been interesting in CF since 1989 when fellow engineers at Eberline (Radiation Detection Equipment) in Santa Fe. NM tried to duplicate the P&F experiment without success. Advanced Energy is a manufacture of precision power supplies for the semiconductor industry. These supplies are ideal suited for testing the Mizuno plasma excess heat experiment. The supplies are fully programmable with voltage, current and power regulation. The supplies can deliver 20KW at voltage to 800V. They can also measure Joules delivered to the target at 1% accuracy, sample rates on the order of 1ms. The supply can be programed to run many different types of waveforms. It is also able to suppress arcing within the plasma. I'm setting up an experiment here in either Fort Collins or Boulder any one in the Denver area that is interested in helping is welcomed. I'm basing the experiment on the one done by Fauvarque and Clauzon. This is the web site for the Pinnacle power supply spec.

<http://www.advanced-energy.com/products/ProductDetail.asp?PID={4D7A014D-32FF-4653-AADC-0B9FEB0C9279}&cat=Cat&subcat=Power%20Systems>

R6:
We are making good progress. I agree, many NAE exist. However, I suggest a common mechanism operates in all of them. Based on your model, part of the common mechanism would be the creation of band states that cause wave formation. Other conditions would include an environment that can dissolve sufficient D and an environment that can properly host the resulting He wave. When all of the many required conditions are combined, very few chemical systems would appear to qualify. The BIG questions is, how does a person identify such a system in advance based on your model?

LK:
I am commenting on information posted by R4. Ability to suppress electric arc, for example, in a Mizuno-type cell described in item #261, might be very important. Uncontrollable arcing was the main cause of our difficulties. Let me speculate about one possible consequence of being able to eliminate arcing. The cell receives a measured amount of electric energy, E, and a measured amount of thermal energy, Q, is produces. The coefficient of productivity, COP, is defined as Q/E.

Many different processes occur in the cell, such as dissociation of water molecules, glow discharge, arcing, etc. We do not know which process creates NAE (nuclear active environment) mentioned in the message above. But we can imagine that

E=E1+E2+E3+ . . ., Q=Q1+Q2+Q3+ . . . and COP=COP1+COP2+COP3+ . . .

where 1, 2, 3, etc. refer to individual processes. Suppose that only process #1 (glow discharge) creates NAE whem E1=50 kJ and Q1=200kJ. This would make COP1=4. Also suppose that the second process, arcing, does not generate any excess heat; for that process E2=5000 kJ and Q2=5000 kJ. To shorten this speculation I will ignore other processes. Not able to measure E1, E2, Q1 and Q2 we measure E=E1+E2=50+5000=5050 kJ and Q=Q1+Q=200+5000=5200 kJ. Then we conclude that CPU=5200/5050=1.03. This shows how much would be gained, in this hypothetical situation, if arcing could be suppresses. It should be much easier to argue that the COP=4 is real than to argue that CPU=1.03 is real. Furthermore, assuming the test lasts 5 minutes, the power without the arc would be 167 W while the power with the arc would be 16,833 W. Designing a cell for 5 minutes of electrolysis with arcing is certainly much more difficult than designing a cell in which the arc is suppressed.

R1:
I've read the posts regarding measurement of "watts in" in complex AC/DC high-voltage systems. ..... R5 and I spent many months investigating this issue and found that it is somewhat of a black hole. We're of the opinion that such measurements cannot ever be accurate or dependable, regardless of the sampling rate. The problem is that in high-voltage, high-frequency systems, the power factor of the AC current is quite unpredictable. Voltage and current are NOT the only variables that lead to wattage. Power factor and phase are a major issue, and they are extraordinarily complex. > . .

Another problem is that conversion efficiency of the power supply. ..... Sometimes the power supply is 60% efficient, sometimes 50% efficient, and you cannot predict that efficiency at any moment. Our solution was to abandon the entire concept of "measuring" AC power as a product of voltage and current. We simply don't try to do it. We adopted a simpler and we think more accurate approach. We have designed our systems so that the power input to the system is always low-voltage DC (6 to 40 volts) -- this can be be accurately and reliably measured as a power input.

Then we convert that DC to high-voltage AC within our apparatus, and the converting electronics are enclosed in a separate and unique calorimeter. This allows us to precisely measure the energy loss of the electronics. By the way, this is also the method that the vendors of wattmeters use to calibrate and test their wattmeters. Ultimately, measurement of heat output via calorimetry is the most "true" form of watt meter. So why not just use that method to start? The above method has proven to be very simple, reliable, and robust for us. But, your mileage may vary.

L.K.:
I know that R4 and R5 work with a gas glow discharge cell. Could their approach be used in working with Mizuno-type electrolytic plasma cells? I do not think so. Here is my reply to the above message. Suppose you begin with a d.c power supply whose voltage is very stable, for example, 60 V. The voltmeter shows volts and ammeter shows current feeding the next step. It the current is 5 A then P1=300 W. The next box is a high voltage power supply. It receives electric energy at the rate of 300 W and uses it to generate, for example, 500 V. You apply this to your gas glow discharge cell. Right?

At what rate is the cell receiving electric energy? The rate is P2=P1-X, where X is part of P1 that is converted into heat in your high voltage supply. You must know X to calculate P2. That is why your entire high voltage power supply is located in a calorimeter. That box has only four wires connected to it, two at the input and two at the output. Input receives electric energy at the rate P1 and the output delivers it to the cell at the rate P2. After waiting many hours the temperature of the calorimeter stops rising and you calculate X. Suppose it is 50 W. You know that P1=300 W because your d.c. is 5 A. Therefore, you know that P2=250 W.

That is a good method when the glow discharge is very stable. But suppose it is not stable because things change inside your cell. You are aware of changes because the d.c. is going down or going up. Your ammeter reacts to all changes at once but your calorimeter takes hours to react. Suppose that the d.c current changed from 5 A to 10 A. Thus P1 becomes 600 W, for exactly 10 minutes. Then the current becomes 7 A and P1 becomes 420 W. Your experiment ends 30 minutes later. How many joules of electric energy was supplied to your cell during the last 60 minutes? I do not think that the answer would be correct if you used the same X at all P1. That is a limitation of your method. I

suppose it is highly reliable when P1 remains constant during the entire experiment. To conduct an experiment at a different cell voltage you would have to wait many hours to find the new value of X. Do you agree?

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265) A paper of Antonella, Antonio, Antonietta, Emilio and Guiliano

Ludwik Kowalski (10/20/05)
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How can cold fusion be explained? The answer to this question can be found in the appendix of a paper of Antonella De Ninno et al. “Experimental evidence of 4He production in a cold fusion experiment.” The pdf file containing this paper was sent to me (October 2005) by Jed Rothwell. Thanks, Jed. Imagine molecules of air through which sound is propagating. They oscillate with amplitude which increases with loudness. Layers of compression and layers of rarefaction follow each other as water crests in a wave propagating along the surface of a pond or pool. Oscillations of sound, however, are in the direction of propagation, not perpendicular to it, as in familiar water waves.

Looking on two nearby molecules, oscillating along the direction of sound propagation, one would see that the distance between them tends to increase and decrease periodically. Something similar might occur in palladium loaded with deuterons. The average distance between the two nearby ^{2}D ions is too large to allow fusion but the nuclei can occasionally fuse when distances are close to the minimum. But this is only part of the story. The other part has to do with answering a different question: “why are cold fusion products,are different from the hot fusion products? Most CMNS products are ^{4}H while most CF products are n, p, ^{3He} and radioactive ^{3}T.”

The authors say that, according to QED (Quantum ElectroDynamics), sudden change is expected to occur in Pd when atomic concentration of dissolved ^{2}D ions exceeds 0.7. Deuterium ions start oscillating “inside shallow potential waves . . . [and] “the Coulomb barrier turns out to be smaller than in vacuo, because of the high concentration of the negative” electrons. That is the answer to the first question “why does cold fusion happen?” Addressing the second question the authors describe cold fusion as a three step process:

a) Two ^{2}D ions fuse forming the excited compound nucleus ^{4}He. The well known excitation energy is 23.8 MeV.
b) Rapid “cooling” -- decay of the excited nucleus to the ground state
c) Transfer of the released energy to the Pd crystal.

The step (a) must occur in any kind of fusion (in gas-like plasma and in condensed matter). But steps (b) and (c) are very different in these two kinds of fusion. In plasma deexcitation , most often, takes place by by fragmentation of the compound nucleus. It breaks into either $n+^{3}He$ or $p+^{3}H$. Deexcitation by emission of gamma rays is also possible but it is much less probably. In condensed matter, on the other hand, fragmentation is prevented by close proximity of charged particles, negative electrons and positive ions. The released energy goes to charged particles, mostly to surrounding ions. Metallic grains in which fusion occurs become very hot, momentarily, and loose heat by well known mechanisms (radiation, convection and conduction). Helium gas is most often released from very hot “micro furnaces.” The appendix end with this sentence: “From our ‘prejudice’ that almost all fusion occurring in the Pd lattice should produce ^{4}He, we derive that the easiest way to evaluate the energy output is to count ^{4}He atoms in the cell gasses.” And that what the main part of the paper is about.

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LET ME APPEND A MESSAGE I RECEIVED TODAY FROM ONE OF THE READERS.

Dear Dr. Kowalski-

The more I examine the information about cold fusion, the more it appears to me that the most probable explanation of the phenomenon is the Palladium catalyzed conversion of D:D to He4 along with an overlooked concurrent Pd catalyzed conversion of H:H to Deuterium atoms. It is very possible that the electrical field is not necessary and the electrolytic cell is serving only to furnish D:D for conversion.

It is possible that Hydrogen gas forced through a thin sheet of Palladium would show Deuterium enrichment on the other side and a temperature rise of the Palladium barrier. Similarly, if Hydrogen gas heavily enriched with Deuterium were forced through a Palladium barrier, it would not be surprising to see heat, Tritium, He3 and He4 produced. In so far as I can tell, no one has considered the molecule to "atomic isomer" transformations as a possible explanation of the phenomenon.

If one looks at the published data re size and mass of the electron and the proton one finds an interesting situation. We usually think of the proton as being large and massive and the electron small and light. However, if the electron were scaled up to a 1 cm. water balloon, the proton would be more like a 100 meter soap bubble! You're undoubtedly a better mathematician than I , so you probably can work out a better comparison than that. The point is that the mass and charge density of the electron is many times that of the proton, indeed, the disparity is so great that there seems no reason at all that electrons cannot pass freely through protons. Similarly, two protons would have to approach to about the diameter of an electron to experience the same repulsive forces as two electrons colliding. In other words, there is a fair chance that a Hydrogen Molecule spends at least some time in the same configuration as a Deuterium atom, but does not stabilize to that form because of the vibrational energy content...,.(This could be considered as essentially the same reason that absolutely pure gaseous water will not condense to liquid droplets .....) The same kind of analysis applies to the D:D, He4 system and the H:D to Tritium possible conversion....Best Wishes, Dean L. Sinclair, BA, MS, PhD

From previous correspondence with Dean, whose life circumstances are not favorable to conduct research, I know that he would be happy to receive comments. Here is his email address: <deanlsinclair@gmail.com>

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Let me also share the URL where Bill Beaty, a friend, and a longtime observer of the CMNS field, tells us how to recognize fraudulent technological claim. I think that potential investors should benefit from reading his piece. Share this URL with them:

<http://amasci.com/freenrg/fnrg.html>

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266) First scared then reassured.

Ludwik Kowalski (11/1/05)
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The experiment described in item #261 is still in progress; without my participation. In about ten days I will be participating in another version of that experiment. The advantage of the second version will be Pinnacle, a very unique power supply. It will allow us to nearly instantly (several microseconds) suppress arcing, if it develops. Several days ago my collaborator sent me a message with a question about the latent heat of vaporization, L. “Do you know what L values were used by Clauzon in their
paper?” Replying to this question I wrote that L was 2260 J/g plus a small correction needed to raise the temperature of 20 C water to the boiling point. The correction was necessary because 20 C water was constantly added to replace the evaporated water.

After answering I started to think about a possible interpretational errors in the determination of excess heat from the amount of water evaporated. Latent heat is determined by attractive molecular forces between the escaping molecules and remaining liquid. In other words, L is determined by the surface tension. Suppose that water is not pure and that a thin layer of "something" is present on its surface. Is it not reasonable to assume that the presence of that "something" might change the effective value of L significantly? Suppose that the presence of that "something" lowers the L by 50% but a researcher is not aware of this. Instead of multiplying the mass of the lost water by 1130 he multiplies it by 2260 and concludes (wrongly) that the amount of released thermal energy is much larger than the amount of received electric energy. That would be an illusion of excess heat.

Is the above scenario possible? Can the surface tension of water be lowered significantly by "something?" What can that "something" be? Can it come from the salt used to make the electrolyte? Can it come from the body of the cell, etc.? Can it come from products of reactions taking place near the cathode? These questions were actually posted on the CMNS discussion list. Fortunately, about 24 hours later, somebody responded by emphasizing that vaporization, at the boiling temperature, takes place deep below the surface. The lowering of the surface tension by an additive would be expected but this should have no effect on the L that must be used to calculate excess heat. I also learned that ionic compounds tend to increase the surface tension of water while organic compounds added to water tend to lower it very significantly.

Responding to this reassuring comment I wrote: “I agree with you. Bobbles are indeed formed below the surface, as one can see, and surface tension should not play a role. This, however, is not true when evaporation takes place below the boiling point. At 40 C, for example, one cannot see bubbles coming from the bottom. Therefore I expect L to depend on the surface purity at temperatures below boiling. Fortunately, this has nothing to do with measuring of the COP (Coefficient Of Performance) in Mizuno-type experiments. Thanks for restoring my confidence in the validity of methodology described by Fauvarque et al.” But a day later I started worrying again. Here is my next posting on the CMNS list.

“Evaporation of water (below the boiling temperature) is a surface phenomenon. Boiling, as emphasized by X, consists of bubbling that takes place in the entire volume. Is it just a coincidence that the latent heat of evaporation and the latent heat of boiling are identical? Slow evaporation seems to be reversible, fast boiling is not reversible. (I have never seen bubbles forming at the surface and going down to condense near the bottom.)

By the way, a large fraction of bubbles is produced near the very hot cathode. Some bubbles are probably born as

chunks of supercritical water (neither liquid nor vapor). Is it possible that supercritical evaporation, if not accounted for, might lead to the lowering of the effective L? Recall that L approaches zero when the critical point is approached in the P,T diagram. Lowering of the effective L could possibly create an illusion of excess heat. Evidence of extremely high temperature (sufficient to melt tungsten) was clearly visible when our cathode was examined under the microscope."

The answer, if any, will be paraphrased below; unless I decide to ask for permission to quote the reply. The responsibility of addressing such issues -- the so called burden of proof -- should be an obligation of those who make a discovery. A month from today I will be presenting results of an ongoing study at a conference in Japan. The issue of the "effective L" seems to be central when the amount of excess heat is deduced from the amount of lost water. Was that issue addressed by those whose conclusions we are trying to either confirm or refute? I do not think so.

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267) Searching for excess heat in Mizuno-type plasma electrolysis

Ludwik Kowalski (a), Scott Little (b), and George Luce (b)

(a) MSU (Montclair State University), Montclair, New Jersey, USA.
(b) ETI (EarthTech International, Inc.), Austin, Texas, USA.

Abstract:
Excess heat generated in the glow discharge plasma electrolysis, first reported by Mizuno and Ohmori (1), has been studied by several researchers, both in Japan (2, 3, 4) and in other countries (5, 6, 7, 8). Most reports, but not all, confirmed generation of excess heat. Facing this situation we decided to replicate the most recent experiment (8) in which excess heat was reported to increase with voltage. Our results do not confirm reality of escess heat. An additional experiment confirming this conclusion will be described in unit #270.

Introduction:
Suppose that a common immersion heater is inserted into a beaker with water. How should thermal energy released in the container depend on the amount of electric energy supplied? The answer is in the law of conservation energy. The amount of electric energy, E, supplied to the cell must be equal to the amount of thermal energy released, Q.

$E = Q$. ..... (1)

But in the setup described in (8) the right side of the above equation was reported to be significantly larger than the electric energy on the left side. The authors believe that some hidden energy, presumably nuclear, is released during the plasma electrolysis. That is the essence of the excess energy claim. Another way of expressing the claim is to say that the coefficient of performance (COP), defined as Q/E, is larger than unity. According to (8), the COP at 350 V is between 1.31 and 1.41.

In this experiment, where the electrolyte is always boiling, part of the heat energy produced goes into vaporizing water (at the rate of 2260 joules/gram) and the remainder is lost from the cell through conduction and radiation. To determine the heat output in this experiment we measure the amount of water evaporated, multiply by the heat of vaporization (i.e. 2260 joules/gm) and then add on a correction to compensate for the nonevaporative loses of heat. Expressing this in terms of power (i.e. the rate at which energy is delivered and released):

$P_{out} = dm/dt * 2260 + P_{loss}$ .....(2)

where P_{out} is the total heat power produced in the cell, dm/dt is the rate at which mass is lost by the cell due to vaporization, and P_{loss} is the heat power lost through conduction and radiation.

To determine P_{loss}, we conducted a series of tests in which the electrolyte was boiled with an ohmic heater at various power levels. Comparing the actual mass loss to the electrical energy input revealed that, as in (8), the heat loss through conduction, and radiation is reasonably constant and independent of the electrical input power. That stands to

reason because the cell is always operating at ~100C and always has approximately the same level of electrolyte in it. As detailed below we observed a P_{loss} value of 94 +/- 10 watts from our cell.

The COP can also be expressed in terms of power as follows:

$COP = P_{out}/Pin.$ .....(3)

where Pin is the electrical input power.

Experimental setup

As described in (9), this experiment was prompted by desire to either confirm or refute reality of excess heat as reported in (8). That is why our setup was very similar to that used by Fauvarque et al. Approximately 725 cm^3 of the K_2CO_3 electrolyte (0.2 M) was placed into a polycarbonate vessel whose depth and the inner diameter were 32.0 cm and 11 cm, respectively. The initially anticipated setup, shown in (9), had to be modified to match (8) as closely as possible. Instead of a spiral Pt anode we used a cylinder made from the platinized niobium mesh. The height of that cylinder was 5 cm and its diameter was 6.3 cm. The anode was a 2.38 mm diameter tungsten welding electrode containing 2% of thorium. Both electrodes, and the steam-escape PVC pipe, were mounted on the lid of the main container. The entire setup was supported by a sensitive scale, as shown in Figure 1. "The fresh water reservoir, also mounted on the lid, provided periodic refilling of the vessel. In our case, as described later, the refilling would not proceed as anticipated. A plastic baffle, near the steam exit, was used to shield the pipe from occasional splashes of the electrolyte foam and liquid.

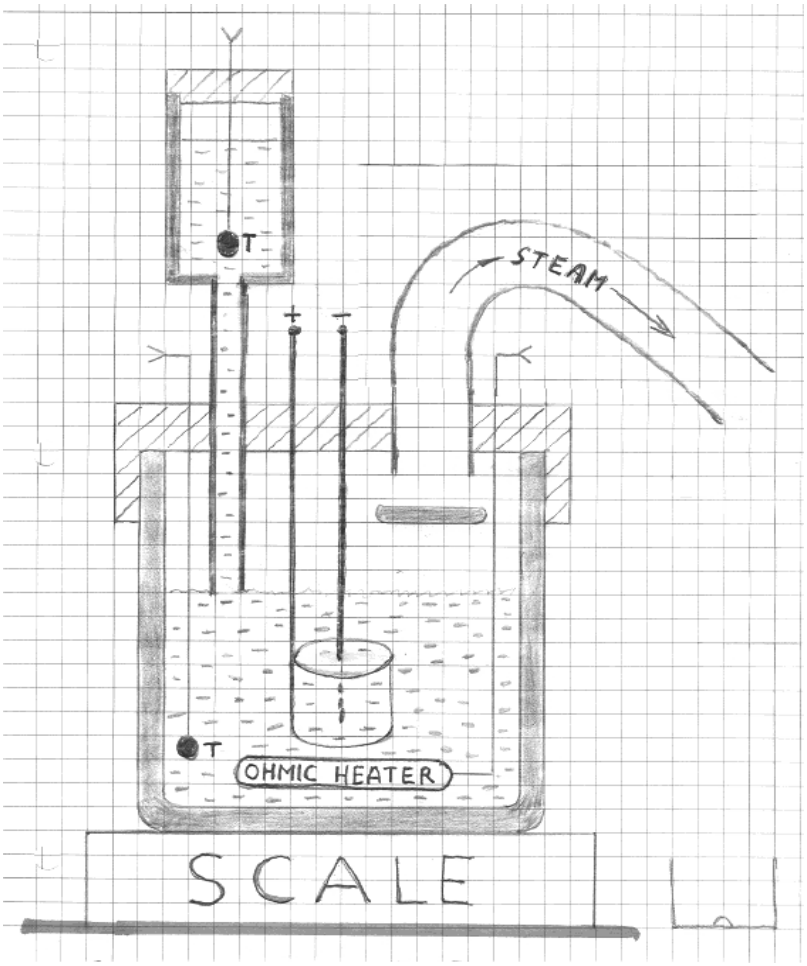

Figure 1
Schematic diagram of our setup. Some condensation is expected to occur on the inner surface of the escape pipe, mostly at the section where the temperature is the lowest. Drops of condensed water could occasionally be seen

dripping into a collector. Temperatures were monitored with two thermistors, T.

The intensity of splashing during the high voltage electrolysis, had a tendency to increase with the potential difference between the electrodes. Unless mechanically reduced, such splashing could possibly create an illusion of a voltage-dependent excess heat. The same kind of illusion could also occur if invisible droplets of foam, mixed with steam, were ejected. Ideally only water vapor (plus some hydrogen and oxygen) should escape from the boiling liquid. The photographs of the entire setup are shown in Figure 2 and 3. Potasium carbonate, used in common detergents, decreases the surface tension of water and causes foaming (10). Foaming interfered with constant replacements of evaporated water, used in (8). The head space in our well-closed cell contained almost nothing but hot water vapor mixed with foam. When bubbles would start up the refill tube so as to permit water from the reservoir to be displaced into the vessel, the bubbles would rapidly collapse and disappear completely upon entering the cooler water in the refill tube. However, when we turned off the power to the cell briefly cooler water vapor would successfully enter the reservoir and allow the refilling to occur. For that reason replacements of evaporated water (typically 30 grams) were preformed manually before the tests. The electrolyte was then brought to the boiling point by using the ohmic heater. Masses of water evaporated in consecutive tests were electronically determined (once every second) from the readings of the scale. Flexible conductive filaments were used to connect electrical components mounted on the lid with outside circuits. This did not interfere with precise measurements of masses (better than 0.5%).

Figure 2
The photo of the setup showing the foam above the electrolyte. The dark cushion below the vessel helped to prevent bouncing of the scale. The inner diameter of the white PVC pipe was about 22 mm.

= =

Figure 3
Electrodes and other components mounted on the lid. The ring below the anode is the ohmic heater, the vertical glass tube on the right contains the thermistor. The transparent antisplashing buffle can be seen below the lid. The side screws were used to mount the lid rigidly on our polycarbonate cell (note the deep groove on the lower surface of the lid). The tip of the tungsten cathode is shown in Figure 4 below.
= =

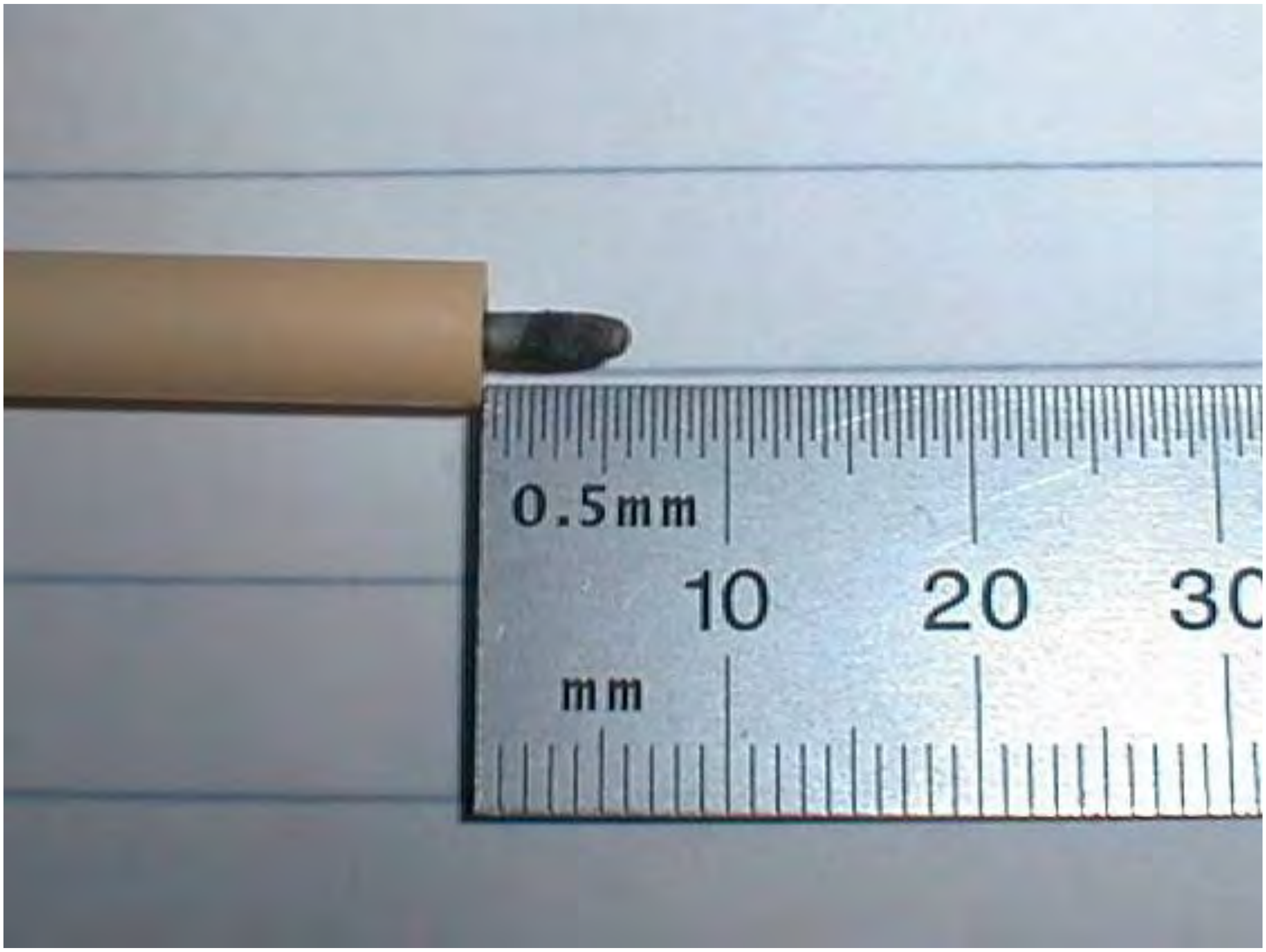

Figure 4
Appearance of the cathode, originally extending 13 mm beyond the ceramic sheath, after a 350 V plasma test (see below) lasting 3 minutes. Typical current densities were in the range from about 3 to 10 A/cm^2.
= =

The ohmic heater was used to measure the P_{loss}, and to preheat the electrolyte before plasma experiments. A computer-based data acquisition system was recording average electric currents, i(t), average potential differences, v(t), and the remainng mass m(t) once every second. The Clarke-Hess 2330 Power Analyzer measured power delivered to the cell by simultaneously sampling v and i at about 2000 Hz, multiplying the pairs of readings together, and averaging the results over several seconds. The data acquisition system interrogated the Clarke-Hess once per second to obtain the lastest values for rms I, rms V, and power. The electrical setup is shown in Appendix 1.

Ohmic heater results
The d.c. power supply of our ohmic heater was similar to that used during the plasma electrolysis. Time dependencies of E and m, at constant electrical power, are shown in Figure 5.

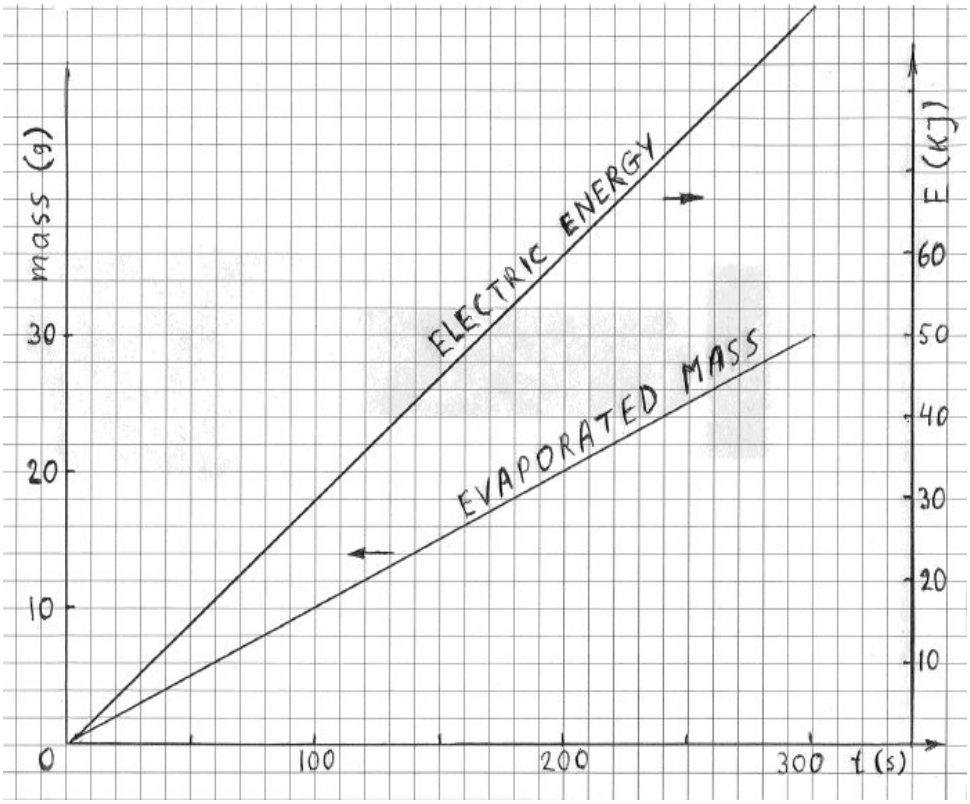

FIGURE 5
Time dependence of the energy received (P_{in}*t) and of the evaporated mass, m(t), at P=300 W using the ohmic heater.

The rate of evaporation, dm/dt, was also constant, as illustrated. The values of P_{in}, P_{out} and P_{loss}, extracted from this figure are shown in the line 4 of table below.

Table 1 (Ohmic heater results)

Pin	Pout	Ploss	Pout+Ploss	COP*
201	107.3	93.7	201.7	1.00
230	131.9	98.1	226.3	0.98
262	184.9	77.1	279.3	1.07
300	208.1	91.9	302.5	1.01
364	280.2	83.8	374.6	1.03
479	377.1	101.9	471.5	0.98
486	380.5	105.5	474.9	0.98
565	461.6	103.4	556.0	0.98

* The last column shows what the COP's would be if they were calculated by using the mean value of Ploss, 94.4 W, instead of numbers from column 3.

That table summarizes results of eight measurements; powers in lines 1 to 3 and 5 to 8 were calculated from the time dependencies similar to those in Figure 5. Note that the third column displays differences between the numbers in column one and two. These are values of nonevaporative powers, P_{loss}. As in (8), we found that the values of P_{loss}, are essentially identical for all P_{in}. This is illustrated in Figure 6. The mean value and standard deviation of P_{loss} turned out to be 94.4 W and 9.9 W, respectively. The last column shows that the COP fluctuations match experimental uncertainties in the third column. Note that the variations in COP using the ohmic heater are essentially the same as they are with the plasma going (last column in Table 2 below).

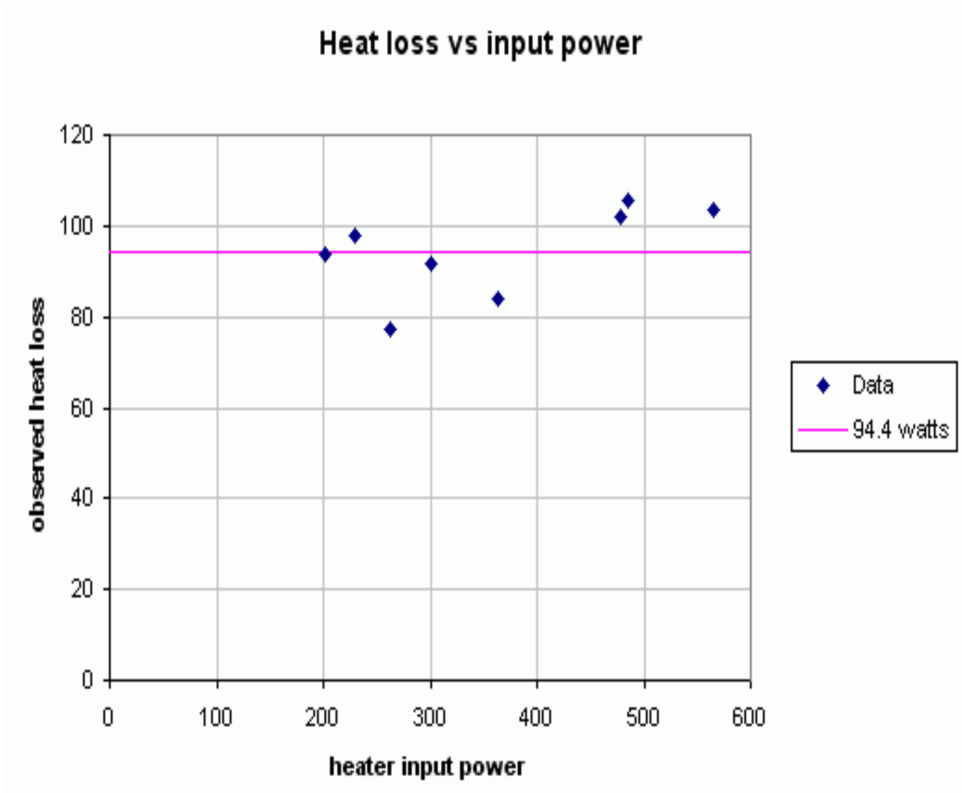

Figure 6
Dependence of P_{loss} (conduction and radiation) on the electric power, P_{in}.
=======================================

In performing these experiments we discovered that the potassium carbonate electrolyte tends to creates foam above the boiling surface. This is illustrated in Figure 2. At first we though that the foam was due to chemical interactions between the electrolyte and cell materials (walls, partially coated electrodes, the lid or the PVC pipe). But foaming was also observed when the electrolyte was boiled in a clean pyrex beaker. Foaming seemed to be less intense during high voltage electrolysis. Occasional accumulation of the foam resulted in its sudden ejections. Fortunately, a setup in which all measurements are recorded once a second can identify ejections as sudden mass changes. After discovering nonevaporative ejections we watched the foam and stopped the runs when foaming became excessive. In that way nonevaporative losses of mass were totally eliminated.

High voltage electrolysis results

Curiously, the foaming seems to be worse during ohmic heating than during plasma operation. It seems that the shocks of the plasma action are breaking the bubbles faster. Table 2 is the summary of experimental plasma electrolysis results. The most important numbers are in the last column; they are the values of COP calculated according to the formula (5). The mean COP, and the standard deviation, turned out to be 1.00 and 0.02, respectively. The table shows that the value of the electric power, P, is not predetermined exactly by the applied voltage. This is probably caused by progressive decomposition of the cathode and by chemical changes taking place during consecutive tests.

TABLE 2 (Voltages, Powers and values of COP's)

volts	P(electric)	Pv (evapor)	Pc	Pe + Pc	COP
250	249.0	155.5	94.4	249.9	1.00
250	338.0	240.8	94.4	335.2	0.99
300	626.4	512.6	94.4	607.0	0.97
300	521.0	424.4	94.4	518.8	1.00
300	563.0	466.4	94.4	560.8	1.00
325	718.8	640.3	94.4	734.7	1.02

350	998.2	934.8	94.4	1029.2	1.03
350	859.7	748.0	94.4	842.4	0.98
350*	331.2	246.9	94.4	341.3	1.03
400*	323.9	243.5	94.4	334.0	1.03

*The last two lines in Table 2 refers to experiments in which the concentration of the electrolyte was reduced drastically (from 0.2M to 0.02M). This reduced the foaming and sloshing inside the cell significantely, most likely due to the unusually low electric electric current (low P). The cathode, however, still achieved a bright incandescent temperature. Thus, even at 400 volts, the COP was still very close to unity. Time dependencies of P_{in} and P_{out} associated with two lines of the table are shown in Figure 7. As in Figure 5, one can see that the evaporated mass increases linearly with with time, according to the value of P_{in}. The values of P_{out}, during plasma electrolysis, are not as constant, as it was when the 360 W ohmic heater was used. At point B that heater was turned off manually and the potential difference of 300 V was applied to electrodes. The power level jumped to nearly 600 W and the rate of evaporation increased accordingly. The voltage v(t) remained constant but the power drifted slowly toward nearly 500 W. Rapid fluctuations of electric power (plus or minus 5%) were due to fluctuations of electric current, i(t). At point C the voltage was increased to 325 V and the power level jumped to nearly 700 W. The rate of evaporation increased accordingly. The direction of the power drift changed and the intensity of rapid fluctuations increased (to about 12%).

The data collection was stopped manually at point D because of intensive foaming. The trend toward more rapid drift and stronger fluctuations of $P_{in}(t)$ continued when potential differences were increased to 350 and 400 V. This was associated with intense arc-like explosions inside the vessel. Obtaining reproducible data became more and more difficult above 350 V. Excessive arcing was unpredictable but short time intervals without excessive arcing at higher voltages -- our rare “windows of opportunity” -- were sometimes available to make measurements. No evidence of excess heat at higher voltages (up to 400 V) emerged from these quick explorations.

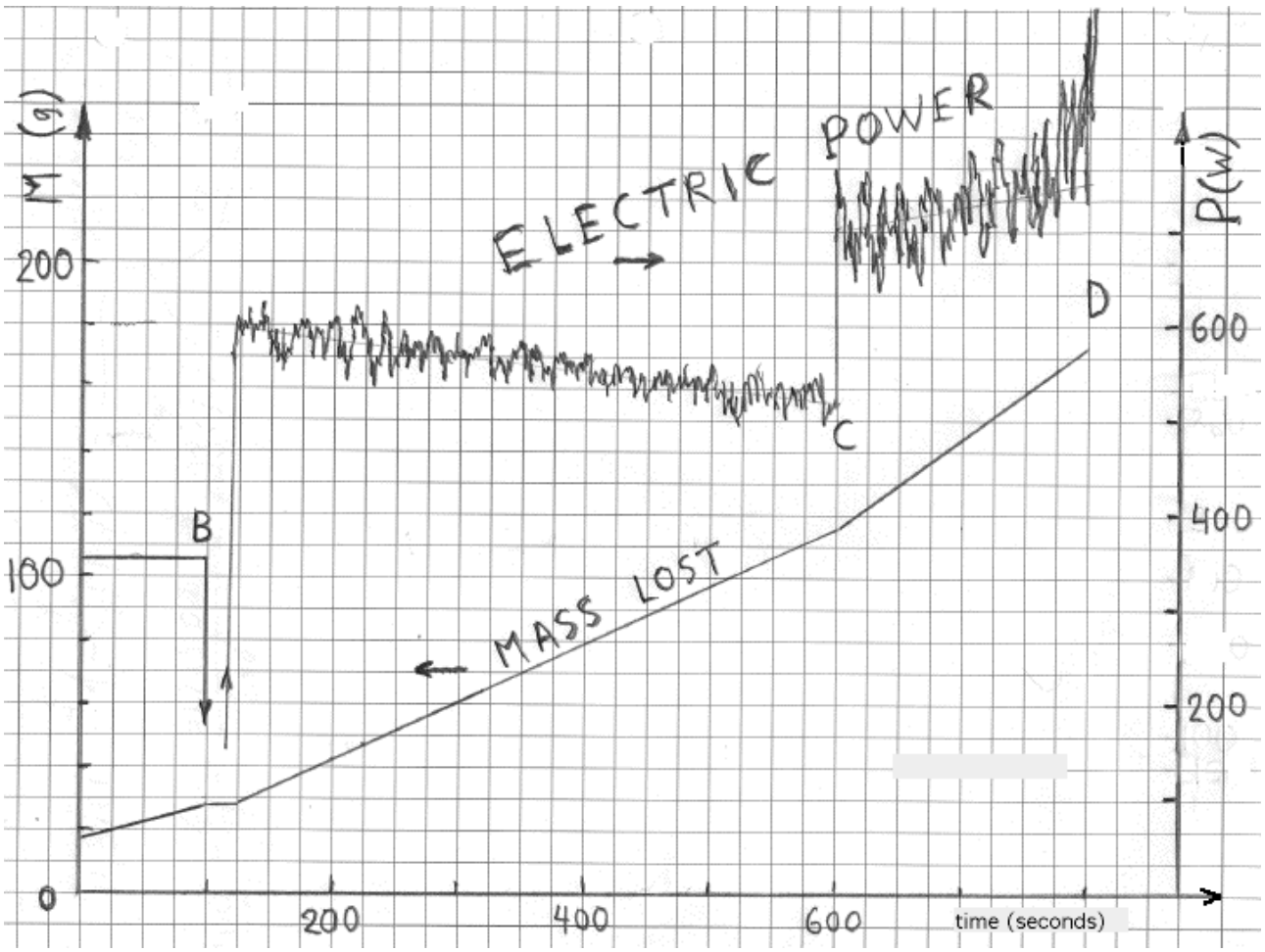

Figure 7

Time dependence of the electric energy received and of the mass of evapoated water at 300 V (between B and C) and 325 V (between C and D) during plasma electrolysis. Note that the values of $P_{in}(t)$, at constant voltages, were not as stable as when the ohmic heater was used to sustain boiling.
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Discussion:
Our experiments show no sign of the large excess power reported in (1,2,3,4,5,7, and 8). But they confirm findings reported in (6). It is remarkable that the COP's at 200 V and 250 V, reported in (8), are very close to ours. Discrepancies, however, become progressively larger at higher voltages. Splashing of the electrolyte also becomes more significant at higher voltages. This suggests that unnoticible loses of the electrolyte, from an open vessel used in (8), might be responsible for differences between the two set of results. Other discrepancies are worth mentioning. According to (5), the excess energy can be much larger than in (8), even below 200 volts. The COP of the cold fusion reactor of Naudein (CFR1.1), for example, was reported as 1.85 at 150 volts and about 2.5 A. The electrolyte used in CFR was exactly the same as in (8). Let us also mention that the COP's values between 1.2 and 1.4 were recently reported in (7). The corresponding voltages, however, were not specified. The issue of foaming, most likely associated with formation of CO_2 at boiling temperature, (11) was, unfortunately, not addressed in (5), (7) and (8).

The challenge presented by ejection of liquid water is indeed very serious becase the latent heat of evaporation is large. Mist ejection, at the rate of 50 milligrams per second, results in the overestimation of P_{out} by 2260*0.05 = 113 W. This, however, is only one possible explanation of discrepancies. Another might be associated with microexplosions we occasionally observed during plasma electrolysis, expecially at higher voltages (see Appendix 3). Such explosions are accompanied by loud popping noise and very intensive arcing. We suspect that escaping hydrogen and oxygen occasionally combine under the influence of arcing. That could be a possible non-nuclear source of excess heat repored by several researchers.

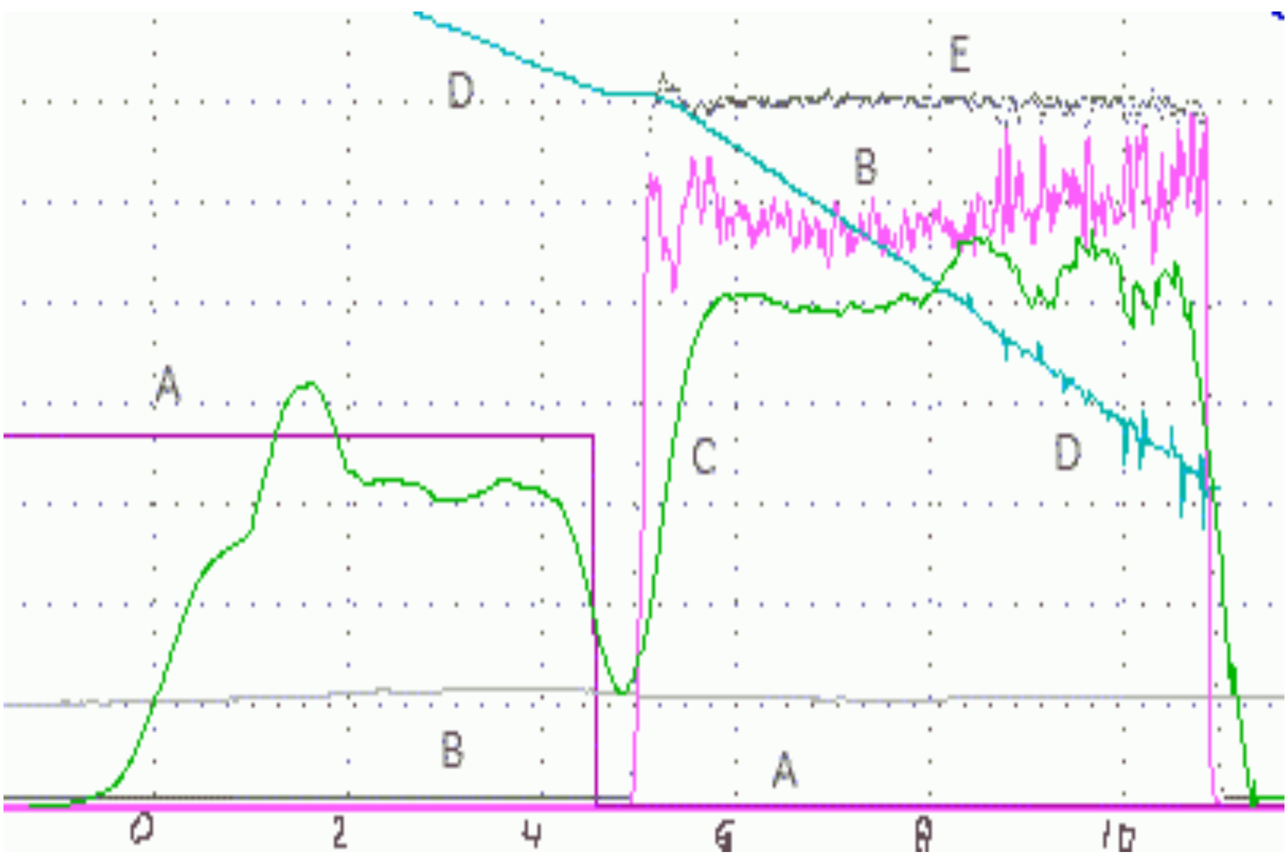

Figure 8
Time dependencies of several parameters. Trace A is the ohmic heater power, initially 360 W and then zero. Trace B is the P_{in}, the electric power during plasma electrolysis. It was initially zero and then fluctuated near 700 W, more or less

as in Figure 8. Trace C, shows the values of P_{out}. Unilke other traces on this figure, each data point along the C is the average power from many intant powers (30 seconds on the left and 30 seconds on the right). That explains why oscillations along the trace C are relatively slow. Note that the boiling temperature was reached shoerly befotr t = 0. Trace D is the decreasing mass of the cell; it can be used to calculate instant the rate of evaporation. Trace E shows that the applied voltage, 350 V, remained essentially constant. The unlabeled gray trace is irrelevant, it shows the temperature in the water reservoir (above the cell lid).
= =

Anticipating a possibility of hydrogen burning our tests were manually interrompted as soon as arcing became extensive. Fortunately, prolong arcing is accompanied by thermal instabilities (large fluctuations in Pout) that can be recognized when data are analyzed after experiments. This is illustrated in Figure 8. It shows that P_{out} was stable between t=6s and t=8s. After that it became unstable, due to arcing. Microexplosions can also be identified by oscilations along the mass trace, D. They are caused by dynamic effects on the scale during microexplosions. In this case data at t > 8s were ignored.

INSERTED on 11/16/05:

1) A different description of the above experiment has been posted at the EarthTech website: <http://www.earthtech.org/experiments/Inc-W/Fauvarque>

2) Richard Slaughter, an electrical engineer in Boulder, Colorado, set up an experiment similar to that set up by Scott Little. I participated in his experiments. This work is described in unit #270 (to be posted in a day or two). The COP values, as you will see, remain very close to unity, even above those listed in table 2.

APPENDIX 1: Measuring electric energy

Electric energy delivered to a cell, is the integral (over a test duration) of i(t)*v(t)*dt. The instrument we used, Clarke-Hess model 2330 power analyzer, sampled the v(t) and i(t) about 2000 times per second. As expected, the voltage between the cell electrodes, v(t), was essentially constant but the current i(t) was highly irregular. Digitized samples of v(t) and i(t) were recorded by the computer and the cumulative input energy was displayed during the data acquisition, as shown in Figure 7. A highly irregular nature of the i(t) waveform guarantees random sampling and the average energy, over a time longer than several seconds, is highly reliable. To verify this we also used an a.c. watt meter at the input of our d.c. power supply. The a.c. energy measured with that instrument was only slightly larger than the d.c. energy. The difference, was consistent with what is necessary to operate the power supply when it is not loaded. Electrical connections are shown below. The Clarke-Hess meter adjusts the sampling frequency to avoid beats with the current and voltage signals.

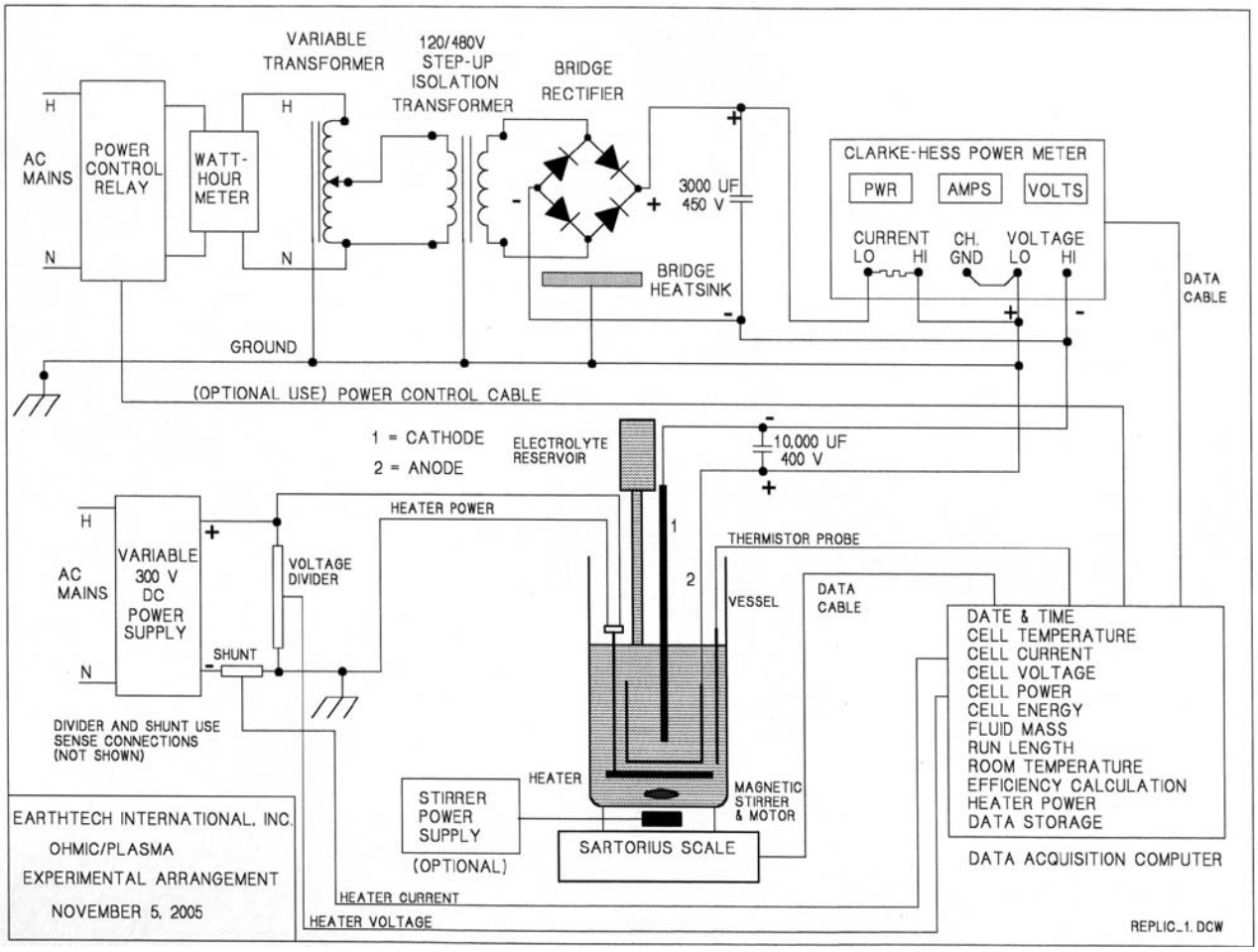

FIGURE 9
Electrical connections.

APPENDIX 2: Discussing Condensation of Steam

Is it possible that condensation of steam inside the cell, for example on the inner walls and on the lid, is responsible for the underestimation of excess heat? The answer to this question seems to be negative. First, condensation is likely to take place near the outlet of the exhaust pipe where the temperature is relatively low. Water condensed near the ending of the pipe does not return to the electrolyte. Second, consequences of condensation, if it were significant, are not as serious as one might think. Heat released in condensation of one gram of steam is the same as heat used to evaporate one gram of water. Thus each gram of condensed water brings back 2260 Joules of heat. Heating of the electrolyte by condensation is not distinguishable from heating it electrically. Thus, in the first approximation, condensation has no effect on the CPU. In reality some heat released in condensation is removed by conduction and convection. But, as in (8), the nonevaporative losses, P_{loss} were measured. In other words, the effect of condensation on COP is practically eliminated. Unfortunately, this cannot be said about splashing; unaccounted splashing might lead to exaggerated values of CPU, and to an illusion that the CPU increases with voltage.

APPENDIX 3: Microexplosions

Using 0.2M K_2CO_3 electrolyte, our cell consumes nearly 1000 watts at 350 volts. The resulting plasma is so violent that the electrolyte sloshes all over the place and periodically exposes the cathode. When that happens an arc, possibly accompanied by a H_2 +O_2 explosion occurs and a small hole is melted in our anode grid. The shock from these explosions is so violent that, even with our present shock-mounting of the cell, the Sartorius balance is unable to provide stable mass readings, thus ruining our data collection.

Despite these difficulties we have tried running at 350 volts about 6 times now (our Pt-coated Nb mesh anode has about 50 small holes in it now). On most of these attempts something inside the cell broke apart or the balance was so disturbed by the shocks that its data was meaningless. But we were lucky enough to catch the cell "behaving" well enough on two of these runs and we got "reasonable measurements" from them.

Another problem is that 1000 watts is out of the power range (0-550 watts) that we can explore with our existing ohmic heater. Thus we are forced to extrapolate the heat losses and that could be erroneous. On the other hand, if we dilute the electrolyte to 0.02M K2CO3, then the cell consumes about 300-400 watts at 350 volts and all of the measurement strategies we developed specifically for this experiment work satisfactorily.

To date we have made three "good measurement" runs at 350 volts. The COP's observed on the two runs with 0.2M K_2CO_3 were 0.98 and 1.03. The COP observed with 0.02M K_2CO_3 at 350 volts was 1.03. As it turns out none of these COP's is significantly different than 1.00.

References:

1) Mizuno, T., Ohmori, T., Azumi, K., Akimoto, T., Takahashi, A. "*Confirmation of Heat Generation and Anomalous Element Caused by Plasma Electrolysis in the Liquid*. in *8th International Conference on Cold Fusion*." 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy. Downloadable from the library at <http://www.lenr-canr.org>

2) T. Mizuno, T.Ohmori, T. Akimoto, and A. Takahashi. "*Production of Heat During Plasma Electrolysis."* Jpn. J. Appl. Phys. A, 2000. **39**: p. 6055. Downloadable from the library at <http://www.lenr-canr.org

3) T. Mizuno, T. Ohmori and T. Akimoto. "*Generation of Heat and Products During Plasma Electrolysis,*" in *Tenth International Conference on Cold Fusion*. 2003. Cambridge, MA. Downloadable from the library at <http://www.lenr-canr.org>

4) T. Mizuno, D. Chang, F. Sesftel and Y. Aoki "*Generation of Heat and Products During Plasma Electrolysis,"*. in *Eleventh International Conference on Condensed Matter Nuclear Science*. 2004. Marseilles, France. Downloadable from the library at <http://www.lenr-canr.org>

5) Jean-Louis. Naudin et al. Several illustrations and references are downloadable from <http://jlnlabs.imars.com/cfr/index.htm> and from <http://jlnlabs.imars.com/cfr/html/cfrtpwr.htm>

6) Scott R. Little, H. E. Puthoff and Marissa E. Little, *"Search for excess heat from Pt electrolyte discharge in K2CO3-H2O and K2CO3-D2O electrolysis."* Downloadable from <http://www.earthtech.org/experiments/Inc-W/Mizuno.html>. For replications of Naudin's experiments see <http://www.earthtech.org/experiments/IncW/2003/replicationjln.htm>. For replication of Mizuno's experiments (Introduction to third series of runs), see <http://www.earthtech.org/experiments/Inc-W/300volt/run1.html>

7) D. Cirillo, A. Dattilo, V. Iorio, "Transmutation of metal to low energy in confined plasma in the water (electrochemical plasma cell)," *,"*. in *Eleventh International Conference on Condensed Matter Nuclear Science*. 2004. Marcella, France. Downloadable from the library at <http://www.lenr-canr.org>

8) Jean-Francois Fauvarque, Pierre Paul Clauzon and Gerard Jean Michelle Lalleve. *"Abnormal excess heat observed during Mizuno-type experiments;"* 2005. Downloadable from the library at <http://www.lenr-canr.org>

9) Ludwik Kowalski <http://blake.montclair.edu/~kowalskil/cf/252clauzon.html

10) V. Iorio (see reference 7), private communication.

11) X, a chemistry professor at Montclair State University who prefers to remain anonymous. That professor thinks

that people studying cold fusion are wasting time and money. But his was, like other MSU colleagues, a valuable resource on several occasions.

Acknowledgments:
We are grateful to Dr. Hal Puthoff, the director of the EarthTech International, Inc., for supporting this study. Correspondence with Pierre Clauzon is also highly appreciated. Replication of the setup described in (8) would be much more difficult without his help.

P.S.
1) Clarke-Hess VAW meter (model 2330) is designed to measure power of arbitrary waveform from d.c to 400 kHz. Power is calculated from sampling volts and amperes about 2000 times per second (16 bit ADCs). Sampling is asynchroneous (to stay away from the fundamental and harmonics of frequencies present in the waveform. <http://www.clarke-hess.com/2330.html>

2) The data acquisition program controls the run length and measures cell current, voltage, power, mass, temperature, and room temperature. The program also records the date and time, calculates total cell input energy, and plots all variables to a screen display. Measurements are written to a disk file unique to each run, which permits later playback and analysis. {And also permits transfer of the data to a separate spreadsheet program.}

3) Startorius model LC620S digital scale connected to the data acquisition system. Resolution 0.01 grams.

4) Our Pt coated mesh was Nb not Ta and not Ti. It was ordered from Rosenthal Jewelry (don't trust what they have at <http://www.titancomponents.com/products.htm>, call them at 1-800-327-5784).

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268) A message from a reader

Ludwik Kowalski (11/9/05)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Several weeks ago I asked a reader of this website for a permission to quote a message received from him. That permission was given but I forgot to append it to the unit on which I was working at the time. But I did post his message on a private list of cold fusion researchers. In doing this I suggested that somebody might be interested to discuss the topic with the author, Dean Sinclair, PhD in chemistry.

To keep my promise I am creating this unit. It is the trace of my correspondence with him. On Oct. 20, 2005, at 4:55 PM, Dean Sinclair wrote:

> Dear Dr. Kowalski-
> The more I examine the information about cold fusion, the more
> it appears to me that the most probable explanation of the phenomenon
> is the Palladium catalyzed conversion of D:D to He4 along with an
> overlooked concurrent Pd catalyzed conversion of H:H to Deuterium
> atoms. It is very possible that the electrical field is not necessary
> and the electrolytic cell is serving only to furnish D:D for
> conversion.
> It is possible that Hydrogen gas forced through a thin sheet of
> Palladium would show Deuterium enrichment on the other side and a
> temperature rise of the Palladium barrier. Similarly, if Hydrogen gas
> heavily enriched with Deuterium were forced through a Palladium
> barrier, it would not be surprising to see heat, Tritium, He3 and He4
> produced. In so far as I can tell, no one has considered the
> molecule to "atomic isomer" transformations as a possible explanation
> of the phenomenon.
> If one looks at the published data re size and mass of the
> electron and the proton one finds an interesting situation. We usually
> think of the proton as being large and massive and the electron small
> and light. However, if the electron were scaled up to a 1 cm. water
> balloon, the proton would be more like a 100 meter soap bubble!
> You're undoubtedly a better mathematician than I , so you probably
> can work out a better comparison than that. The point is that the
> mass and charge density of the electron is many times that of the
> proton, indeed, the disparity is so great that there seems no reason
> at all that electrons cannot pass freely through protons. Similarly,
> two protons would have to approach to about the diameter of an
> electron to experience the same repulsive forces as two electrons
> colliding. In other words, there is a fair chance that a
> Hydrogen Molecule spends at least some time in the same configuration
> as a Deuterium atom, but does not stabilize to that form because of
> the vibrational energy content...,.(This could be considered as

> essentially the same reason that absolutely pure gaseous water will
> not condense to liquid droplets.....) The same kind of analysis
> applies to the D:D, He4 system and the H:D to Tritium possible
> conversion... Best Wishes, Dean L. Sinclair, BA, MS, PhD

My replying to the above was: I asked:

”1) Can I have permission to append your last message to the next unit on my website?
2) Would be OK to add this sentence:

‘From previous correspondence with Dean, whose life circumstances are not favorable to conduct research, I am nearly certain that he would be happy to receive comments. Here is his email address: <deanlsinclair@gmail.com>’ ”

Dr. Siclair wrote “Sure, go ahead. Thanks.”

On Nov 4, 2005, at 4:25 PM, dean sinclair wrote:

Hi, Ludwik,
I was just looking at a couple of the papers on your site. It seems that no one has pointed out that, in the "relativity" of nuclear particles the proton actually has a very small charge density in comparison to the electron, and an much smaller mass density. There seems no reason that electron orbitals could not pass right through the protons. Also, there seems no reason that protons cannot interpenetrate far more than is usually thought. The "Coulumbic barrier" to intereaction may be virtually nonexistent. Ciao. Dean L. Sinclair

My reply was simple: “Thanks for the comment. Interesting. But discussing such things is far above my level of confidence.” That was my last message to Dean. And his last, very informal, message was: “Oh, well, discussing such things is probably well beyond my level of COMPETENCE, but I'm stupid enough or crazy enough.... ”

Perhaps a theoretically inclined person will contact Dr. Sinclair and discusses the topics with him. Once again I apologize for not posting his first message earlier.

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269) About some analytical instruments

Ludwik Kowalski (11/21/05)
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

1) In this unit I want to to collect information about "new and fancy" analytical methods used in CMNS field, particularly in the CT (cold transmutation) sub-field. Early in November Frank Gordon sent me an Excel table listing these methods. The document is too wide to fit here and I will turn it into a different format below. But first let me quote what Jean-Paul Biberian wrote about two of these methods, ICP-MS (inductively coupled plasma - mass spectrometer) and SEM (scanning electron microscope).

2) On November 16, 2005, at 4:58 AM, **Jean-Paul Biberian wrote:**

a) ICP-MS uses a mass spectrometer to analyze the components of the plasma. This is a very sensitive technique that can easily detect parts per billions. Also one interesting aspects is that it can do isotopic analysis. Unfortunately, there is a problem with the argon which might interfere with other masses for example calcium.

ICP emission on the other hand analyses the light produced during the discharge. This is also a very powerful tool for chemical analysis. However the sensitivity is lower than ICP-MS, but almost all elements can be measured. There are equipments that do both. In order to analyze a material, it needs to be in solution. Therefore for metals, they are dissolved in acid and then fed in the plasma.

b) SEM stands for Scanning Electron Microscope. This is an electron microscope that produces physical images of the surface by measuring the intensity of the secondary electrons produced by the scanning electron beam. The secondary emission varies with material composition and angle of incidence. The lateral resolution can be very high, almost atomic. In order to obtain chemical information, an X-ray detector measures the X-rays produced by the incident electron beam. Therefore a local chemical analysis can also be performed. However, because the incident beam has a relatively high energy, typically 30 keV, a depth of about one micrometer is analyzed. Almost all elements can be detected that way.

3) And here is **information from F. Gordon's table** mentioned at the beginning:

IC -IOS (Inductively induced plasma - optical emission spectrometer)
liquids,
benefits --> used as screening tool
detriment --> liquids only
high maturity
low cost
informs about bulk composition
useful at ~100 ppb level

NNA (nuclear activation analysis)
liquid or foil
benefits --> definitive for analyzable isotopes nondestructively

detriment --> some molecular interference
maturity high
cost low
informs about bulk composition
useful at ~10 ppm level

ICP - MS ((inductively coupled plasma - mass spectrometer)
liquids,
benefits --> used to identify isotopes
detriment --> liquids only
high maturity
low cost
informs about bulk composition
useful at ~200 ppt level

GD - MS (glow discharge mass spectrometer)
foils
benefit --> used to identify isotopes
detriment --> some molecular interference
maturity high
cost low
informs about bulk composition
useful at ~ppb level

XRF (X-ray fluorescence)
foil or powder
benefit --> non-destructive gross screening tool
detriment --> sensitive only in first ~10 micrometrs (of depth)
maturity high
cost low
bulk composition
useful at~10 ppm level

TEAMS (trace elements accelerator mass spectrometry)
dry powder or foil
benefits --> definitive differentiation of molecular Interference
detriment --> surface only
maturity median
hight
surface composition
useful at ~40 ppt level

RBS (Rutherford backward spectrometry)
foil
benefits --> surface screening tool
detriment --> insensitive if impurity > Pd present
maturity high
low
surface composition
useful at ~10 ppm level

XPS (X-ray photoelectron spectrometry)
foil
benefits --> non-destructive surface screening tool-chemical information-depth profile

detriment --> marginal sensitivity
maturity high
medium
surface composition
useful at ~1000 ppm level

AES (Auger electron spectrometry)
foil
benefits --> non-destructive surface screening tool-chemical information-depth profile
detriment --> marginal sensitivity
maturity high
medium
surface composition
useful at ~1000 ppm level

SEM (scanning electron microscopy)
foil
benefits --> compare surface topography
detriment --> marginal sensibility
maturity high
medium
surface structure

SEM/EDAX (scanning electron microscope/energy dispersive analysis of X-rays)
foil
benefits --> gross screening tool
detriment --> sensitive in first 10 microns
maturity high
medium
bulk composition
useful at ~10 ppm level

TEM (transmission electron microscopy)
standard TEM preparation
benefits --> phase definition
detriment --> small field of view
maturity high
cost medium
microstructure

SIMS (secondary ions mass spectroscopy)
foil
benefits --> depth profile
detriment --> molecular interference
maturity medium
cost medium
bulk composition
useful at ~100 ppb to 10 pm level

NRA (nuclear reactions analysis)
foil
benefits --> definitive for one isotope at a time
detriment --> measures 1 isotope at a time
maturity low

cost medium
surface composition
useful at ~100 ppm level

MALDI (matrix assisted laser dispersion and ionization)
foil
benefits --> detects high molecular weight species
detriment --> wrong mass sensitivity
maturity high
cost low
surface composition
useful for organics

PIXE (proton induced X-rays analysis)
foil
benefits --> screening tool
detriment --> lacks sensitivity
maturity high
cost medium
surface composition
useful at 100 ppm level

4) Appended after the ICCF12

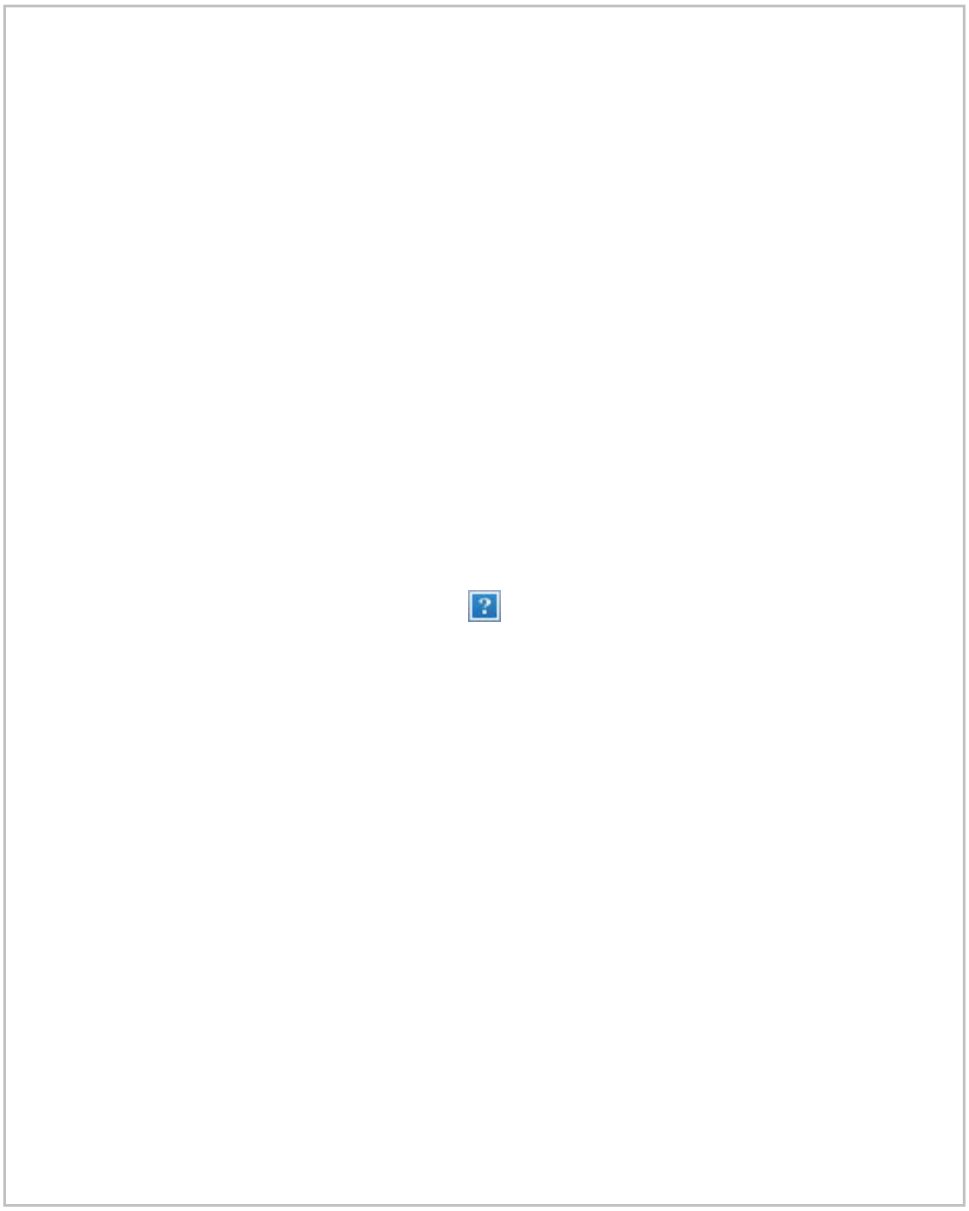
Yokohama, near Tokyo, is the place where the ICCF12 (International Cold Fusion Conference #12) will begin tomorrow Actually it started this afternoon, in the form of a set of “tutorial” sessions. Four leading CMNS researchers reviewed areas in which they have been working. One of them was Yasuhiro Iwamura, a researcher from Mitsubishi Heavy Industries in Japan. First he described his findings in the area of cold transmutations, then he focused on analytical instruments used in that kind of work. He said that such instruments can be subdivided into two broad categories: those for identifying elements (by the energy levels of atoms) and those for determining atomic masses. The figure below is from a slide Iwamura used in his presentation.

Note that column 3 names the “driver;” the kind of radiation, or particles, used to bombard the analyzed atoms. The column 4, in turn, lists carriers of information; such as X rays whose energy spectra must be analyzed, etc. Regions accessible to individual instruments are listed in the last column. Iwamura is certain that he identified several unexplained nuclear transmutations, such as Cs turning into Pr.

In general, Iwamura said, a transmutation research project begins with a qualitative examination. The goal is to find out if the elements in which one interested are present or not. If they are present then one must conduct a qualitative analysis (to find out if it is an impurity or a product of a nuclear reaction). Once the impurity is ruled out one must conduct additional qualitative analysis. The distribution of products (on the surface, in depth, and as a function of time) must be studied to infer what happens. Most scientists are extremely skeptical about nuclear transmutations without fast projectiles, such as neutrons or protons. That is why convincing them that nanograms of new material, such as Pr or Mo, generated in specially prepared foils, is very difficult. Fortunately, modern instruments are very sensitive and, at least in principle, Iwamura's findings should be reproducible in other laboratories. The pictures below were given to me by Iwamura; they were used in his tutorial lecture.

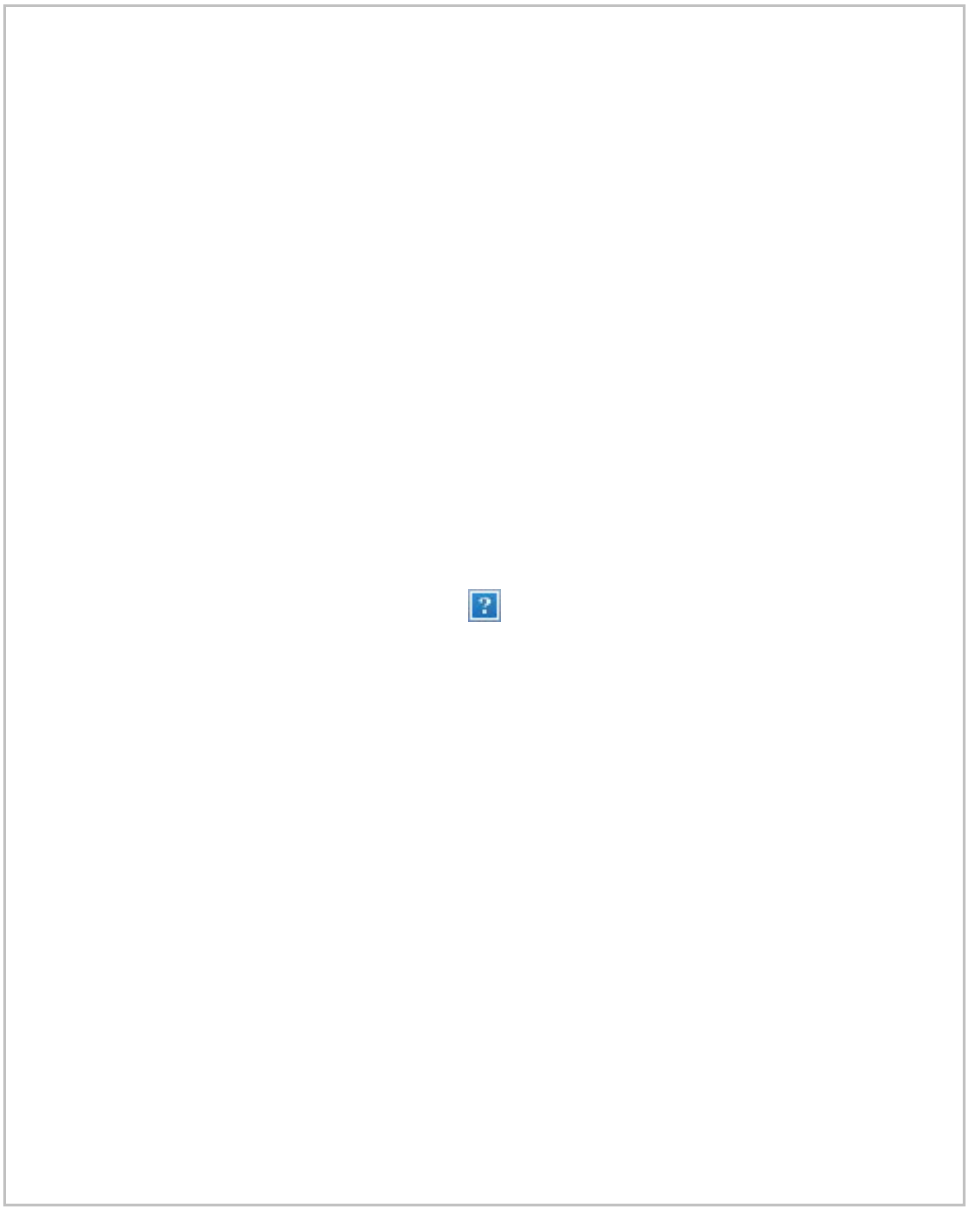
****

SLIDE 8



Principles of XPS

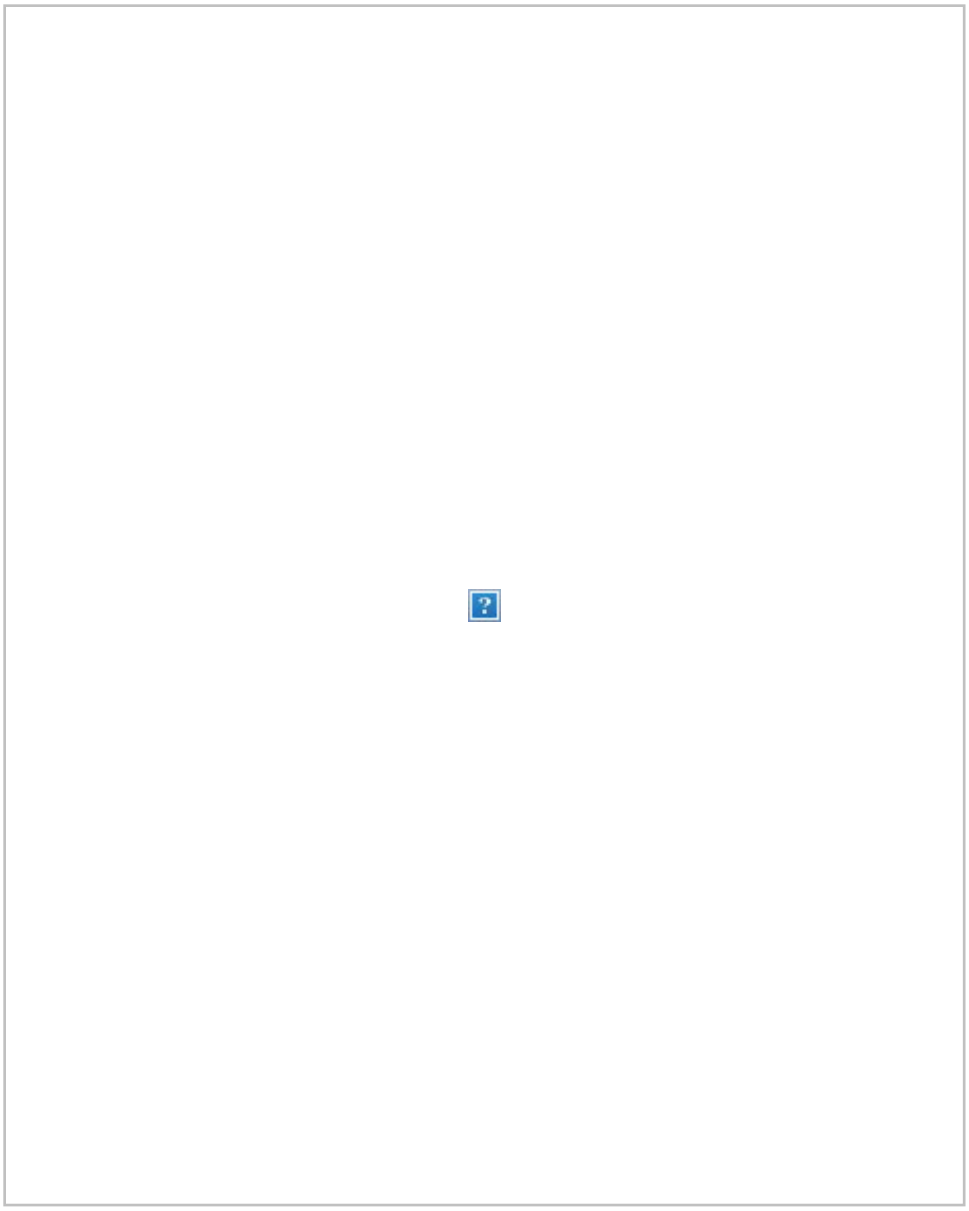
****
SLIDE 9



Principles of XFR

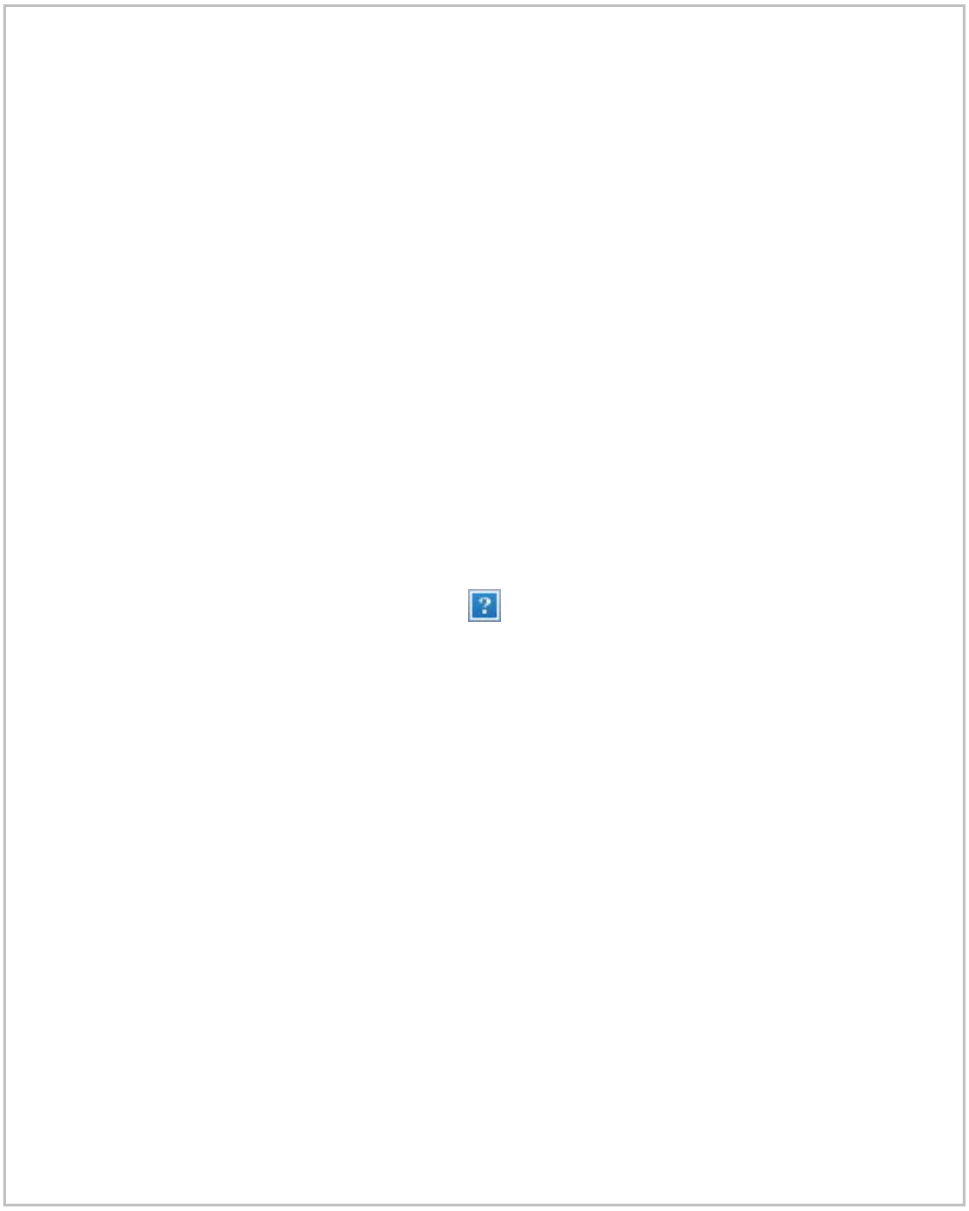
****

SLIDE 10



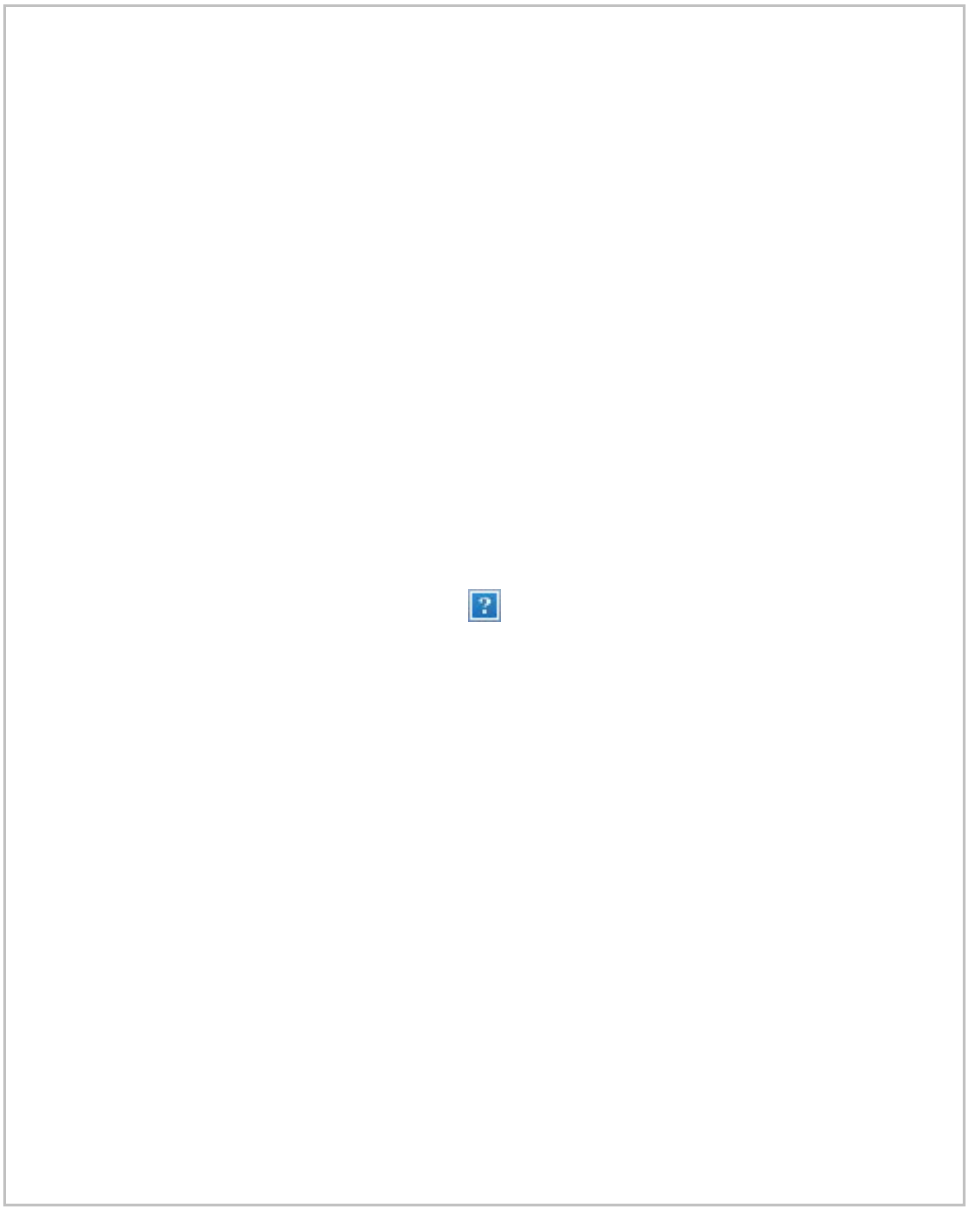
Principles of EDX

****
SLIDE 11



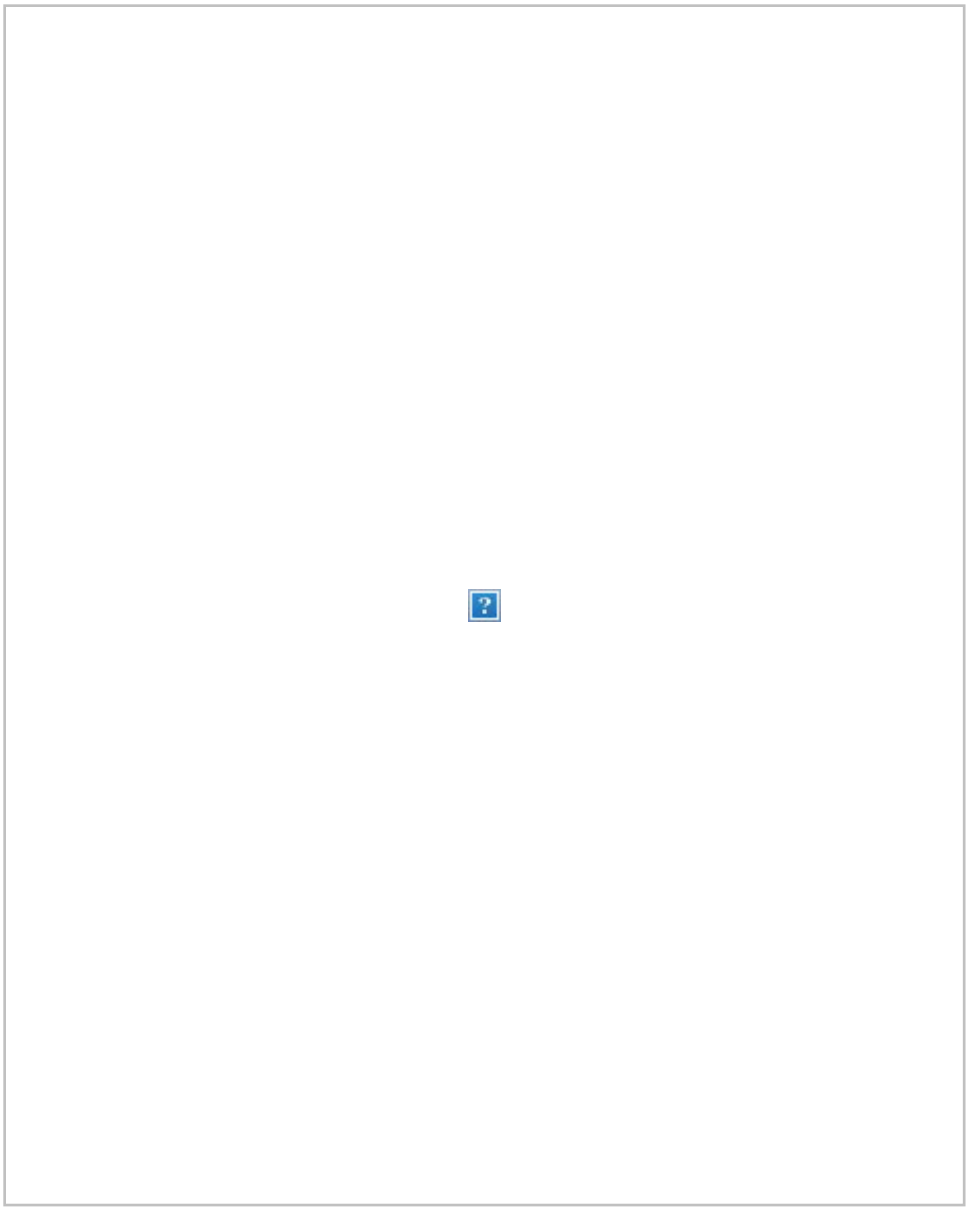
An example of EDX spectrum

****
SLIDE 12



Another example of EDX spectrum

****
SLIDE 13



Principles of ICP-MS

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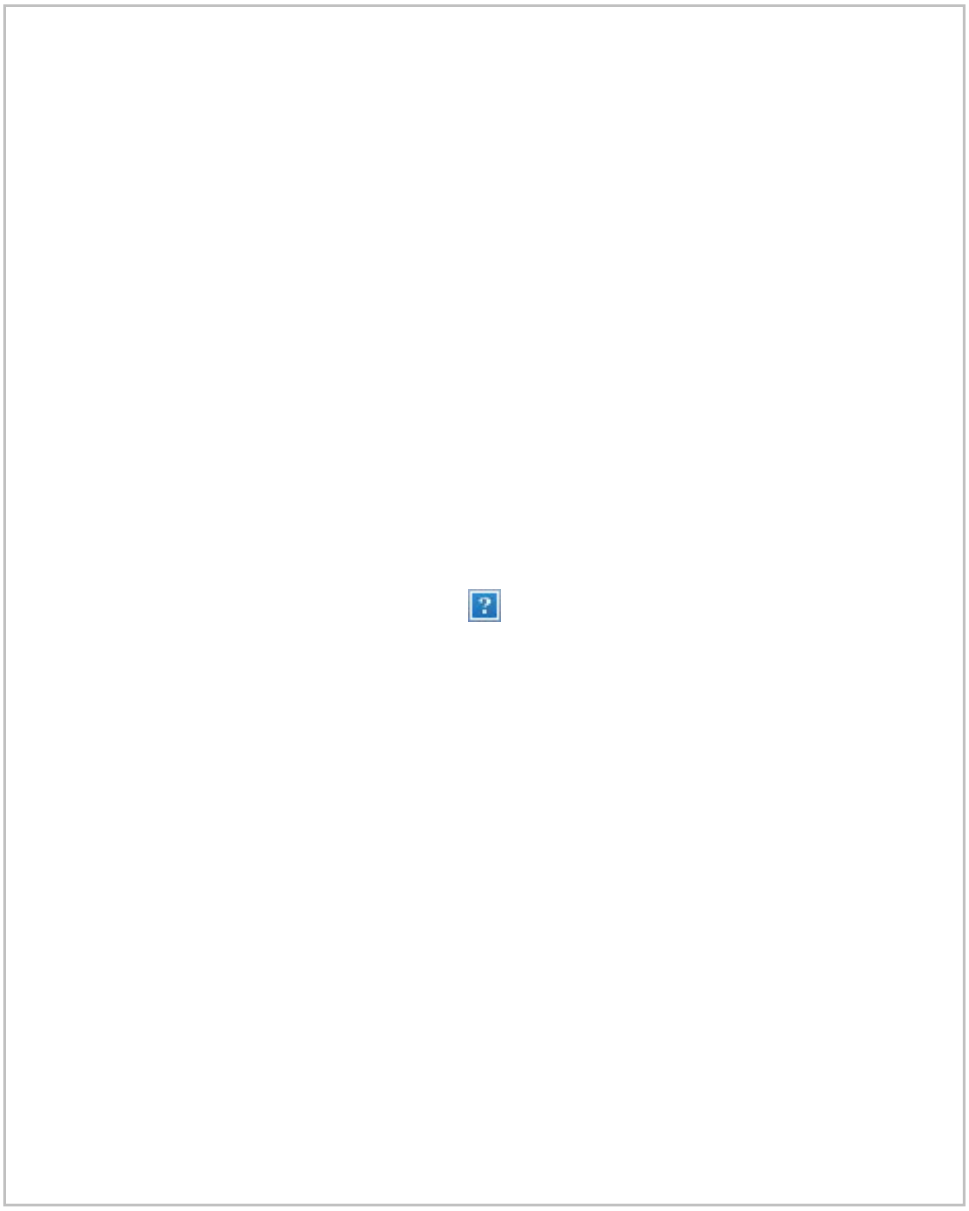
SLIDE 14
Principles of SIMS



High Sensitivity. Local analysis is possible.

Sensitivity differs greatly depending on the elements of the sample and primary ions.
Effects of molecular ions should be considered.
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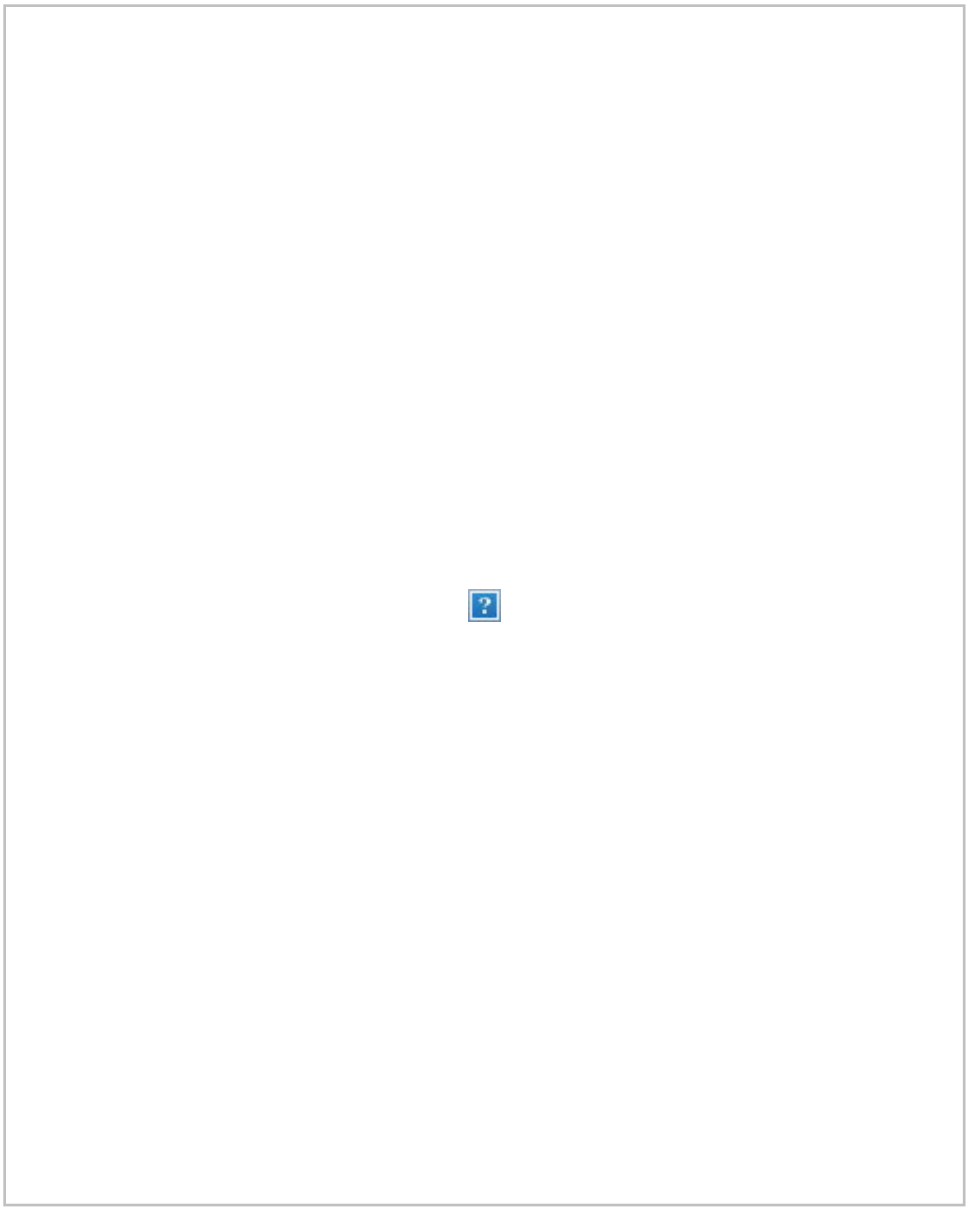
SLIDE 15
Principles of TOF-SIMS



P.S. Iwamura, after seeing the above, asked me to mention that slides were not his own; he found them on the Internet and added some words.

P.P.S. Iwamura has been studying CMNS transmutations for many years. His work dominated all three International Cold Fusion Conferences that I attended (ICCF10 ICCF11 and ICCF12). **His papers can be downloaded from the library at <http://www.lenr-canr.org>.**

5) Contribution from Mark Tsirlin (12/15/05)

Let us observe some experimental proofs of low-energy nuclear reactions (LENR) presented repeatedly in reports at conferences and in papers published in journals of CF community. The main analytical methods applied in this case are various modifications of SEM-EDS and SIMS methods. Let us remind their potentialities.

1. SEM-EDS

This method provides a surface image at the magnification of tens thousand and sometimes, under especially favorable

conditions, – up to 200-300 thousand. Electron beam ~1 micron in diameter excites X-ray irradiation generated by a small volume of substance approximately equal to the volume of a sphere 1.5 micron in diameter. The generated X-ray irradiation is recorded using a silicon spectrometer. Thus, we obtain information on the qualitative and quantitative composition of in the small volume of the substance. Detection limits of the method are 0.1-1%. Apparently, the sensitivity of the method is not very high and approximately corresponds to that of the X-ray diffraction method.

However, due to high localization, the method provides rather efficient detection and analysis of heterogeneous materials, especially those containing microscopic inclusions or multilayered structures. It is worth mentioning that the sensitivity and accuracy of the method are essentially reduced at the analysis of rough surfaces. In my opinion authors reporting detection of some "new" elements e.g. in Pd after D absorption or after the passage of D flow (by using this method) must experimentally demonstrate that:

a) the mentioned "new" elements were absent in the initial metal. This requirement does not seem strange taking into account the fact that Pd in its initial state contains a large number of microscopic inclusions entering the latter at various stages of metallurgical processing. In particular, our experience shows that such inclusions of various nature and origin contain many elements detectable (sic!) by EDS method. Among these elements, we can mention carbon, alkali and alkaline earth metals, silicon, oxygen, aluminum (alumo-silicates of various compositions), chlorine, fluorine, sulfur, tungsten, titanium, etc.

b) details and components of the apparatus could not be a source of "newly formed" elements. This requirement is especially topical for high-temperature processes (glowing discharge, Mizuno-type experiments, etc.). Besides, the map of newly detected elements distribution on the metal surface should be carefully examined. Finally, last but not least, it is highly desirable to coordinate the nature and amount of newly formed elements with the respective nuclear transformations and their gross power output.

SIMS

The method is extremely sensitive (with the accuracy of ppm). That imposes additional responsibility on a researcher. He has to analyze carefully all possible sources (even minor ones) of surface contamination with foreign elements, since it is practically impossible to avoid such contamination working with Pd. Moreover, the specific character of the mass-spectrometric method entails the danger of mixed estimations. Thus, molecules of different nature and mass (including those of organic origin) adsorbed on the metal surface can turn into vapor phase after partial destruction or, on the contrary, polymerization, in the course of ion bombardment in the instrument chamber. Unfortunately, the mass-to-charge ratio of such fragments can coincide with that of an elementary isotope of one or another element.

Usually, modern mass-spectrometers are equipped with respective software and databases facilitating the process of identification of such mixed estimations. Nevertheless, this process is rather labor-consuming and requires special attention. Quantitative analysis within the SIMS framework is rather complicated. This is due to the nature of the method, namely, to the mutual influence of all components of the substance on the ionization cross-section of elementary or complex ions. At the same time, this method offers a possibility to determine isotopic ratios of of various elements.

This remarkable feature is widely used for proving reality of nuclear reactions in cold fusion experiments. The above mentioned hindering factors, however, should not be ignored. One should be aware that different elements often have identical atomic masses and that molecules can be misidentified as elements. Unfortunately, these aspects of the SIMS methodology are not always discussed by those reporting discoveries of abnormal isotopic ratios. Discussing sources of potential errors, and convincing readers that such errors were not made, would strengthen the claims, especially in the eyes of experts from general scientific community.

P.S. (1/13/06)
Graham Hubler, a material scientist at Navy Research Laboratory (Washington D.C.), told me that a small accelerator, in his laboratory, has been modified to become an AMS instrument. They call it Trace Elements Accelerator Mass Spectrometer (TEAMS). I plan to write more about it.

This website contains other cold fusion items.
Click to see the list of links

New results and an ongoing excess heat controversy

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Introduction:

This report describes results of two separate experiments, one conducted in the EarthTech laboratory (Austin, Texas) and another in the laboratory of Richard Slaughter (Boulder, Colorado). The purpose was to either confirm or refute the reality of excess heat in a plasma electrolysis cell. Experimental setups were similar to that of Fauvarque et al. (1). They consisted of electrolytic cells containing the electrolyte (K_2CO_3, 0.2 M) and two electrodes: a cylindrical anode (platinized niobium) and a tungsten cathode (welding rod). A constant difference of potential, for example 400 volts, was applied to electrodes and a glowing layer of gas was formed around the cathode. Three parameters were measured during each test: the electric energy delivered to the cell, E, the amount of heat lost through water evaporation, Q_1, and the amount of heat lost through conduction and radiation Q_2. The difference between Q_1+Q_2 and E, if any, is called excess heat. As in (1), our experimental results are reported in terms of the coefficients of Performance, COP. That coefficient is simply the ratio $(Q_1+Q_2)/E$.

Experimental results:

According to (1), the COP, at 350 volts, is approximately 1.35. Our results, shown below, conflict with that conclusion.

Table 1 Coefficients of performance at several voltages:

VOLTS -->	250	250	300	300	300	355	350	350	400	650
COP -->	1.00	0.99	0.97	1.00	1.00	1.02	1.03	0.98	1.05	1.02

The first nine results are based on experiments performed in Texas (2, 3), the 650 volt result is based on experiments performed in Colorado (4). The Colorado setup was similar, in principle, to that used in Texas except for the power supply (see Appendix 1). An important observation concerning both experiments can be seen in Appendix 2. The Colorado experiment was simply a two-liter beaker, mounted on a balance, with two electrodes as shown in Figures 1 and 2.

Another difference between our two experiments was that the diameter of the anode in Texas was 6 cm while in Colorado it was 3 cm. The range of potential differences in Colorado was from 500 to 700 volts. The mean value of COP in that region turned out to be 0.90 +/-0.2. The last value 1.02, shown in Table 1, resulted from eight separate tests. The standard deviation calculated from these high voltage tests turned out to be 0.11. It is interesting that foaming and splashing in Colorado was so insignificant that an experiment could be conducted in an open beaker. We believe that this was due to the differences between the power supplies used in two experiments.

Figure 1
The cathode (shorter tungsten rod) and the anode (welded to the longer Ti rod), suspended by a nylon thread, are being lowered into the beaker. Rigidity was assured by two plastic disks (sterilite remains rigid at 100 C). The second function of the lower disk was to act as an antisplashing baffle, inside the electrolyte.

Figure 2
The cathode and the anode inside the two-liter beaker during a 700 volt test.

Discussion:

Favarque et al report observing COP's up to 1.41 in their experiments. Similar values were observed by other researchers. Clearly we have not replicated such results. How can this be explained? By experimental errors (measuring E, Q_1 and Q_2), or by unrecognized, but significant differences in experimental setups. Conditions under which experiments were performed might also have been different. Our anodes, for example, were made from platinized niobium while the anode used in (1) was made from platinized titanium. Instruments used to measure E were also very different. Accurate measurement of the electrical input power during plasma operation is complicated by the highly erratic current.

In Texas, for example, the average current drawn by the cell was 2-3 amps but the waveform peaks frequently exceeded 10 amps. A sophisticated Clarke-Hess instrument was able to recognize overloads and signal them by flashing. On a less sophisticated instrument such conditions might be overlooked and will lead to underestimation of the average power being delivered to the cell. (Overloads were observed during the initial testing; they were eliminated by placing a large filter capacitor between the Clarke-Hess instrument and the cell, as described in (8). Possible errors in measuring Q1 and Q2 are discussed in (3).

Let us mention that appearances of the cathode glow discharge in Texas and in Colorado were quite different. In the first case we occasionally observed white light flashes coming from large regions inside the cell, in the second only a steady yellow light was seen from a small region surrounding the cathode. Intense white light coming from large regions inside the electrolyte can also be seen on the Internet pictures of similar experiments performed by other researchers. Another difference worth mentioning was the fact that a typical current (for a given voltage) in Texas was about four times higher than in Colorado. The plasma differences between the Texas and Colorado experiments were probably due to the arc suppression abilities of the Colorado power supply (see Appendix 1).

Why is excess heat observed in some recent experimental setups (1, 5, 6, 7) and not in others (8)? Are the discrepancies due to experimental errors or to unrecognized, but essential ingredients present in some setups and absent in others? It is difficult to answer such questions. Scientific disputes are not resolved by voting but a large number of confirmations of excess heat, by qualified researchers in many countries, should not be ignored. The existing controversy would be

eliminated if a highly reliable portable excess heat generator could be built. Such generator would then be studied in different laboratories. How else can a consensus be reached?

Referring to the issue Jed Rothwell (9) wrote: "This is more art than science. Direct, hands-on learning may be the only way to grasp these things; Mizuno himself does not know what is going on in many ways. Building devices on the basis of described protocols does not seem to be sufficient." That is a very good point. A tentatively accepted theoretical model would certainly synchronize our dispersive efforts to make sense out of what is going on in the CMNS field. Trying to confirm or to refute claims made by individual researchers is certainly much less effective than testing various aspects of a tentatively accepted model.

P.S. (12/2/05, by Ludwik Kowalski):
Unfortunately, one important detail, about the protocol used by Fauvarque et al., was not specified in (8). As I learned from Pierre Clauzon (at the ICCF12) -- the voltage applied to the cell must change gradually, typically in 20 V increments every minute or so. During that time the cathode was said to be preconditioned to produce excess heat. Clauzon said that they also failed to confirm reality of excess heat without preconditioning.

P.S. (1/3/06, by Ludwik Kowalski):
In a private message Richard wrote to me that his 30 new attempts to generate excess heat were not successful. He is in contact with Pierre Clauzon. I hope that the contoversy will eventually be resolved. How can it faile to be resolved when two honest and edicated researchers are willing to travel and work together, if necessary? I plan to write about this later.

Appendix 1; Pinnacle, a power supply that prevents arcing:

The power supply used in Colorado was a Pinnacle 20kW (10). Although the supply was designed for other purposes it seems to be ideal for GDE (Glow Discharge Electrolysis) experiments. Its typical applications include dc sputtering with RF bias, basic magnetron sputtering, cathode-arc deposition (sputter etching and dc-biased RF sputtering). The special features of the Pinnacle are:

1) High line to load efficiency 91%
2) Power factor > 0.9 for loads greater than 1.2kW
3) Quick response to changes in the load
4) Extremely low stored energy in the output filter
5) Fast arc-suppression
8) Ability to preset joules to be delivered to the load
9) Provides up to 20kw of continuous power (we did not exceed 2 kW).

The supply provides very accurate power measurements and the ability to control how the power is delivered to the cell. The Pinnacle can be used as a source of constant voltage, constant current or constant power, depending on what is chosen. The other two parameters are then determined by the load. The output voltage, current or power will hold to within +/- 1% as long as the load impedance stays within the voltage and current limits of the unit. Our tests were performed by using the constant voltage mode, in the voltage range between 400 and 800.

The low stored energy of the output filters of the Pinnacle allows the regulation system of the supply to rapidly respond to changes in the cell impedance. The low stored energy also prevents the supply from dumping large amounts of energy into the cell as its impedance changes. The low stored energy is probably one reason that foam, white light and violent reactions, observed in Texas (see Figure 3) were not seen in Colorado. The other reason was probably the built-in arc suppression.

Figure 3
Foam and white light observed at 350 volts in Texas were not observed at 700 volts in Colorado (see Figure 2).

The pinnacle has several methods of arc suppression. The one we used is called "micro arc suppression." When an arc of less than 10 microseconds occurs, the stored energy is diverted from the cell and the growing arc is extinguished. The voltage is restored, approximately 5 microseconds later. This ability is used in industry to maintain steady even plasma that are required for precise deposition of material on the manufactured product.

This arc suppression in combination with the low stored output energy of the Pinnacle was most likely the reason that we were able to increase the potential difference to over 700 volts without the violent reactions in the electrolyte. The arc suppression might also be a reason that our results are different from those reported in (1). According to (9), Mizuno says "anyone can generate plasma with hundreds of volts. The trick is to generate it and then gradually reduce input power to the minimum." In one of his papers he writes: "Some researchers have attempted to replicate the phenomenon, but it has been difficult for them to generate large excess heat. They have tended to increase voltage to a very high value, around several hundred volts, but they measured no excess heat." Mizuno also indicates that when the cell begins to produce excess heat the conditions inside the cell are placid. These are the same conditions that we observed in Colorado.

We recognize that the cost of the Pinnacle would make it prohibitive for most experimenters. There are, hpwever, less expensive techniques for arc suppression that could be added to power supplies. It is likely that arc suppression will help to minimize the power delivered to the cell, at a given voltage. This might be an important precondition for the generation of excess heat, as indicated by Mizuno.

Appendix 2; On condensation of steam inside cells.

A critical reader might notice that Fauvarque et al.(1) used an open beaker while the cell used in Texas had a lid on top of it. Can this possibly be a reason for the discrepancy between the outcomes of the experiments? The fraction of steam condensing in a cell with a lid is certainly larger than in a nearly-open cell. The rate at which the mass is lost is used in the calculation of COP. Therefore, a critical thinker might say, the difference in the reported values of COP is not surprising. This issue was discussed in (3). The conclusion was that no significant errors are expected from condensation of water inside the electrolyte, provided the non-evaporative losses are also measured, as in (1). But that was only a logical conclusion based on certain assumptions. Here is how the conclusion was experimentally validated in Colorado.

a) One liter of water was placed into a two-liter beaker standing on a scale, as in (1). An ohmic heater was used to

sustain boiling. The cell had no lid. The mass lost in 350 seconds was found to be 40.1 grams.

b) The cell was then covered with a plastic plate. That plate had 8 holes (about 1 cm diameter each) plus a hole for the ohmic heater cable. The amount of water lost in 350 seconds turned out to be 26.1 grams. That confirmed the obvious - condensation is enhanced by covering a cell.

c) A pair of plasma experiments was performed -- one with a nearly open cell and another with a nearly covered cell. The calculated COP turned out to be 1.00 when the cell was closed (to prevent anticipated splashing that did not materialize) and 1.07 when the cell was essentially open. This pair of tests, by the way, was conducted at 700 volts. Pairs of tests at 600 and 550 volts also showed that the values of COP are nearly identical and very close to unity.

Appendix 3; On reporting negative results:

After finishing Colorado experiments, one of us (L.K.) called a friend and talked about the discrepancy between our results and results reported by other investigators of plasma electrolysis. This prompted him to write the following message: “I am a retired physics teacher interested in all three CMNS phenomena: CF (cold fusion), CT (cold transmutations), and EE (excess energy). We know that there are two kinds of experimental results: unreliable and reliable. Unreliable data are obtained when experiments are being set up and when we are not yet sure that things work properly. Once the initial difficulties are overcome researchers think that results are reliable.

Suppose that reliable experimental data were obtained in ten experiments. Two of these experiments confirmed the expectations and eight did not confirm them. How should such situation be handled? What should a scientific conference report contain, only two positive results or all results? I know that our natural tendency is to focus on the most interesting new results. But is this appropriate? I do not think so. Negative results should not be confused with unreliable results..... ”

But the situation is complicated by the fact that some researchers might be hesitant to publish negative results. They might fear that negative statements, taken out of context, can be quoted by pathological skeptics criticizing the field. In our opinion such fears are not justified; benefits from sharing all reliable results far outweigh negative consequences of self-imposed secrecy. Unscrupulous critics, such as Dr. Richard Park, should not be feared by honest researchers. The issue of publishing negative results goes to the heart of scientific methodology. Here is what Ed Storm wrote about publishing negative results (a private message, 11/19/05): “I would like to throw out some counter thoughts. I think publishing negative results is a waste of time unless these results reveal a useful pattern. For most phenomena, millions of ways are available that will fail to generate the desired results. In contrast, many fewer methods will give the desired results. When isolated negative results are reported, they provide no guidance because they are only a few of so many possible ways to fail. The negative results must be shown to relate to a pattern or a general characteristic. For example in CF, if the H_2O content of the electrolyte is too high, the results will be negative over a wide range of concentration. Unless this fact is related to a positive result obtained when the H_2O content is low, the observations have little value. My point is that people reporting negative results should be required to show an understanding of their meaning just as people reporting positive results must show what their results mean. However, this does not mean that negative results can be used to deny the possibility of positive results. The two kinds of results have very little relationship to each other until the positive results are well understood, because as I said at the start of these comments, many ways exist for a real result to fail.”

We do not agree with this. Negative results are worth sharing and discussing. In this report, for example, one of us (R.L.) speculated that our negative results might be associated with the absence of arcing (collecting data when arcing was not too intensive in Texas and suppressing arcing electronically in Clorado). Is it possible that arcing superimposed on the quite glow discharge is an essential component of a device generating excess heat? R.L. plans to answer this question experimentally in the near future. We think that negative results are worth sharing because, like positive results, they guide our attempts to understand what is going on.

At one point two of us (R.L. and L.K.) were discussing the magnificent Naudin's report: The scatter plot called "efficiency . . . " shows results from a large number of experiments that were similar to ours. We noticed that in all 22 experiments (performed at 200 V or above) the excess heat was larger than 20% of the input energy. That is a highly

significant result; the voltage-dependence trend seems to be consistent with what was reported in (1). That is how the issue of "publishing positive results only" came about. Negative results were not mentioned. We assumed that the impressive 22 data points represent 100% of reliable experiments. But what if this were not true? We would be much less impressed if these 22 data points represented only 30% of all reliable experiments. Negative results, if any, are important indications of encountered difficulties.

P.S.
Subsequently Ed wrote: "You may quote me and even use my name. However, your counter argument to my comment missed the point I was making. As I said, "I think publishing negative results is a waste of time unless these results reveal a useful pattern." The "useful pattern" is the important aspect of my comment. In your reply, you showed that your negative results were being used to reveal a useful pattern, i.e. that the nature of arcing was a possible important variable. .....During my present study of the F-P effect, I have had many "negative" results. However, I know that certain variables are important and must be controlled. For example, no excess energy is possible unless the D/Pd ratio in the surface is very high, which is a difficult condition to achieve. Therefore, I'm investigating the processes that affect this composition. If I report all the negative results without relating them to the important variables, I would be wasting everyone's time.

References:

1) Fauvarque, J., P. Clauzon, and G. Lalleve, Abnormal excess heat observed during Mizuno-type experiments. 2005, Laboratoire d'Electrochimie Industrielle, Conservatoire National des Arts et Métiers: Paris.
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2) See <http://www.earthtech.org/experiments/Inc-W/Fauvarque>

3) See <http://csam.montclair.edu/~kowalski/cf/267little.html>

4) See <http://csam.montclair.edu/~kowalski/cf/270slaughter.html>

5) T. Mizuno, D. Chang, F. Sesftel and Y. Aoki "*Generation of Heat and Products During Plasma Electrolysis,"*. in *Eleventh International Conference on Condensed Matter Nuclear Science*. 2004. Marseille, France. Downloadable from the library at <http://www.lenr-canr.org>

6) D. Cirillo, A. Dattilo, V. Iorio, "Transmutation of metal to low energy in confined plasma in the water (electrochemical plasma cell)," *,"*. in *Eleventh International Conference on Condensed Matter Nuclear Science*. 2004. Marseille, France. Downloadable from the library at <http://www.lenr-canr.org>

7) Jean-Louis. Naudin et al. Several illustrations and references are downloadable from <http://jlnlabs.imars.com/cfr/index.htm>. and from <http://jlnlabs.imars.com/cfr/html/cfrtpwr.htm>

8) Scott R. Little, H. E. Puthoff and Marissa E. Little, *"Search for excess heat from Pt electrolyte discharge in K_2CO_3-H_2O and K_2CO_3-D_2O electrolysis."* Downloadable from <http://www.earthtech.org/experiments/Inc-W/Mizuno.html>

9) Jed Rothwell, in an email message, November, 2005.

10) The Advanced Energy Industry, Inc, Fort Collins, Colorado, 80525 80525 <http://www.advanced-energy.com>

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271) Are we now confirming French EE results?

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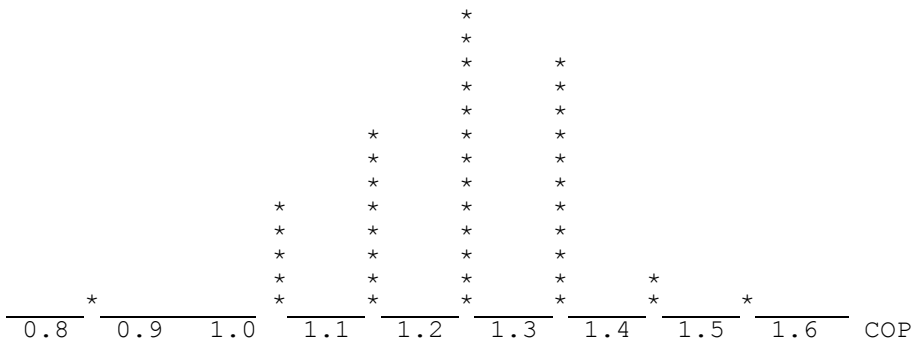
Introduction and tentative conclusions:

The controversy about the excess energy (EE), described in the item #270, remains unresolved. Let me tell you about a recent attempt to resolve it. The idea of repeating the experiment under the protocol used in Paris was discussed with Pierre Clauzon during the 12th International Conference on Cold Fusion (ICCF12) in Japan. He, and other researchers, including Mizuno himself, said that our kind of plasma was very different to what they were working with. About a months ago Pierre invited me to work with him at CNAM in Paris. Come to learn how to produce desired orange-red plasma, he wrote. My suggestion was that he comes to the US. A confirmation in another laboratory, I said, would be more convincing than repeating an experiment in my presence. That is why Pierre came to work in Richard Slaughter's lab in Boulder, Colorado. He brought his own electrodes and his own potassium carbonate. During the first four days we had many difficulties. But on the day #4 the situation changed and we started confirming French results.

We learned how to recognize favorable conditions under which results become very reproducible. It is the end of day #8. Out of 41 experiments, performed at voltages ranging from 300 V to 350 V, only 6 resulted in the COP smaller than 1.1 (generating "excess heat" at the rates less than about 50 W). The mean COP and the standard deviation turned out to be consistent with a bell-shaped curve whose mean COP and standard deviation are 1.24 and 0.13 , respectively. Note that 1.24 is significantely higher than the COP at 300 V reported by the French team at the ICCF12. The histogram below shows 14 cases in which the COPs are consistent with what was reported for 350 V.

Why I am using the quotation marks for the "excess heat? " Because I am aware of possible interpretational errors, both in Paris and in Boulder. It is better to be prudent than sorry. Additional verifications will follow. So far we are happy that similar experiments produced similar results.



It will be useful to invent labels for different projects. The French investigation we originally failed to replicate will be referred to as Paris-1 experiment. Our subsequent investigations, described in the unit #270, will be referred as Texas-1 and Colorado-1 experiments. Our French-American project, now in progress in Boulder, will be referred to as Colorado-2 experiment. I believe that project Paris-2 and Texas-2 will start after the end of the courent experiment. So far reality of excess heat in a setup similar to that in Paris-1 seems to be confirmed. But Colorado-2 results did not confirm that the COP at 350 V is between 1.3 and 1.4, as claimed in the Paris-1 experiments. The mean COP from five tests at 350 V was not significantly different from the main COP at 300 V.

My Internet messages (appended on 3/21/06):

In what follows I will show messages that I posted on the restricted Internet discussion list. The operational definition of the COP can be found in the appendix.

Message 1:

1) We have runs with the COP (coefficient of productivity) above 1.20 at voltages between 300 and 350 volts. But there were many traps on our path. Four days ago I was nearly certain that we are again not seeing excess heat. Here is one illustration. Two days ago we ended the first sequence of consistently large COPs (at least 10 runs). The next morning we worked for about 6 hours and every single COP was nearly one. Frustrated we started to think about all possible differences. Then we realized that the diameter of the W cathode was 2.4 mm yesterday and 3 mm this morning. With the 2.4 mm cathode we again had a sequence of consistently large COPs.

The essential thing is to create conditions under which the electric wattage and the electric voltage are not too different. For example, 350 volts and about 1 or 1.5 A. This happens when the cathode looks as a uniform yellow-orange cylinder and the tip of the cathode is nearly as yellow-bright as the sun. These details were supplied to us by Jean Louis Naudin who seem to be ahead of everybody in this game. According to Pierre, he has a setup that performs "on demand; in his laboratory." He turns the switch on excess heat starts coming, like after turning a bathroom heater. We are still very far from such comfortable situation. Perhaps Naudin will share some details about this device here.

2) Here is our protocol, more or less.

a) Our container is an ordinary two-liters beaker. The level of the K_2CO_3 electrolyte (0.2 M, as we had) is about 1.2 liters. The beaker is open and fresh hot water is added when the level fols to about 1.0 liters, or more often. There is no splashing at all (or very little, less than one gram for 50 grams evaporated. We do have splashing when power is too high at 300 or 350 W. Under such conditions the COPs are always close to 1.0. Under favorable conditions, on the other hands, we hear a steady sizzling roar. The surface of the electrolyte is surpassingly quite. We do not stir the liquid; intensive boiling takes place only between the electrodes, mostly near the very hot cathode. The thermometer, situated near the wall, shows the temperature of between 89 and 91 C.

b) Mizuno told me that the rate at which the voltage is increased progressively should be small. Here is how we arrive to favorable conditions:

aa) Apply 100 V for about 15 minutes.
bb) Apply 150 V for the next ~5 minutes
cc) Apply 200 V for the next ~15 min
dd) Apply 250 V for the next ~15 min
ee) Apply 300 V for about 60 min
ff) Apply 350 V and start measuring the COP (5 min per run)

Transitions from one voltage to another were also slow, typically in one or two minutes. At stage (bb) blue sparks become visible at the tip. At stage (cc) the entire cathode (initially about 20 mm below the ceramic tube) is orange-red. The inner diameter of the ceramic tube is larger than the cathode and we can push the cathode when what is left become ~5 mm or less. Allowing the cathode to become too short leads to large splashy ark-like booms.

We use distilled water but ordinary water is likely to be acceptable. At one point our beaker was broken and we used a plain glass container. The COP was 1.3 at 350 V. At present we are using a plastic container (the lower part of a large bottle in which the windshield wiper fluid is sold. It works very satisfactory.

What else is important? Our non-evaporative losses are between 66 W (in the now broken beaker) and 95 W in a wider glass container. The losses seems to be more or less proportional to the open area of the container. This indicates that what is lost via conductivity through the walls is only a small fraction of the total non-evaporative loss. This is confirmed by the fact that the plastic foam isolation of the wall does not result is a large reduction of the overall non-evaporative losses. The dependence of our non-evaporative losses, Pl, for our wrapped plastic container on the electric wattage, Pe was as follows:

Pe (W)	Pt (W)	
406	53	measured
688	73	measured
848	84	measured
260	30	extrapolated
200	20	extrapolated

Non-evaporative loses are, very approximately, are about 10% of the electric power. A good indication of the potential success is to see a very small change in current (preferably going down) when the voltage is changed from 200 to 250 Volts. There is a lot of what we do not understand but at least we know what conditions should be avoided. We suspect that occasional white "explosions" (producing strong splashes) are due to ignitions of hydrogen bubbles. It is important to make sure that hydrogen does not accumulate below the anti-splashing shields. Fountains of water were often seen escaping from the ceramic tube containing the tungsten cathode. The inner diameter of the ceramic tube should be 2 to 3 mm larger than the tungsten cathode. That allows the cathode to be cooled by the rising electrolyte. A teflon disk above the ceramic tube, mounted on the tungsten rod, stops the rising fountains.

3) The radius of the anode does not seem to be important; we used Pierre's anode (diameter of about 4.4 cm) and Richard's anode (diameter 8 cm) at 300 V and obtained very similar COPs, about 1.2 or so. Pierre's anode is platinized Ti while Richard's anode is platinized Nb.

4)The rate at which cathodes are consumed are low when favorable conditions (low input power) are found. We do not even start measuring the COP when the current is higher than about 1.8 A (at 300 V). We reduce the voltage, wait a little and hope for the best. Sometimes favorable conditions (described above) are found rapidly but most often one has to make several attempts before finding them. Results are very reproducible under favorable conditions only. Our protocol does not guarantee favorable conditions. But we now know how to recognize unfavorable conditions, more or less. The COP is nearly always one when we see a lot of white light light and a lot of splashing.

5) The length of the cathode, sticking out of the ceramic tube, should be about 2 cm, initially. Richard has a nice way of pushing the cathode rod down manually, when necessary.

6) Unfortunately, frustration has not been eliminated; we still cannot say that a Mizuno-type experiment can perform "on demand." Looking for a reliable protocol should be part of future efforts. We are counting on help from other experimentalists..

7) Data collected under favorable conditions are in agreement with what was reported by Fauvarque et al. We constructed a histogram of the COP distribution for the run performed at 300 and 350 V. So far it has 33 data "bricks." It shows 24 results with the COP between 1.2 and 1.4, 3 results with the COP between 1.4 and 1.4, 10 data points with the COP between 1 and 1.2, and one result with the COP of 0.81 I hope this description will be useful to many.

8) Another detail worth mentioning is that ordinary tungsten cathodes perform as well as those that have 2% of Th added. This is good news for those who might wish to detect nuclear particles. And to those who might think about possible health effects from consuming too much thorium. Tungsten we use is from the welding supply store; it costs about $50 to buy a box with 10 rods. The fact that 2.4 mm rods work while the 3 mm rods do not work is an indication that even thinner rods (or wires) might perform better. The local strength of the electric field (V/cm) increases rapidly when the diameter becomes smaller. A thin wire will perhaps produce good COP plasma at lower voltages.

Message 2:
Here is how a "watmeter" was calibrated yesterday in our ongoing Mizuno type experiment. We essentially repeated a procedure developed in CNAM in Paris. The electrolytic cell (producing typical glow discharge current) was connected in series with a resistor of about 100 ohms and with a constant-voltage power supply. The average current was, for example, 2 A, depending on the total voltage. The UNIGORE 390 instrument (that we wanted to calibrate) was measuring the electric energy (in W*h) received by the resistor. That resistor was immersed in a second beaker containing boiling water. The function of the electrolytic cell, connected in series was to produce the randomly fluctuating current. The amount of water evaporated, for example, in 20 minutes, was measured at the same time. Knowing that amount, and knowing the rate of non-evaporative losses (conduction and convection)we were able to

determine thermal energy generated during the same 20 minutes.

The difference between the thermal energy and the electric w*h better turned out to be 0.43%. Knowing the about 1% uncertainty associated with our determination of thermal losses we concluded that the electric energy measure with the instrument can be reliable at the level of about 1.5%. We are lucky that Pierre Clauzon, who developed the method, was with us, in Richard Slaugther's lab. We hope that the ongoing French-American cooperation will soon resolve the controversy that developed last summer. That controversy, by the way, was published in the form of two reports at the ICFF12. It is also described in the unit #270 at my CMNS website:

http://blake.montclair.edu/~kowalskil/cf/

Note that the calibration was performed under the worst possible conditions; both i(t) and v(t) were variable (while the total voltage on the power supply was constant). Is there any reason to doubt that the uncertainty of less than 2% (determined as above) cannot be trusted? Comments will be appreciated. The small portable Unigore 390 multimeter, by the way, costs about $1000, I was told.

P.S.
In reading my own message (shown below) I see something that should be specified. The thermal calibration of an electric energy measuring device is valid provided this device is not used in thermal measurements. That was not exactly true in our case. Let me elaborate on this. The thermal energy generated consists of two components, heat lost via evaporation and heat lost by conduction and convection. Only a scale is used to determine the first component, which is dominant. But the second component was determined by methods at the same time, one with Unigore 390 and another with other instruments. I do not have the numbers to report at this time. But the results were nearly the same. Pierre just reminded me that the very same instrument was calibrated many times in Paris and results were also very satisfactory. Let me add that current experiments are done with the open vessel. More about this will be written later.

Message 3:
1) Can it be that tiny droplets (primage in French) are responsible for the illusion of excess heat? During one experiment we placed two paper towels, of known weight, on the table next to our active container for exactly 2 minutes. Comparing the amount of water lost during that time with the tiny increase of the mass of the paper towel we concluded that no more than 2 +/- 1% of water was lost in the form of droplets that were large enough to hit the towels (smaller droplets evaporate in air).

In principle such tests should be perform during each experiment. But this was practically impossible. We plan to perform another test to address the same issue. What else is worth checking in anticipation of future criticism?

2) Here are five more data points for our histogram; all at 300 C.

COP=1.15, 1.19, 123, 1.19 and 1.21

3) We had only 5 points at 350 V (to stay away from possible troubles. The mean value is not significantly different from what one gets at 300 volts. But we do get the COP of unity at low voltages. This is a very strong argument that instruments are reliable.

4) About our tungsten -- in case somebody might be interested. It was manufactured by Osram Sylvania Inc., Towanda, Pennsylvania, 18848, USA.
The label on the box (purchased today for $43) is: 2 7033 275 53 3/32X7

Message 4:
Prompted by comments about tiny droplets, presumably escaping with the steam, we performed an additional experiment. It was our last test together; Pierre is returning to Paris tomorrow morning, Richard is going back to work tomorrow (after using 1/2 of his yearly three weeks of vacation) and I will start writing a unit about this "Colorado Project" for my CF website. As suggested by somebody on this list (sorry for not remembering the author), we took a very fine stainless-steel screen and covered the cell with it. The holes in our screen represented about 60% of the entire

surface, the rest were wires. Each rectangular hole was about 0.2 by 0.2 mm. This would allow the steam to escape but would probably stop most of the drops (both small and large).

The COPs, measured at 300 W, while the cell was ~90%-covered by the screen, turned out to be: 1.20, 1.26 and 1.46. These were more or less the same values as for the open cell. If the excess heat, measured with the open cell, was apparent (due to droplets escaping from the cell) then the COPs would be lowered significantly when the screen was used. But this did not happen. We concluded that the excess heat was not due to droplets. A similar comparison was made with the ohmic heater. The COPs turned out to be the same (1.0) with and without the screen. In the case of the ohmic heater ~95% of the surface was covered by the screen.

Message 5 (posted by M.J. from France)
"I have another concern: non-evap losses surely depend not only on the temperature but also on the convection speed, couldn't this speed be higher for some reason when calibrating with the ohmic heater than during the run, leading to overestimating non-evap losses occurring during the run, and therefore overestimating excess heat? " [That is a very good question. Paris-2 tests, now in progress, will probably provide the answer. The vessel used now is a thermos. The non-evaporative losses will probably be negligible. Will the COPs still be close to 1.24? We will see.]

Message 6:
1) In an earlier message I said the noise one hears from plasma, under favorable conditions, is "like from a machine gun." A more appropriate description is to say that it is like when something cold is placed on a large frying pan with very hot oil, for example, when frying an egg. That sizzling noise is not as regular as from a machine gun.

2) Reading messages posted today I was thinking about a branch of science called plasma physics. Most of us performing Mizuno type experiments are not event amateurs, as far as that discipline is concerned. Very sophisticated methods of plasma diagnostics have been developed in the last 50 years. Experts in plasma physics would have no trouble in the identification of the red-orange-sizzling-machine-gun-like plasma we were lucky to produce in the Colorado-2 tests. They probably have a short name for it. And they would have no trouble with creating a device generating excess heat on demand. The only person on our list familiar with plasma physics, as far as I know, is George Miley. I would like to know what he thinks about our Mizuno type experiments.

3) Yes, I know what many plasma physicists said about CF in the past. And I recall what Gene Mallove wrote about their selfish motivation. But we should be looking into the future, not into the past. It is silly that we are working in the darkness when powerful sources of light are available. How can we benefit from what is known to real plasma physicists? How can we find access to their sophisticated tools? How can we gain support from at least some of them?

Message 7:
Here is a little rough calculation worth sharing. In our typical 5 minutes test (input electric energy = 400 W * 0.0833 h = 33.3 Wh) the amount of excess heat is about 6.5 w*h (if the COP=1.2). This translates into $1.46*10^{23}$ eV. The estimated mass of tungsten lost during the same time is about 0.5 grams. That translates into $1.63*10^{21}$ atoms. Thus the excess heat per atom of tungsten seems to be close to 100 eV. That is about two orders of magnitude larger than what I associate with common fuels.

What happens to our spent tungsten? Part of it probably melts and then solidifies in the electrolyte. If so then pure tungsten must be found in the cell (in the deposits we see at the bottom, after some waiting). Suppose that 50 % melts and the rest combines with other atoms or ions. Not being a chemist I might be allowed to postulate that all possible chemical reactions are equivalent to one representative exothermic reaction. Suppose that our excess heat is due to that reaction. What would the Q value of the representative reaction be? It would be 178 eV. That is a lot !

The Q value would be two times larger if only 25 % of tungsten was used in chemical reactions while the rest was simply melted. This shows that Q increases when more tungsten is melted. At the other extreme the Q value would be 89 eV if nothing was melted (all consumed in exothermic chemical reactions). Even this number is unusually high. Did I make a wrong assumption, or a numerical mistake, somewhere?

I think it was Pierre (or was it Richard?) who suggested I make this kind of calculation. Fortunately, a possible 100% error (in the estimated loss of mass lost) would still support the conclusion -- the unusually high energy per atom of tungsten. In the future experiments cathodes should be weighted before and after individual tests. This would provide a more accurate result.

The "what if" fantasizing:

Let me assume, very optimistically that Scott Little, and other investigators, will confirm that excess heat is real. What should be done next to convince mainstream scientists that at least one so-called "cold fusion" phenomenon is worth studying? The answer is obvious -- it is to publish a good paper in a serious refereed journal. But editors of these journals are biased against cold fusion. They already know that our research is pesudo-science and do everything possible to block publications. How to overcome the bias of the editors? How to overcome the bias of the referees?

The first thing is to write the article very well; all possible objections should be anticipated and addressed. We should clearly state, in the introduction, that we have no evidence of nuclear origin of excess heat. The error made nearly 17 years ago should not be repeated. It is not difficult to imagine what would happen if Fleischmann and Pons did not speculate about chemically induced nuclear reactions. Instead of discussing theories scientists would focus on validity of experimental data. That would probably lead to subsequent confirmations and open the door for additional research to understand excess heat. Our paper should be strictly experimental and we should be honest about having no idea what causes the observed plasma anomaly. The term cold fusion should not even be mentioned.

We should also not rush toward publishing the paper. We should write and rewrite the draft, discuss is among the CMNS scientists and perform additional experiments, if necessary, to back our claim. Announcing the discovery too early was another big mistake of Flieschmann and Pons. It will probably take a year to check and double-check everything, and to collect supportive evidence from other researchers. We should publish consecutive paper drafts on the restricted discussion list for CMNS researchers. And we will ask them not to refer to our ongoing investigations. I am assuming that everybody on our list will respect our wishes. The paper should not be submitted before we have a setup which demonstrates excess heat in at least 75% experiments. That is why I think it might take another year before we are ready to publish the paper in a reputable journal. A journal should be carefully chosen to avoid editors who are likely to biased.

Appended on 3/22/06:
In a message sent to Richard this morning I wrote: "I think that the 'Fantasizing' part, in the unit #271, should be expanded. What will we do if Scott's results turn out to be negative? Please draft a paragraph about such eventuality. Then we will discuss it."

His reply was: "I think it is very unlikely that Scott's results will be negative. ..... But if they are then we must first assist him by phone, internet, photos and web video. If we could set up the web video I really believe we can walk him to positive results. We should also be prepared to send him our anodes and cathodes. Pierre's assembly has produced the best results. Could we get Pierre to build and test a second assembly and have it ready for Scott? It doesn't have to be identical but should use the same material and have the same size for the anode. Perhaps Pierre wants to build a new assembly and Scott could use the old one? I will also build and test a second assembly but my results so far have been smaller than Pierre's. If Scott still can not get positive results then one of us has to go to Texas and assist him in running the experiment. I would like to go but don't think it will be possible, I only have so much vacation time remaining. But I would be able to fly down for a three day weekend to assist. I think that we should discourage Scott from publishing negative results until we all agree that something was wrong with the French-1, Colorado-2, French-2 and Italian experiments. It would be best if we could discover exactly what was wrong before any publication. We want to avoid giving the field a black eye due to a defective experiment. It we can fix the experiment that would be ideal if not then we should post the negative results and our conclusion as to why the experiment is flawed. It might be best to just do this on your website and on the CMNS list as a discussion thread. CMNS members may have insight into fixing the experiment or challenge our conclusion."

Well, I think that Richard's strategy, about what to do if the Texas-2 results are negative, is correct. We will try to identify the cause of disagreement. But what if the cause is not found? How much time and effort should we invest

before publishing a paper describing the situation? Perhaps it would be better, at some later time, to submit two papers to the same journal, our and his. Let us hope that Scott's results will also show some excess heat.

Appendix:

Those who are not familiar with our jargon would probably appreciate if I explain what the COP (coefficient of productivity) is. Imagine a bathroom heater consuming electric energy at the constant rate of 1000 W and releasing heat at the constant rate of 1100 W. Such magic heater would have the COP=1.1; it would produce 10% more energy at the output (heat) than it received at the input (electricity). The term "magic" is used because, according to the low of conservation of energy, we expect a zero difference between the energy going in and the energy coming out. But suppose that such heater is invented. Would this be a device demonstrating that sometimes the energy is not conserved? Not at all. It would mean that some hidden source of energy exists and it is responsible for the unexpected excess heat.

The COP, in our setups, is simply the ratio of the electric energy received by the cell and the thermal energy released by it at the same time. Our COPs were calculated as Pt/Pe, where Pt is the average thermal power (rate of evaporative and non-evaporative losses of heat), while Pe is the average electric power (rate at which electric energy was used), both in watts. Note that hydrogen is produced in our experiments. That hydrogen is a fuel and burning it produces additional heat. The rate of hydrogen production is sufficiently law to neglect potentially useful chemical energy of hydrogen. The COPs would be only slightly larger than the Pt/Pe if generation of hydrogen were also taken under consideration.

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272) Trying again to confirm reality of excess heat

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Introduction

Those who read previous items (#252, #253. #267 and #270) are probably aware of the fact that two recent attempts to replicate a Mizuno-type results failed. This has been reported, and discussed, at the ICCFF12. Pierre Clauzon, and other people at the ICCF12 (including Mizuno himself), think that we failed to generate excess heat because our protocol did not allow for sufficient loading of Pd with hydrogen. That must be true; the details of the protocol used in Paris were not described in the paper of Fauvarque et al.. That reminded me an observation made by Jed Rothwell last Summer. Jed wrote:

"I think the NEDO fiasco bears an important lesson for both Scott Little and Kowalski: first-hand on-site consultation and training with experts is essential to a test of this nature. You must look over people's shoulders and use their instruments and thoroughly familiarize yourself with the experiment before attempting to do it yourself. In the future, we hope that cold fusion will become more of a science and less of a black art, and more people will be able to do the experiment from a printed recipe. . . The discrepancy between Mizuno's results and Little's are a mystery. I do not think progress toward resolving this mystery will be made until Little and Mizuno spend several weeks working together, perhaps using some of both sets of instruments. Mizuno reportedly has a great deal more space these days in his new lab, so it should be easier to work with him."

After the conference Pierre did invited me to work with him in Paris. We discussed this and decided that working together in Richard's laboratory, in Colorado, would be more desirable. Confirming realty of excess heat in another laboratory, using different instruments, would be more important than observing excess heat again in Paris. Pierre is going to work with us and the purpose of this unit is to describe the results. Jed is right; nothing is better than personal cooperation and working in each others laboratory.

A Mizuno-type experiment, especially when excess heat can be measured in terms of the amount of evaporated water, can be relatively simple and relatively inexpensive. I hope it could be used to produce excess heat on demand. Suppose that Richard and myself convince ourselves that excess heat can indeed be generated at the rate of 50 W or more, as reported at ICCF12. If this happens then independent investigators will
be needed. I hope that some readers of this list will become our partners. The goal would be to demonstrate that a nearly 100% reproducible setup can be built in any lab and used by any qualified researcher. Then, perhaps, something will change in the attitude of mainstream scientists toward excess heat. But we should not repeat the old mistake by saying, prematurely, that the effect is nuclear. In describing and demonstrating the "excess heat on demand" the scientific question will be "what produces the totally unexpected heat?"

That introduction was composed on 2/25/06. I do not know what the outcome of the new sets of experiments will be. But, as suggested by Perre, our results, either positive or negative, will be published. Publishing negative results, as I wrote in unit #281, is as important as publishing negative results, especially when there is a controversy. The cause of discrepancy must be found; it will be either a faulty procedure we followed last Fall or a systematic procedural error in Paris. I think that a line between what is positive and negative should be clearly defined before we begin experimenting. Pierre wrote that the COP in Paris, at 350 V, is between 1.35 and 1.40. On that basis I suggest that any

COP>1.20 should be considered a confirmation of the French result. Any COP below 1.20, on the other hand, should be considered a failure to confirm. But that is only the confirmation aspect of our work. Consistent presence of excess heat at the level of COP>1.10 should be considered a great step forward. It would mean that both teams must continue working to determine limits of accuracy (maximum possible systematic errors) and levels of precision (standard deviation of COP values).

I am not going to write anything else here till the end of March. For the time being this unit will sit on the desktop of my computer. The situation will probably be clear to all of us after one week of intensive work in Colorado.

Appended on 6/29/2009

1) Results of our work were presented at the ICCF12, in Yokohama, Japan, 2005 (15th International Conference on Cold Fusion). The report can be downloaded from

http://lenr-canr.org/acrobat/KowalskiLsearchingfa.pdf

2) As described in Unit 358, a systematic error in measuring electric power with this watt-meter, was found by Clauson and his colleagues. Their most resent results, as reported at ICCF13, are consistent with negligible excess heat in Mizuno-type experiments.

References:

1) Fauvarque, J., P. Clauzon, and G. Lalleve, Abnormal excess heat observed during Mizuno-type experiments. 2005, Laboratoire d'Electrochimie Industrielle, Conservatoire National des Arts et Métiers: Paris. Available as: <http://www.lenr-canr.org/acrobat/FauvarqueJabnormalex.pdf>
2) Ludwik Kowalski, Scott Little, George Luce and Richard Slaughter,
Available as: <http://www.lenr-canr.org/acrobat/FauvarqueJabnormalex.pdf>
3) Mizuno ICCF12
Available as: <http://www.lenr-canr.org/acrobat/FauvarqueJabnormalex.pdf>

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273) A note on microbial transmutation

Ludwik Kowalski (11/29/05)

Montclair State University, Montclair, New Jersey

One of the fields of CMNS (Condensed Matter Nuclear Science) is CT (Cold Transmutation). And microbial induced transmutation is a small part of that field. Last year, during the ICCF12 (12th International Conference on Cold Fusion), I heard about Ukrainian - Russian experiments in that subfield. They were conducted in a laboratory near Chernobyl. Scientists from the University of Kiev and university of Moscow were able to transmute radioactive cesium - ^{137}Cs - into nonradioactive products. Today I learned that more recent phenomena (in the same laboratory) confirmed the earlier report. Radioactive salt was dissolved in water and some kind of bacteria, highly resistant to radiation, were introduced into small (20 cc) flasks. One half of each sealed flask was filled with the solution while the other half was air.

Gamma rays emerging from the loaded flasks was observed for 100 days. During that time radioactivity was reduced, typically by 30%. The half-life of ^{137}Cs, the most troublesome fission product, is 30 years. With the help of bacteria, according to the A.A. Kornilova and V.I. Vysockii report, that material can be eliminated much faster than by natural radioactive decay. Destroying ^{137}Cs, and other radioactive fission products, would be a tremendous boost to nuclear electricity. The problem of storage of radioactive waste would be simplified greatly if Cs and Sr could be destroyed. According to the report, the reduction of radioactivity, measured by several large Ge detectors, could not be due to changes in the counting geometry caused by progressive redistribution of radioactivity. That was my first concern. They used several large detectors; the total solid angle was about 2*Pi.

After hearing the news I said that such important invention, if real, would be a solution of our energy dilemma. Here is how that dilemma was summarized by Dr. Akito Takahashi, the conference organizer, in the opening talk:

a) Energy is an essential component of social progress. Sustained development of the world depends on available energy.

b) Energy resources are limited. Supplies of oil will end in about 60 years. Solar energy (including wind) satisfy no more (?) than 10% of our needs. Fuel for reactors based on ^{235}U will be exhausted in about 50 years. Fuel for breeder reactors will be also be exhausted, sooner or later.

c) Pollution, and climatic changes associated with coal, are very real. Economic developments in China and India depend mostly on burning coal.

d) Industrial hot fusion reactors will not be available for at least another 50 years.

e) Clean fusion devices, in this situation, might be the only possible short-term solution of the worldwide problems associated with energy supplies.

Future of reactor technology, in my opinion, does depend not only on availability of fuel; it also depends on social attitude of toward radioactive waste. Make your technology known to general public, I said to Alla and Vladimir, and those in charge of technological development will beg you for help. Not only would you become rich; you would be recognized as benefactors of mankind.

They smiled. They said that authorities know about their published results and that their proposals have been blocked by those whose business depends on existing technologies. I said that a way to overcome such conspiracies can be found. To convince a potential investor an inventor should also demonstrate willingness to share risks. Suppose, I said, an inventor is offering to invest 50% of his savings into a business if the big investors puts one thousand times more. Those who are able to invest big money probably need assurances that the inventor is serious.

Another thing to do, if a working microbiologic solution to the problem of radioactive waste were at hands, would be address general public. That is where a help from a journalist would be very useful. In a democratic society public opinion can become an important factor influencing what does and what does not happen. Trying to find a journalist, interested in the invention, would be an important part of my strategy. Another part of that strategy would be have a constantly-working setup. Using ^{137}Cs radioactivity, for example, from dry mushrooms, I would start a new experiment every year (for example in a museum).

If only 70% of initial radioactivity remains in the flask after three months then the biological lifetime is about six months. This means that only 25% would remain after one year, only 6% after two years, only 0.4% after three years, etc. Fresh load of bacteria would probably have to be added each year. Such result would be very convincing. Personally I would not stop testing when only 30% of the initial radioactivity is destroyed. I would keep collecting data for two years before announcing the result. Monthly meetings to discuss the results would be organized and students from local schools, as well as scientists, would be invited. Internet summaries of results would be published periodically at my webste.

After listening to the draft of this note, and after correcting my wording in one or two places, Alla and Vladimir told me about their strategy. With help from experienced lawyers they have submitted a patent application in Russia and expect its approval early in 2006. Russian government, if I understood them correctly, will become the co-owner of the patent and will take a large fraction of potential royalties. In exchange for this the government will protect interests of inventors. Recent changes in the Russian patent law make such arrangements possible.

Neither Alla nor Vladimir are biologists; they are nuclear physicists. Alla works at the University of Moscow while Vladimir works at the university of Kiev. They do cooperate with qualified microbiologists. The biologists, however, are only technical advisers; they are not partners. In their position I would try to find a partner who is a recognized authority in the area of microbiology. With that person I would go to microbiologic conferences to address biologists, rather than physical scientists. A proposal from a recognized authority in the area of microbiology would likely to be taken more seriously (by potential investors) than a proposal from a cold fusion researcher. In the present unfortunate situation an association with cold fusion is likely to result in more harm than good. People living near Yucca Mountain, and in other areas where radioactive waste is going to be stored, would naturally become my supporters. With their help turning a working scientific solution into desirable technology would not be difficult. But I would not do anything before being 100% certain that the effect is real.

Appended on 12/1/05
Let me add what another CMNS researcher, Jean-Paul Biberian, told me about microbial induced transmutation. Jean-Paul's laboratory is at the French University in Marseilles. He also conducted research on Microbial transmutation and is totally convinced that the phenomenon is real. I asked Jean-Paul to briefly describe his own research on biologically induced transmutations. Here is his input:

"I conducted biological transmutation experiments with seeds (wheat and oat in a sealed container containing microbes: lacto basilius and sea bacteria). Chemical compositions of material from containers with bacteria and from containers without bacteria were found to be different. No changes were observed in the homogenized material from cells without bacteria. Material in cells with bacteria, also after being homogenized, was depleted of silicon and enriched of calcium. The possible explanation is a nuclear reaction Si + C --> Ca. The growth of wheat seeds was accompanied by the decrease in the concentration of heavy metals, such as mercury and lead. Changes by the factor of nine or more were not uncommon. The homogenized material was analyzed with the ICP-AES and ICP-MS instruments."

That is very interesting. The ICP-AES methods are used to perform elementary analysis while the ICP-MS methods are

used to perform isotopic analysis. Jean-Paul did not study isotopic ratios because the cost of analyzing mass spectra are too high.
Neither the university nor the French government support cold fusion research.

Appended on 12/2/05:
What I did not know, when the above was written, was that Kornilova and Vysockii are deeply involved in the issue project. The hard to believe, issue claims are described on the company website <http://www.iesiusa.com> also (see items #216, #226, #229 and #237 on this website). Yang and Chai were present at the conference but, unfortunately, they preferred not to talk about the issue project. They were listed as coauthors of a paper presented by Vysotskii. The title of the paper was "Observation and investigation of ^{4}He fusion and self-induced electric discharges in turbulent distilled light water." I did not notice any information about generation of ^{4}He. Presence of helium "substantially above the background," was detected via optical spectroscopy. But this happened only when ^{11}B was added to ultra-pure water circulating In the setup.

Helium is presumably produced when protons react with ^{11}B. The origin of these protons is not clear to me. The^{12}C compound nucleus was said to break into three alpha particles releasing 8.7 MeV of kinetic energy. Was the amount of ^{4}He produced consistent with excess heat? This question could not be answered because investigations are in progress. But the abstract of the paper contains this sentence: "Initial calorimetric measurements indicate a significant evolution of thermal energy along with the production of helium, as expected from the mass deficit of the reaction products."

The first author of the paper, by the way, is Koldamasov. Contrary to what I wrote in #216, that Russian researcher is not dead. In a private conversation Kornilova told me that he has some health problems. She insisted on making Kopldamasov the first author because he deserves it. Vysotskii talk followed an extremely interesting presentation made by S. Krivit. Steven was one of those who were invited to visit the company test side. As a journalist he was allowed to film the event. The film showed the apparatus being tested. It also showed Martin Fleischmann (and several other CMNS researchers) desiccating the experiment with Yang and his coworkers. Unfortunately, Steven said, the signed nondisclosure agreement does not allow him to share what else he saw and heard.

After listening to Krivit's presentation I asked about two other claims described at the company website -- generation of hydrogen and the electric generator at Norwood foundry (producing electricity at the rate of 12 MW while using it at the rate of 2 MW). Steven said that he has no permission to answer this question. At that point Yang made an interesting general comment. He said that he is not involved in commercial aspects of the project; his role is to promote technology. At the same time he made it clear that scientific work, as opposed to practical engineering, is being conducted in Russia. I was really surprised to learn that my friends Alla and Vladimir are deeply involved in that project.

Contrary to what I expected, Vysockii's talk was mostly about a "laser-like" electromagnetic radiation (X-rays and visible). The "laser-like" term, in my opinion, was not appropriate. Most people would probably think that "laser-like" means "coherent." But no evidence of coherence was presented. High intensity, and a narrow beam diameter do not make light "laser-like."

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274) Washington Post on Fraud in Science

Ludwik Kowalski; 1/9/2006
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Introduction:
An interesting article on fraudulent science has been published yesterday in an American newspaper. What follows is the letter to the editor I emailed last night. It is followed by my own comments, and by appendix in which the content of the original paper, sent to me by Jed Rothwell is shown.

My letter to the editor:

Dear Editor:

Please publish the attached letter; it refers to the "Trials & Errors" (written, on 1/8/06) by Bettyann Holtzmann Kevles.

Thanks in advance,
Ludwik Kowalski

The content of the attached file:

Accusation without evidence

To the Editor:

In "Trials & Errors" (January 8) Bettyann Kevles points to real dangers in science. But to suggest, without any evidence, that cold fusion (CF) claims are fraudulent is inappropriate.

As a retired physics teacher, and a nuclear science researcher investigating the CF field for more than three years, I have found no evidence of a fraudulent claim. Nor have I found the field pseudoscientific. Scientific methodology of validation is evident in nearly all publications I have read; they are downloadable from <http://www.lenr-canr.org>. At three International CF conferences I witnessed numerous debates and saw no difference between these debates and those at other scientific gatherings. A lot of healthy criticism reflected honesty and dedication. The CF field is unfairly discriminated; but it is alive.

In my opinion, however, that field is still not science; it is only a very promising proto-science. To become science the field needs at least one truly reproducible effect -- an effect recognized by many qualified mainstream scientists. Unfortunately, due to numerous errors made when the discovery was announced in 1989, validating a cold fusion claim became much more difficult than in other areas. This letter is sent from Japan where I came (at my own expense) to assist a university scientist investigating a CF phenomenon. The quality of his research is just as high as in other projects I have worked on.

Ludwik Kowalski
Professor Emeritus
Montclair State University
Montclair, New Jersey.

A message from Jed Rothwell:
Jed is a long time observer of the cold fusion. Last morning he posted the content of the Washington Post article. In a subsequent message he suggested that some of us write letters to the editor with comments about the unfortunate Kevles' article. I am afraid that my input will not be published. It will probably be ignored, like two other, recently submitted letters to editors, one to The New York Times and another to Physics Today. The editors seem to follow a "party line" according to which cold fusion letters should be rejected. This is not limited to letters; it applies to scientific papers as well. Fortunately, the LENR-CANR library, managed by Jed, preserved several hundreds of papers. Future generations of real historians of science will thank him for this work.

My own additional comments:
I suspect that the author of the paper, Bettyann Kevles, was simply not aware that cold fusion is still alive, as I was, up to about 3.5 years ago. This is rather surprising for a Yale University lecturer whose research interests seem to be in area of history of research. She is certainly not alone among those who took premature rejection of curious cold fusion claims for granted and stopped paying attention to what was going on in that field.

But, apart from labeling cold fusion a fraud, some of her observations are valid. Motivation for fraud is very well described by Kevles. She is right that we should be aware of it. She is also correct in saying that fraudulent episodes are not going to hurt established areas of research, such as paleontology. But the situation is dramatically different in proto-science, like CMNS, struggling for recognition. In that case even a single case of fraud can deliver a devastating blow to the entire discipline. The consequences of such episode would be difficult to repair.

One kind of potential fraud was not mentioned in the article. It is possible use of honest research to support fraudulent manipulations of financial investments. Secrecy is not compatible with science. But it is compatible with business; financial officers are not obligated to reveal work in progress before soliciting investments. Responding to a request for clarification they may say, for example, that "nothing can be revealed to protect a patent application". Secrecy makes us vulnerable. What would the effect of a disaster, in the area of financial manipulation of proto-scientific claim, be? Would it destroy reputation of honest scientists whose work has nothing to do with the disaster? How would it affect reputation of scientists whose work was actually exploited by financial wheelers dealers? That is difficult to predict. I hope, perhaps naively, that such things will not happen.

But danger is real. The big question is how to protect the CMNS field from fool's gold sellers. Hope for the best, assume nothing, suspect everything and ask good questions? That sounds like a good advice. But not in an area protected by secrecy. The essence of science is its methodology of validation, and its openness. And even that is not enough, as far as an individual is concerned. One has to be trained to ask right questions, and to understand the answers. Dependence on recognized authorities, and on institutions, such national academies, national laboratories, etc. is also very important.

A fraudulent scandal whose probability is small, will happen, sooner or later. Fortunately, nothing of that kind surfaced in the nearly 17 years since the discovery of cold fusion was announced. But it can happen any day. This might kill the entire field, unless it is no longer proto-science. Unresolved controversies about CMNS should be resolved as soon as possible. In my opinion an injection of research founds, combined with an official rehabilitation, would be perfectly justified at this level of development. Even a very modest support, less than one percent of what is being spent on hot fusion, would be a big help. Major CMNS researchers are old; their knowledge and skills will not be available in another decade or so. Young scientists, on the other hand, will not commit themselves to a highly discriminated field. They know that research in a "forbidden area" could hurt them, even if their work is excellent. That is the tragedy of the present situation.

Appendix 1:

Trials & Errors: Barely a Drop of Fraud; Why It Shouldn't Taint Our View of Science
From the Washington Post, Jan. 8, 2006; page B03.
By Bettyann Holtzmann Kevles

Seldom in our history have fame, fortune or a heady mix of the two tempted so many people into committing fraud. The halls of Congress are reverberating with the jingle of hastily discarded donations as elected officials distance themselves from lobbyist Jack Abramoff. Onetime employees have not forgotten Enron, WorldCom and the giant compensation packages of failed CEOs.

No surprise, then, that South Korean biologist Hwang Woo Suk, the putative creator of Snuppy, the first cloned dog, should come to occupy the spotlight of suspicion. There is doubtless a sense of schadenfreude among people envious of -- and at the same time fearful of -- scientists whose work they only partially understand but nonetheless depend on. Some are even asking whether biomedical research can be trusted.

But the specter of a cloud of fraud hanging over the microscopes and telescopes of scientists around the world is largely imaginary. It is true that there have been some great scientific misdeeds in the past. Who can forget Piltdown Man , the manufactured fossil skull that puzzled anthropologists for decades? Or the claims of the discovery of cold fusion in 1989 at the University of Utah? But those examples are famous because they are so rare. And, as the South Korean stem cell case shows, the scientific process means that frauds are typically revealed before they harm anything but the reputations of the perpetrators themselves. The far greater risk is that they erode our faith in science. . . .

THE REST OF THE THE KEVLES' ARTICLE WILL BE ADDED WHEN PERMISSION IS GRANTED.

P.S. (1/20/06)
I do not know why neither Washington Post nor Dr. Kevles replied to my messages.

Appendix 2: Added on 1/26/06

What follows are comments on Kevles' article made by Edmund Storms. His letter to Yale Daily News (first item below) was published in that newspaper on 1/13/06. The second item is the letter to the editor of Yale Daily; it has not yiet been published, as far as I know. Ed gave me permission to post his message, as below.

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I AM WAITING TO MAKE SURE THAT ED HAS NO OBJECTION FOR POSTING HIS PIECES HERE.

Appendix 3: Added on 1/29/09

What follows is a message that Dr. Robert Bass posted at a restricted Internet list for the members of ICCMNS (International Society of Condensed Nuclear Matter Science). Replying to my request for permission Robert wrote: "A friend pointed out that there IS a Yale connection [Dr. Mitchell Swartz's collaborator Dr. Alex Frank IS a Yale Graduate] so I have made a couple of minor additions/correctionsand wish now to submit for publication the following REVISED version:"

= =

Professor Kevles rebuttal still shockingly misinformed!

The letter from Prof. Kevles published on Tuesday, January 17 remains shockingly replete with demonstrably false statements. As Nobel Laureate physicist Julian Schwinger pointed out at the time, the claims of Fleischmann & Pons (F&P) did NOT "contradict the known laws of physics" because "the circumstances of cold fusion are different" than the circumstances in which the theoretical predictions pertaining to conventional hot fusion were made, i.e. in a

rarified gaseous plasma wherein fusion reactions between two deuterons can be treated as a collision between two isolated particles in a vacuum, contrasted with a periodic lattice of deuterons embedded in a Palladium lattice.

On my website www.innoventek.com, in the Science sub-site, the reader can find Schwinger's 1990 ICCF1 paper, together with my 2005 MIT Colloquium slideshow containing references to my paper at ICCF4 (Dec. 6-9, 1993), based in part on pro-CF papers by Schwinger and by Nobel Laureate physicist Willis Lamb, which passes the "Rabinowitz Acid Test" in that my theory ("albeit crude" in Schwinger's words) predicts that an F&P experiment of electrolysis with Pd cathodes will produce aneutronic d+d fusion & excess heat in the case of heavy water but will fail with ordinary water, whereas both d fusion & p fusion will work in a Nickel cathode. It is not true that the Utah legislature made available $1 Million to F&P; the Utah "National Cold Fusion Institute" was directed by former GE electrochemist Dr. Fritz Will, who did achieve a reproducible protocol for producing radioactive tritium via deuterium fusion in an F&P type of experiment before giving up, though this more conventional reaction established that contrary to Establishment dogma Low Energy Nuclear Reactions (LENRs) are indeed possible.

Distinguished electrochemist John Bockris of Texas A&M not only replicated the F&P aneutronic excess heat but found massive amounts of the nuclear "ash" (helium-4) in a Pd cathode which had been quick-frozen in liquid nitrogen while producing heat. Similarly electrochemist Dr. Melvin Miles of the Naval Research Labs not only duplicated the F&P excess heat but measured the helium "ash" in sufficient amount to "explain" the heat. Other early F&P replicators included Dr. Dennis Cravens, Dr. Edmund Storms (of LANL), Dr. Michael McKubre (of SRI International) and Drs. Mitchell Swartz & Alex Frank (of www.theworld.com/~mica/cft.html).

As documented in the recent books of Beaudette and of Krivit, which Prof. Kevles has evidently not read, the most notorious "failures to replicate," in the cases of Caltech, MIT, and Harwell (UK) reflect negatively upon those famous institutions. The neophytes at Caltech evidently had not read the earlier papers of F&P because they neglected to check that their "loading" had passed beyond alpha phase into the known beta phase. At the MIT Fusion Plasma lab there was outright criminal "fraud on the public" in that a curve which oscillated around 10% excess heat was, prior to publication, artificially moved down to oscillate around zero excess heat! As Dr. Mike Melich demonstrated at ICCF4, after he had prevailed upon Harwell to release their raw data for his study, they had given up prematurely because they had actually achieved positive results in the form of bursts of excess power using heavy water, but nonesuch ever when using ordinary water (and had been receiving active cooperation from Fleischmann contrary to Prof. Kevles's falsehood that F&P had "persistently refused" to supply colleagues with more information).

To verify the utter incorrectness of the Establishment version of this historical episode, which Kevles irresponsibly parrots, the reader need only read the cited two books & visit the cited websites and also visit www.LENR-CANR.org and www.newenergytimes.com for overwhelming documentation. For justified sarcasm about all this from a Nobel Laureate, visit http://jcbmac.chem.brown.edu/baird/coldfusion/schwinger.html. Shortly before his death in 1993, this Nobel Laureate physics professor wrote: "…my first topic, cold fusion, has caused many eyebrows to raise. Cold fusion? Isn't all that nonsense dead and buried? How can anyone be so insane as to talk about this totally discredited subject?" Sadly, Dr. Robert W. Bass, Prof. of Physics & Astronomy, BYU (1971-81, retired), donquixote@innoventek.com [no connection with Yale, though Dr. A. Frank mentioned above is a Yale graduate]

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Dr. Robert W. Bass, *M.A. Oxon* [Rhodes Scholar]
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P.S. (1/30/06): I am going to send the URL of this item to Dr. Kevles and ask her to reply to our comments. If she does then I will be happy to append her input. I do not think that she will disagree with our attempts to open a path of reconciliation between CF researchers and mainstream scientists. She was simply not informed (and not interested) in what others wrote about CF after the first two or three years of great excitement. I was in the same situation up to about four years ago. She will probably be with us, after learning that we are not charlatans or con artists. Her input below will probably enrich this document.

Appendix 4: Added on ???

PLACE RESERVED FOR DR. KEVELS, IF SHE ALLOWS ME TO SHERE HER REPLY.

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276) Bars of errors

Ludwik Kowalski; 2/3/2006
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About three days ago I posted the following message on the restricted list for CMNS researchers:

In reading it again I decided to add a short tutorial. How can a retired teacher resist an opportunity to share what he learned recently. First something well known then something that I was not aware of. I will use a Geiger counter as an illustration.

1) Suppose that the number of counts, from a single experiment, is N = 100. We then say that the expected distribution of N (if the experiment could be repeated many times) is Gaussian whose standard deviation is the sqr(100)=10. We can say, at the level of 68% confidence, that the true value is between 90 and 110 (mean plus or minus one sigma). And at the level of 90% confidence the true value is between 80 and 120 (mean plus or minus two sigmas).

2) But this approach is valid only when N are not too small. How to establish the range of values (bar of expected errors) corresponding to the 90% level of confidence when N is smaller than 10? By using the following table:

	90% confidence
0	0.00-2.44
1	0.11-4.36
2	0.53-5.91
3	1.10-7.42
4	1.47-8.00
5	1.84-9.99
6	2.21-11.47
7	3.56-12.53
8	3.96-13.99
9	4.36-15.30
10	5.50-16.50

If N=4 then we can say, "at the confidence level of 90%, the true value is between 1.47 and 8.00." Likewise, if N=9 then we can say, "at the confidence level of 90%, that the true value is between 4.36 and 15.30. That not exactly what we would say if the error bar range was +/- 2*sqr(N). For N=4, for example, the range of values would be 0 to 6 instead and not 1.47 to 8. For N=9, the range of values would be 3 to 15 and not 4.36 t0 15.30. The differences become less and less significant as N becomes larger. I did not penetrate the mathematical derivation but I accept the rule because I know that a Poisson distribution of random counts become practically indistinguishable from the symmetric Gaussian distribution for a large N. A similar table was constructed by statisticians for the 95% level of confidence, as shown below.

	95% confidence
0	0.00-3.09
1	0.05-5.14
2	0.36-6.72
3	0.82-8.25
4	1.37-9.76
5	1.84-11.26
6	2.21-12.75
7	2.58-13.81
8	2.94-15.29
9	4.36-16.77
10	4.75-17.82

I think that my illustration on how to use these two tables is correct. But I am not sure. Please correct me, if necessary. The document with mathematical derivations, from which these two tables were taken, begins as follows: "Revised

April 1998 by F. James (CERN); February 2000 by R. Cousins (UCLA); October 2001 and October 2003 by G. Cowan (RHUL)." The footnote of the first page states "CITATION: S. Eidelman et al., Physics Letters B592, 1 (2004) available on the PDG WWW pages (URL http://pdg.lbl.gov/) June 17, 2004, 10:56"

3) It should be obvious to most of you that I am addressing the issue of precision (random errors) and not the issue of accuracy (systematic errors). The issue of systematic errors has to do with the concept of reproducibility.

For small N the bar of error becomes larger above the plotted point than below the point. What about the way of establishing bars of errors when the background is subtracted. The estimated net result, R, is always calculated as

R = A - B

where A is the apparent value and B is the background value. For A=100 and B=36, for example, R=64. We want to say, for example, that, “at the confidence level of 95%, the true value is between x1 and x2.” How to calculate x1 and x2? The well known rule, when A and B are not too small, tells us x1 and x2 should differ from R by two standard deviations. In this illustration, x1=64-2*11.66 = 40.68 and x2=64+2*11.66 = 87.32. That is because the standard deviation of R is the square root of A+B.

But how should x1 and x2 be determined, for a chosen level of confidence, when A and B are very small numbers, for example, 9 and 4? I know that distributions of A and B, for small mean values, are Poissonian rather than Gaussian. But I do not know how to turn this into a practical rule for calculating x1 and x2.

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277) A strange outburst of messages

Ludwik Kowalski; 2/7/2006
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1) Introduction:
What is the essence of the CF (cold fusion) controversy? It is not the excess heat per se; it is its nuclear origin. This topic surfaced in a recent outburst of messages that appeared on the restricted CMNS list. The first message was posted Jed Rothwell, the author of a book about cold fusion. That book, by the way, is downloadable (as a pdf file) from the library at < www.lenr-canr.org>.

Long time ago Jed gave me permission to quote anything he posts on the CMNS list. That is why only contributions from two of us are shown below. Others also contributed to the outburst but I am not going to ask them for permissions. On Jan 28, 2006, quoting the Desert News, newspaper at Salt Lake City,

2) Jed wrote:
. . . He [Jones] continues to do research on cold fusion, which he prefers to call metal-catalyzed fusion "to distinguish it from the claims" of former University of Utah chemistry professors B. Stanley Pons and Martin Fleischmann, "which we do not accept as verified. This is the first time I have been pleased to see someone attack cold fusion in the press. I would not want someone who supports cold fusion to be associated with Jones' views about 9/11. [Jones' views are described at:]

http://www.physics.byu.edu/research/energy/htm7.html

It does not surprise me that Jones reached this conclusion. The 9/11 disaster has been investigated in more depth, by more experts, than any other disaster in history. Their findings are based on massive, irrefutable evidence grounded in basic physics and engineering. The same is true of cold fusion, of course. Cold fusion is actually even more solid, because it has been independently reproduced in hundreds of laboratories with full instrumentation, whereas the 9/11 building collapse only happened two times, and will never be reproduced full-scale again. It takes a special kind of imagination to ignore the scientific method and to deny the reality of overwhelming physical evidence.

3) In another message (Feb. 4, 2006) Jed wrote:
Steve Jones has often gone on record claiming that all excess heat results in cold fusion are wrong. See, for example:

http://www.lenr-canr.org/acrobat/JonesSEchasingano.pdf

He often claims that all heat is caused by recombination. Several people, including me, have asked him how this can be true with closed cells such as McKubre's. As far as I know he has never answered this question.

4) In another message Jed wrote (in part):
Steve Jones has often gone on record claiming that all excess heat results
in cold fusion are wrong. See, for example:

http://www.lenr-canr.org/acrobat/JonesSEchasingano.pdf

That was one example. There are many others in his papers and e-mail messages. His claims that recombination can

explain McKubre's results, for example, are elsewhere. He refers to x-ray film as "crude." There are two problems with that: It is not a bit crude. It is completely reliable and very sensitive. It is all that most researchers can afford, as the Russians pointed out at ICCF12. Jones has campaigned and pulled strings to prevent funding for CF, reducing researcher to shoestring budgets. Then he has the chutzpah to say the results are not good because the budget is too small! You have to know the background. He wrote: "It is disquieting that some researchers select open electrolytic cells over closed cells, and excessively long sampling intervals (e.g., 5-minute sampling intervals for input voltage used by Pons and Fleischmann in calculating excess heat over a 10-minute boiling period [3])."

This is outrageous nonsense, but Jones has repeated it ad nauseam. He has taken one measurement out of context, ignored all the other measurements and instruments including oscilloscopes, and ignored the implications of this particular measurement, and the fact that it was repeated 64 times in each run, and the runs were repeated many times. A 10-minute boiling period repeated several hundred times adds up to a large number of data points. Plus later boiling runs lasted for weeks, not 10 minutes. Fleischmann himself showed Jones the oscilloscope data at a conference, in person, while lecturing in front of dozens of people, but Jones will never admit that or discuss it.

This is not a controversy. Jones has a right to his opinions, and I have no problem uploading his papers to LENR-CANR.org. He categorically denies that any cold fusion experiment anywhere has ever produced excess heat. He has told me that many times, in person, and he has written it. Many other people such as Taubes and Park agree with him. I am not accusing him of anything; I am merely reporting his opinions. He may not wish to emphasize them here, but he has never hidden them as far as I know. I assume he is sincere. He honestly believes there is no heat, just as he honestly believes the Twin Towers disaster was caused by explosives, not airplanes. To paraphrase Chesterton, when a man stops believing experimentally proven facts, he doesn't then believe in nothing, he believes anything.

. . . There is no question about the origin [of excess heat]. Many cells are closed, and others are gas loaded, so recombination is ruled out. No cold fusion cell has ever had any significant amount of chemical fuel, and no chemical ash has ever been found, so chemical heat is ruled out. Hundreds of cells have produced tritium, x-rays, helium, and transmutations, so obviously a nuclear reaction is occurring. The only possible explanation for the excess heat is that it comes from an aneutronic (or nearly aneutronic) nuclear reaction. Any other explanation is out of the question, absurd and illogical..... ”

5) In another message Jed wrote:
I wrote: “Jones has campaigned and pulled strings to prevent funding for CF, reducing researcher to shoestring budgets. Then he has the chutzpah to say the results are not good because the budget is too small! “ I should explain what I mean, for the benefit of people who do not closely follow the dismal & petty inside story of cold fusion politics. The views of Jones and his allies dominate the DoE 2004 review --

http://lenr-canr.org/acrobat/DOEreportofth.pdf

where it says: "A second area of investigation is the use of state-of-the-art apparatus and techniques to search for fusion events in thin deuterated foils. Several reviewers specifically stated that more experiments similar in nature to those that have been carried out for the past fifteen years are unlikely to advance knowledge in this area." In plain English, this means: Stop looking at excess heat! No more calorimetry! Let us do Jones' style experiments looking for particles, and let us pretend that Sigma 90 excess heat is marginal, or better yet it never happened."

Also: "The current reviewers identified a number of basic science research areas that could be helpful in resolving some of the controversies in the field, two of which were: 1) material science aspects of deuterated metals using modern characterization techniques, and 2) the study of particles reportedly emitted from deuterated foils using state-of-the-art apparatus and methods..... " Translation: Heat is not controversial; it simply does not exist. And oh by the way, if Melvin Miles asks us for funding to find it again, we will blow him away. See:

http://www.lenr-canr.org/acrobat/LENRCANRthedoelies.pdf

You have to read between the lines a little. It helps to know the players.

(6) Ludwik wrote: (referring only to what he underlined in Jed's message)

a) The sensitivity of an X-ray film is not as good as that of a photo-multiplier; some P.M. tubes can be used to count single photons.

b) A film can only tall us that some radiation, able to produce latent image, was present. All by itself a film does not tell us anything about the kind of radiation. A darkness on a film exposed to beta particles, for example, is not at all distinguishable from the darkness due to X-rays, gamma rays, visible light, neutrons, protons etc. Neither does a film provide information about the energy of particles or photons (except when filters are used).

c) There are instruments that provide more information about radiation. That is why I tend to agree that, in some cases, films are crude detectors.

d) X-ray films recording cumulative darkening should not be confused with thick nuclear emulsions, or other track detectors.

e) For many purposes X-ray films are highly appropriate. That is why a statement about them ("crude" versus "completely reliable") may or may not be correct. I would prefer to discuss such statements in the context of information needed in particular experiments.

f) Do not forget that radioactivity was discovered by Becquerel by using crude photographic films.

g) Suppose an x-ray film, wrapped in dark paper, is applied to a Pd cathode, after it was used to generate excess heat. Suppose that darkness is observed at some locations. That would be a strong indication that parts of the film were exposed to some kind of radiation. Dark paper is a filter and visible light can be excluded, unless it was somehow produced inside the dark paper envelope. Very soft particles can also be ruled out. What is left? X-rays, gamma rays, neutrons and particles able to traverse the paper.

h) Normally initial observations would be followed by repeating the experiment and by using different kinds of detectors. But such luxury does not exist in our field. Nothing serious can be done unless experiments become reproducible.

(7) Jed replied:
You can learn a lot more from film than most people realize. Some of the Italian ENEA papers describing x-ray film findings have described the radiation in great detail, using computer modeling, based on things like the shadow of the anode and its diffraction." The people at BARC who made the image shown here:

http://www.lenr-canr.org/Experiments.htm#AutoradiographsMSrinivasan

also used sophisticated detectors on this and other samples. And they made about 100 images of this sample over a year, which can be compared to learn more. They took elaborate precautions to exclude the effects of light or chemistry. The Italians have also use film plus other instrument types. .....The experiments were reproducible enough to do these studies back in 1990. . . .

8) Ludwik's message:
Jed Rothwell wrote: "The only possible explanation for the excess heat is that it comes from an aneutronic (or nearly aneutronic) nuclear reaction. Any other explanation is out of the question, absurd and illogical." Why do you exclude a possibility that the excess heat is due to a form of energy that is not yet recognized (neither chemical not nuclear). The only convincing evidence of nuclear origin is compatible amount of new elements and new isotopes. . . .

9) Jed asked:
Do you mean something like ZPE or the Mills hypothesis? I concede they are remote possibilities, but I think they are ruled out by the nuclear effects. I know little about theory, but I gather Mills is generally considered impossible. . . . That would be nuclear fusion, right? What is your point?

10) Ludwik:
No, I did not mean ZPE (zero point energy) or the Mills hypothesis. I was simply saying that, by definition, nuclear energy is energy released when a nuclear transformation takes place. A nuclear transformation is a process in which new isotopes or new isobars are created. Such isotopes and isobars, by the way, are usually called "nuclear ash." By accepting such restricted definition one should agree that the only convincing argument for the nuclear origin of excess heat is production of new elements or new isotopic ratios.

Why is this definition restricted? Because gamma decays are also nuclear processes. And we know that neither Z (number of protons) nor N (number of neutrons) changes in gamma decays. Most often gamma decays follow nuclear processes in which either Z or N (or both) change, as in beta decay or alpha decay. That is why I think that generation of excess energy of nuclear origin must produce a corresponding amount of "nuclear ash."

The confirmed evidence of that kind, as far as I know, is accumulation of 4He releasing ~23 MeV of heat per atom. It was reported by several researchers in several countries. Such accumulation can be logically explained by the D-D fusion facilitated
by some kind of screening (lowering of the effective coulomb barrier in condensed matter). Yes, the 2D+2D --> 4He reaction is not-neutronic, not-protonic and not-alphonic. Yes, 4He is a new isotope and its accumulation, at the rate consistent with the rate at which excess heat is generated, is a very convincing argument. I used this reaction as an illustrative example. Other nuclear processes have been mentioned at our CMNS conferences. In my opinion the next "official" investigation of CF should be focused on generation of 4He from 2D. That is the strongest existing argument for the nuclear origin of excess heat.

P.S.
11) Ludwik, after reading replies from several people:
It was interesting to read what others think about the 9/11/02 tragedy, or about the ongoing war, etc. But such topics are not related to the purpose of our CMNS list. What I would prefer to hear from those who participated in this thread were comments on my statement. Let me repeat it again: **If nuclear reactions are defined as processes in which "nuclear ashes" are produced then the most convincing arguments, for the nuclear origin of excess heat, are demonstrations showing presence of "nuclear ashes."**

(a) Do you people agree that accumulation of helium at the rate of one atom per ~23 MeV of excess heat is our best argument so far? Accumulation of 4He was reported many times, by many people, in many labs. (b) "Excess heat without nuclear ashes" would be a clear indication that something else (not nuclear reactions) is responsible for excess heat. Do you agree?

12) Jed:
You wrote: “Do you people agree that accumulation of helium at the rate of one atom per ~23 MeV of excess heat is our best argument so far? Accumulation of 4He was reported many times, by many people, in many labs.” I quibble with this. Measuring helium is difficult, and only a few researchers have the equipment needed to do a convincing job. The "best argument" is a somewhat nebulous concept, and partly a matter of taste. You might argue that tritium or x-rays are the best proof that cold fusion is nuclear. (Or, at least, that it must have a nuclear component.) Some cold fusion reactions do not produce measurable amounts of tritium. But because tritium is radioactive, it is much easier to detect than helium, and because it sometimes appears in massive quantities no one can deny it is there.

I expect that all cold fusion reactions produce helium, but that is impossible to confirm, because measuring helium is difficult and because many experimental setups preclude the possibility of measuring it. In other words, I vote for evidence which is strong and convincing, rather than always present. Evidently, cold fusion is complicated and the reaction products vary -- sometimes it makes tritium, and sometimes it does not. When tritium is present and you record something like the autoradiograph from BARC, I would say that is the best proof. See:

http://lenr-canr.org/Experiments.htm#AutoradiographsMSrinivasan

2) You wrote "Excess heat without nuclear ashes would be a clear indication that something else (not nuclear

reactions) is responsible for excess heat. Do you agree?" How can you be sure there are no nuclear ashes? It is easy to confirm there is no commensurate chemical ash is easy, because it would have to be present in macroscopic amounts. But trying to prove there is not 1 ppm of extra helium sounds like an exercise in futility.

Cold fusion might be a nuclear effect plus something else. Perhaps the tritium is only a side effect of Mills' shrinking atoms, or what-have-you. But whatever else it may be, it also affects the nucleus, and transmutes elements. We can be absolutely sure it is not a chemical reaction, or recombination, or a mistake. That is decisively ruled out. At present that is all we can be sure of. If a theory emerges that can be experimentally tested and proved, then we will know what sort of nuclear effect cold fusion is. Until then we must live with a degree of uncertainty.

13) Ludwik:
You wrote: "Measuring helium is difficult, and only a few researchers have the equipment needed to do a convincing job. The "best argument" is a somewhat nebulous concept, and partly a matter of taste. You might argue that tritium or x-rays are the best proof that cold fusion is nuclear. (Or, at least, that it must have a nuclear component.) Some cold fusion reactions do not produce measurable amounts of tritium. But because tritium is radioactive, it is much easier to detect than helium, and because it sometimes appears in massive quantities no one can deny it is there."

Tritium, 3H, is a nuclear ash and Jed is correct that it is easier to measure, even in very small amounts, than 4He. I am aware of several reports on generation of 4He at the rate close to one atom per 23 MeV of excess heat. But I was not aware of similar reports about 3H. On the contrary, several people emphasized that the rates of accumulation of 3H are always many orders of magnitude smaller that what would be consistent with the D(d,p)3H reaction producing excess heat. Here is a numerical illustration.

The Q of the D(d,p)3H reaction is about 4 MeV. But only 50% of D+D fusion events generates 3H. So on the average we expect 2 MeV of excess heat per fusion. Suppose that 1,000,000 fusion events take place in each second. The corresponding rate of generation of excess heat would be 2,000,000 MeV per second. Knowing that 1 MeV is the same as 1.6*10^-13 J we translate this into 0.32 micro-watts. Suppose the excess heat is generated at the rate that is one million times larger (0.32 watts). That would produce 3H at the rate of 10^12 atoms per second. If the experiment lasts about 3 hours (10000 seconds) then the number of 3H atoms produced would be N=10^16. For that amount of nuclear ash the activity would be 17.8 billion beta particles per second. Jed is correct, " tritium is radioactive, it is much easier to detect than helium." But, as far as I know, those who reported reproducible excess heat never observed such levels of 3H radioactivity. That is why I still think that the best argument for the nuclear origin of excess heat is accumulation of helium at the expected rate.

You also wrote ". . . How can you be sure there are no nuclear ashes? It is easy to confirm there is no commensurate chemical ash is easy, because it would have to be present in macroscopic amounts. But trying to prove there is not 1 ppm of extra helium sounds like an exercise in futility." But is it not true that accumulation of 4He, at expected rates, has been reported by several CMNS researchers?

We should not confuse two very different kinds of claims: (a) Nuclear signatures -- tritium, neutrons, protons, alpha particles, gamma rays, and (above all) abnormal isotopic ratios -- have been reported by CMNS researchers. (b) CMNS researchers presented evidence for nuclear origin of excess heat.

14) Jed:
You wrote: "But is it not true that accumulation of 4He, at expected rates, has been reported by several CMNS researchers? " Not that many, but some have been convincing. The ones I recall include Miles, Bush, McKubre (especially with the Case cell), and several researchers at ENEA Frascati. Some researchers have reported helium-4, but they have not tried to correlate it with heat. I think Chein (1992) is an example.

You also wrote: "We should not confuse two very different kinds of claims: (a) Nuclear signatures -- tritium, neutrons, protons, alpha particles, gamma rays, and (above all) abnormal isotopic ratios -- have been reported by CMNS researchers. (b) CMNS researchers presented evidence for nuclear origin of excess heat."

This strikes me as a false distinction, or one that serves no purpose. There is no telling which type of evidence will be

more valuable to a theoretician, and there is no telling which is actually "better" or "the primary origin of the nuclear events." Perhaps there is no primary origin, and many different reactions produce heat. Suppose someday a theoretician looks at the tritium and the heavy-element transmutations in Iwamura's cells, and suddenly this theoretician sees how these two apparently disparate products fit together in one picture, and how a single underlying reaction produces tritium in one situation and Mo in another. Extreme or "outlier" effects are sometimes more useful to establish a theory than more common effects. Helium may be produced by every CF reaction, whereas Mo production only occurs in special circumstances, but the Mo may still tell us something that helium does not.

Furthermore, even if helium does always occur, the Mo transmutation may not be a "side effect" in any sense. Yes, perhaps it is a side-effect caused by particles from the primary helium-generating reaction. Then again, perhaps it is the primary CF reaction in this case, and no helium is involved. We do not know yet. Iwamura is not able to measure helium, as far as I know. Tritium is also impossible to judge at this stage. It may be that reactions on some areas of the cathode produce tritium but not helium. Perhaps both of these reactions also produce heat (at different rates). Both are nuclear signatures, and for all we know both may be the nuclear origin of excess heat. In other words, this question may be like asking: "In a pile of burning tarpaper and firewood that produces black and white smoke, you have two different combustion reactions occurring, with different products. Which of these two reactions is producing the excess heat? Which points to the origin of combustion?"

15)Jed:
There is one nuclear signature which probably is ether secondary or completely unrelated to cold fusion: neutrons. Ed Storms and others have speculated that the neutrons observed in CF experiments may be produced by very low level conventional reactions such as fracto-fusion. I think Ed believes Taleyarkhan's sonofusion effect is also probably a form of plasma fusion with no connection to CF. Perhaps the neutrons from Ti reported by Jones et al. are also caused by low level hot fusion, and cannot help us discover the origin of heat producing CF reactions. See:

http://lenr-canr.org/acrobat/JonesSEneutronemi.pdf

In other words, it isn't that CF reactions produce anomalously low levels of neutrons; actually they produce no neutrons all, and a few neutrons have been caused by coincidental hot fusion. This seems intuitively likely to me. I have never understood why neutron production would be reduced by 11 orders of magnitude but not 12 or 15 or 20 orders. Why would there be just enough neutrons left to detect in some cases? If the cold fusion reaction can combine deuterium without emitting a neutron, why would it fail to do that once in 1E11 cases, and suddenly "revert" to a hot fusion path? (Of course the notion of "reverting" is silly. It refers to the fact that people discovered hot fusion first. It is not as if the cold fusion reaction learn how to generate itself in 1989, and from time to time it forgets that it is not hot fusion.) Given all the attention paid to neutrons in the early days of the research, it would be ironic if this turned out to be the case.

Ludwik, 2/9/06
. . . I guess I know what XX has on mind. But I am not sure. He is certainly correct that all ideas are worth exploring. I would like to hear from people who did study generation of 4He associated with excess heat. Do they agree with the suggestion than the most convincing argument in favor of "nuclear origin of excess heat" is generation of 4He at the rate of several MeV per atom? It does not have to be exactly 23.67 MeV, considering the difficulties mentioned by YY. The curves I have seen did have large error bars. But they displayed many experimental data points. The trends were compatible with the expected Q value of the pure D+D-->4He. Is this still a correct observation? I would also like to know if the accumulation rate of 3H (tritium) has ever been reported as compatible with at least one mW of excess heat. If the rate of tritium generation is compatible (even within one order of magnitude) with the rate of producing excess heat then Jed is correct that production of 3H should be a more convincing argument (for the nuclear origin of excess heat) than accumulation of 4He. Small quantities of 3H are certainly much easier to measure than comparable quantities of 4He.

Jed's reply:
This [adjective “non-chemical”] makes no sense to me. Obviously, cold fusion is a nuclear effect! It changes the nucleus. It produces tritium, helium and heavy element transmutations. That makes a nuclear by definition. Perhaps it is a combination of fission and fusion, but it definitely is nuclear, as anyone can see. Why try to hide that fact by

changing the name? Calling in "non-chemical" is a kind of euphemism. A euphemism is a substitute word intended to cover up some embarrassing or awkward reality. . . .

Ludwik:
Once again I want to emphasize that the two issues: (a) “nuclear origin of excess heat” and (b) “cold fusion is a nuclear effect,” should not be confused. In this thread I am focusing on (a), not on (b). What is our best evidence that at least one kind of excess heat has nuclear origin? For the time being I still think that it is the D+D-->4He fusion (at the rate of several MeV per 4He atom). But I want to hear from those who performed D+D-->4He experiments. Why don't they defend their reports on this list? Neither Jed (?) nor myself have experience with that kind of work. My position is based on several reports, not just one.

Jed:
The best evidence is that the heat exceeds the limits of chemistry by many orders of magnitude. When Fleischmann and Pons saw this they knew they were seeing a nuclear reaction, not a chemical reaction. Even though we do not know the exact nature of the nuclear reaction, we can be certain it is nuclear. I am, of course, assuming that only three sources of heat exist: (1) Mechanical, (2). Chemical and (3) Nuclear.

I do not understand what you mean by, "at least one kind of excess heat." I assume there is only one kind of excess heat, and it must be nuclear. Perhaps there are some varieties or different paths of nuclear reactions, but as I said in my analogy to combustion, they are fundamentally the same.

Not only does the heat prove beyond doubt that cold fusion is a nuclear effect, it is also, as Fleischmann says, the "principal signature of the reaction." That is to say it is the most prominent, the easiest to detect, and the most certain proof that the reaction is nuclear. As a general rule, if you do not detect heat, you cannot be sure a cold fusion reaction is occurring. You should confirm the heat before you begin to look for helium and other nuclear products. Otherwise you may be fishing in a dry hole. The only exceptions to this rule so far has been the Claytor and Iwamura experiments. The signatures in these experiments are tritium and transmutations, respectively, but I assume the reactions also produce excess heat. However, the heat it too small to be detected, and their instruments preclude calorimetry in any case.

Ludwik:
That explains our disagreement. Nature, however, is more complex. Two days ago ZZ promised to contribute to this thread. I am probably not the only person interested in what he is going to say about the nuclear origin of excess heat. Contributions from other experienced researchers will probably also be appreciated by many on this list.

Jed:
I hesitate to ask, but what other sources are there? Energy comes in several different forms, such as heat and electricity, but as far as I know it only originates from these three sources. "Chemical" is shorthand for anything that affects only electrons and molecules, not the nucleus. "Nuclear" would include fission, fusion and falling into a black hole I suppose.

Ludwik:
Heat can be produced by slowing down electric charges, by moving magnets or electromagnets, by annihilating antimatter, etc. Even sound can produce heat in a wall, usually very little. Radio waves also produce small amount of heat, even in our bodies (but much more in a frankfurter placed in microwave ovens). The E=m*c^2 is not limited to nuclear reactions. Other forms of energy will probably be invented to explain things to be discovered. Energy is not a simple concept. I was thinking about this recently, but not in the context of CMNS.

Ludwik (not posted):
A well known CMNS researcher, who commented on this thread, summarized difficulties associated with helium work. That researcher thinks that although accumulation of 4He is a strong argument (for the nuclear origin of excess heat), the accumulation of 3He, resulting from the decay of 3H, in the Arata/Zhang experiment, is even more convincing. I already wrote about Arata's experiment in item #23. But I do not recall that the amount of 3He detected was said to be consistent with the amount of the excess heat produced. Perhaps this was reported in a paper that I have

not seen. The Arata and Zhang 1999 report on 4He, entitled “Anomalous production of gaseous 4He at the inside of “DS-cathode” during D2O-electrolysis” is downloadable from the library at <www.lenr-canr.org>.

My conviction that the accumulation of 4He is the most convincing argument (in favor of the nuclear origin of excess heat) was based, mostly, on experimental results summarized by P. Hagelstein et al. in a 2004. Their report entitled "New Physical Effects in Metal Deuterides," can be downloaded from the library at <www.lenr-canr.org>. The report was written for scientists selected by the DOE to investigate the CMNS field. To my taste, experiments performed by several teams of scientists, from different labs, are more convincing than experiments performed in one laboratory.

Ludwik: (blue is from Jed, green is him quoting me, black is from me now.)
On Feb. 11, 2006, at 3:03 PM, Jed Rothwell wrote:
>Ludwik Kowalski writes:
>> Heat can be produced by slowing down electric charges . . .
>That falls in the "chemical" category. (That is awkward.
>Perhaps I should call it "electron level reaction.")
I was thinking about radiation, such as light or X rays, that can then be absorbed producing heat.

>> . . . by moving magnets or electromagnets
> Mechanical movement changes electrons.
I was thinking about a copper tube being heated when a magnet is falling down inside of it. An identical nonmagnetic cylinder (same shape and size) does not generate heat. unless there is also some mechanical friction.

>>by annihilating antimatter, etc.
>I forgot about that antimatter. It is nuclear; it affect the nucleus. It annihilates the nucleus.
I was thinking about annihilation of an electron and a positron only. No nuclei are involved.

>>Even sound can produce heat in a wall, usually very little.
>Sound is mechanical. It is motion that does not involve changing molecules or
> electron states. (Intense sound can break molecules, but that's another story.)
Yes, sound is a mechanical motion. The energy moves along the direction of propagation but individual molecules oscillate about equilibrium positions, for example, inside a wall.

>>The E=m*c^2 is not limited to nuclear reactions.
> Right. Every energy release, including mechanical energy, involves
> a mass-energy conversion. When you wind up a spring the mass
> increases by a tiny amount, much too small to detect.
Yes again.

>Other forms of energy will probably be invented to explain things to be discovered.
> I was listing sources of energy, not forms. The form of energy released by CF is heat.
> We already know that. Perhaps RF and other forms are also released.
OK, I should have said "other forms of energy convertible to heat"

Jed, I think we now understand each other. I am glad that X shared his personal preference. I did fetch the 2004 review paper he mentioned (Hagelstein et al.). In reading it I see that generation of 4He was presented (to the DOE) as the most convincing argument for the nuclear origin of excess heat. Arata and Zhang experiment on "massive" production of 3He, resulting from 3H, is not mentioned in that paper. Do you know where I can learn what the word "massive" stands for? Does it mean at least one 3H for each MeV of excess heat? I hope so. That would indeed be a very convincing argument. The half-life of 3H is about 12 years. Only a very small fraction of that ash would be turned into 3He during a time interval shorter than one year. I am sure that the authors took this fact under consideration.

As far as 4He is concerned, I find Figure 6 on page 8 (in Hagelstein et al.) to be extremely powerful. It shows that 4He is accumulated at the rate of one atom per 31 +/- 13 MeV of excess heat. The uncertainty of 13 MeV probably corresponds to difficulties mentioned by Mike. The expected value, 23.67 MeV is inside of the error bar. I suspect that

my opinion about "the most convincing argument" was solidified when I read the 2004 review for the first time. A subsequent conversation with Russ George also played an important role. He showed me that the measured relative concentration of 4He exceeded what is normally found in the atmosphere. I am surprised that he decided not to participate in this discussion. What I say, and what you say, is much less important than what the authors of reports say. They know all the details and difficulties; we do not.

P.S. (replying to ZY)
If experimentally observed excess heat has nuclear origin then nuclear ashes of some kind must be generated, in the corresponding amounts. Where are the ashes? This question has been asked as early as 1989, as illustrated in Peter's 1992 report. The 2004 answer to that question can be found in the report to which ZX referred earlier today. As stated above, that answer seems to be "production of 4He -- in some excess heat setups." Is this still a correct answer? Do we already have something more convincing today? That is what I am trying to find out.

P.P.S. (not posted)**:**
I just read the 2004 report (Hagelstein et al.) again. Reality of excess heat, in setups similar to those of Fleischmann and Pond, is discussed first, its origin is discussed in the following section. The title of that section is "Helium and Excess Heat." About 20 references to original reports are given. Here is the ending of that section; it is a comment on the experimentally determined rate of 4He accumulation, approximately one atom per 24 MeV of excess heat.

". . . This value remains the most accurately determined in this field (in the sense that contributions from both the gas stream and the metal are included), but it suffers from the criticisms that the numbers of samples were few, and the largest value of 4He measured was less than 50% of that in air. We note that 4He has been produced numerous times in excess heat experiments at levels above that of the concentration in air. One example is shown in Figure 6. This plot illustrates the real-time correlation between excess heat and the growth of 4He concentration in a metal-sealed, helium leak-tight vessel. ..... Because of the importance of this result, it is discussed further in
Appendix B."

The report shows that accumulation of 4He was viewed (in 2004) as the most convincing argument for the nuclear origin of excess heat. Is there any reason to abandon that position today? I am not aware of any recent experimental results that would be more convincing.

Addendum (2/12/06):
Steven Krivit commented on what I wrote. That was yesterday. This morning I asked for permission to quote him here. In what follows I will show what he wrote (in blue) and comment on it (in black). I will continue replacing some names with X, Y, Z, etc. because I did not ask for permission to show names of those who decided to contribute to the thread.

Ludwik, [X] tried to make a point. I want to emphasize it. Charles wrote a whole book about this: It's called Excess Heat. His point was twofold: (1) Huizenga, Close and Taubes wrote books all about cold fusion, yet they paid no attention to excess heat. (2) As Martin [Fleischmann] says, "excess heat is the principal signature of cold fusion."

If you are pre-disposed to the idea that fusion evidence must conform to what nuclear physicists expect, you will have a long, frustrating and unsatisfying road ahead of you in your earnest quest to become comfortable with the idea of low energy nuclear reactions.

I think that scientists should always try to understand new facts in terms of what is already known. Then they should explore other alternatives. Fleischmann and Pond did try to explain the unknown in terms of known when they hypothesized nuclear origin of excess heat. The accumulation of 4He, in commensurate amount, is a confirmation of their hypothesis.

If you, however, consider that excess heat has been measured by experts, with precision instrumentation, by people who are of sound mind, then you have your answer as to what is the best evidence for cold fusion. . . .

Yes, non-specialists, and many specialists, usually have no choice but to rely on the reputation of recognized experts.

Will this perspective be satisfactory to people who choose to dismiss calorimetry? No. You will note that in Charles' book, a reporter asks Nathan Lewis of Caltech, "Should reporters disregard calorimetry?" Lewis' response was "Absolutely." . . .

How can anybody “dismiss calorimetry?” It is one a recognized branches of physical science. Joule used to discover important facts about natural phenomena. What Lewis responded makes sense only if one replaces the word “calorimetry” with “claims for excess heat based on calorimetric measurements.” Personally, I am not dismissing anything in this thread; I am taking reality of excess heat for granted. I do not know why several people, like Steven, ignored my question (about nuclear origin of excess heat) and replied as if I were questioning reality of excess heat in this thread.

I will take the bold step to say that such a question is no different than the catholic church fathers being asked by the public if they should avoid looking through Galileo's telescope to see the rumored imperfections on the surface of the moon. The telescope was *the* tool for this investigation. Not only that, but only the best telescope, with lenses made by the best, state-of-the art methods was capable of providing an accurate view. Honestly, when should any good scientist indiscriminately discard data?

Lewis' comment was from 1989, by the way. When I attempted to interview him in 2004, he declined, writing, "I've been out of that area for a decade or so," Lewis said. "Consequently, I have no basis for commenting on anything that has happened in that period of time sciencewise." I consider his 2004 response to be quite honest and respectful.

Now consider Karabut's attitude: "The problems of the skeptics are their problems. I measured excess heat, nuclear products and penetration radiation (X-ray emission) with 100 % reproducibility." (Karabut is a specialist in Heat Physics, Nuclear Rocket Engines, and Nuclear Material Science at the LUCH Association at Podolsk, Russia, November 12, 2003).

Karabut's paper had profound effect on me; my renewed interest in CMNS was triggered by his paper. That was more than three years ago. And now I know that many of his findings (in cooperation with Kutcherov and Savvatimova) were known to cold fusion researchers as early as 1992. What puzzles me is that no other cold fusion researchers tried to replicate his experiments independently. Nearly every CMNS phenomenon (see item #13 on this website) was demonstrated with a setup which was clearly described and which was not too difficult to replicate. Findings of Podolsk scientists, however, are not at all mentioned in the 2004 report written for the DOE. Should I assume that this is an indication that findings of Karabut et al. were not confirmed by cold fusion researchers in other laboratories? If they were confirmed then Russian experiments would probably be more convincing. How can a flow calorimeter be wrong when excess heat is generated at the rate of 10 watts? It was shown to be roughly compatible with the rate at nuclear ashes (not 4He) were accumulated.

. . . Consider the words of Italian physicist Antonella De Ninno: "I believe that cold fusion is like the finger pointing the moon, only silly people look at the finger, wise people look at the moon. This phenomenon sheds a new light on the physics of condensed matter and can really be the gate for a new revolution in physics. Don't believe that physics is just the challenge in measuring with higher accuracy what is already known."

Ludwik (2/13/06):
Several people made interesting contributions to this thread today. What follows is my post; it was reaction to what they wrote. In particular, Steven Krivit asked: "... So who really knows all or even part of the origin/mechanism/theory? . . . "

As I wrote to Brian, trying to construct a theory makes things more complicated than necessary. In this thread we address experimental facts, such as generation of excess heat and production of 4He. Are the facts qualitatively consistent with each other? That is our topic. I think they are. They were shown to be consistent, more or less, and that was a giant step toward better understanding of CMNS. In this thread I would like to hear comments on reliability of facts, especially from "old-timers" who made significant contributions. Y already outlined great experimental difficulties. But I viewed this as an explanation of why the Q value, expected to be 23.7 MeV, could not be determined

better than 31 +/- 13 MeV. His comment about the highly abnormal 3He/4He isotopic ratio (factor of 40,000) is also very important.

Is there any reason to believe that unacceptable experimental errors were made by measuring either the amount of excess heat or the amount of 4He produced at the same time? There are about 20 references to relevant publications in section 3 of the 2004 report presented to the DOE. I note that at least one 4He atom per several MeV of excess heat has been reported by more that one team of researchers. That is why I still think that the 4He ash is the best indicator of the nuclear origin of excess heat (in Fleischmann-Pond kind of experiments). Those who disagree probably think that more convincing evidence exists (and has also been confirmed by several researchers). Please share your information. But some might be convinced that no experimental evidence is consistent with the nuclear origin of excess heat. It would be interesting to hear from them as well.

I suggest that a separate thread is devoted to the topic "chemically activated nuclear transmutations." And another thread on "where is an accepted CMNS theory." No progress can be made when we chaotically jump from one topic to another. Yes, Iwamura's findings are extremely important but, as far as I know, they can not be used to argue that excess heat has nuclear origin. And having an accepted theory would be highly desirable and extremely useful.

Jed:
You wrote: "That is why I still think that the 4He ash is the best indicator of the nuclear origin of excess heat (in Fleischmann-Pons kind of experiments)." That depends on how you define "best," and as far as I know there is only one kind of experiment. I do not know what other kind you have in mind. Many methods of producing metal hydrides have been used to demonstrate that Fleischmann-Pons effect: that is, excess heat, helium, tritium, transmutations, etc.

You also wrote "Yes, Iwamura's findings are extremely important but, as far as I know, they can not be used to argue that excess heat has nuclear origin."

Iwamura's present experiments cannot be used to argue this, because he does not try to measure heat. (It is not possible with this apparatus.) However, in his earlier experiments he performed calorimetry and he also looked for host metal transmutations, and he always observed both. Therefore his evidence is as definitive as helium. As far as anyone knows, all cold fusion reactions produce helium, and they all transmute the host metal. I do not think anyone has ever looked for both helium and heat and not found both together; and by the same token I do not think anyone has ever looked for host metal transmutation and heat, and not found both together.

Many people have observed heat without trying to look for helium or transmutations. These tests do not prove anything either way. Neutrons and tritium do not always accompany heat. But as far as I know helium and host metal transmutations always do. Perhaps the ratios vary. I do not think there is enough data to determine this.

Jed:
I wrote: "Iwamura's present experiments cannot be used to argue this, because he does not try to measure heat. (It is not possible with this apparatus.)" I mean that strictly speaking, taken in isolation, these particular experiments do not prove that the heat and heavy-element transmutations have the same origin. As a practical matter, based on common sense and Occam's razor, it is obvious that they do have the same origin. It would be absurd to argue that although metal hydride systems produce a thousand times more heat than any chemical system does with no chemical ash, and they also produce transmutations, these two phenomena are unrelated. They cannot be a coincidence! You might as well argue that daylight and the sun have no causal connection.

If you are curious to know the roots of this debate . . . Ludwik, in the grand tradition of the Cartesian French philosophers, is trying to establish a logical or theoretical connection between helium and/or transmutation and the heat. I am content with Hume's empirical definition of causality, perhaps because I come from a long line of English inventors and statisticians, and to us there is no other kind. Strictly speaking, you can never be certain that you truly understand a logical causal connection, and strictly speaking -- purely from a philosophy-of-science viewpoint -- the only reason we know for sure the sun will come up every day in the East is that it has always done so in the past. (This sounds facetious but it is not.) We assume that Newton's first law ensures that the earth will not spontaneously reverse spin or stop, because nobody has ever seen the first law violated. Repeated observations are all we have to go on.

There is no deeper proof. As Newton himself put it: "I have not as yet been able to discover the reason for these properties of gravity from phenomena, and I do not feign hypotheses. For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction." - Principia Mathematica.

(This is usually translated "I make no hypotheses.") Perhaps string theory or ZPE or something like that will eventually "explains" momentum, but that will only be theory, and theory might always be wrong. Empirical proof is the only kind that exists in reality, and you can never be 100% certain of it any more than you can have a 100% pure sample of iron.

XXX (2/15/06):
Regarding the relationship between excess energy and helium production, polyneutron theory gives various ratios depending on the nuclear fuel.

Consider for example oxygen-18 in the electrolyte as a fuel. Let (18O) represent a nucleus of oxygen-18, and let (An) represent a polyneutron containing A neutrons. Successive polyneutron reactions are:

```
(18O) + (An)  --->  (16O) + ((A+2)n)        (neutron transfer)
(18O) + ((A+2)n) ---> (16O)+((A+4)n)        (neutron transfer)
((A+4)n) ---> (An)+(4He)  (double beta decay + alpha decay).
```

The polyneutron acts as a catalyst for transmutation. Its contributions to excess energy cancel out. The overall reaction is

2(18O) ---> 2(16O)+(4He) + 5.5 MeV

and the energy per helium atom is 5.5 MeV.

Other possible sequences:
Many electrolytes contain lithium. For lithium-7 as the fuel the polyneutron reactions are:

```
(7Li) + (An) ---> (4He) + (1H) + ((A+2)n)
(7Li) + ((A+2)n) ---> (4He) + (1H) + ((A+4)n)
((A+4)n) ---> (An)+(4He).
```

The overall reaction is

2(7Li) ---> 2(1H)+3(4He) + 8.0 MeV

and the energy per helium atom is 2.7 MeV. When lithium-6 is the fuel the polyneutron reactions are:

```
(6Li)+(An)  --->  (4He)+(1H)+((A+1)n)
(6Li)+((A+1)n)  --->  (4He)+(1H)+((A+2)n)
(6Li)+((A+2)n)  --->  (4He)+(1H)+((A+3)n)
(6Li)+((A+3)n)  --->  (4He)+(1H)+((A+4)n)
((A+4)n) ---> (An)+(4He).
```

The overall reaction is

4(6Li) ---> 4(1H)+5(4He) + 15.1 MeV

and the energy per helium atom is 3.0 MeV. Deuterium fuel generates more energy per helium atom.

```
(2H)+(An)  --->  (1H)+((A+1)n)
(2H)+((A+1)n)  --->  (1H)+((A+2)n)
(2H)+((A+2)n)  --->  (1H)+((A+3)n)
(2H)+((A+3)n)  --->  (1H)+((A+4)n)
((A+4)n) ---> (An)+(4He).
```

The overall reaction is

4(2H) ---> 4(1H)+(4He) + 21.0 MeV

and the energy per helium atom is 21.0 MeV. In a light water electrolyte containing lithium (and a trace of deuterium) all of these reactions are expected. The overall release of energy in the electrolyte will probably be in the neighborhood of 4 MeV per helium atom. On the other hand in a system employing heavy water electrolyte and in which deuterium is concentrated in a palladium cathode, reactions in the cathode can lead to 21 MeV per helium atom, comparable with that for deuterium fusion. (Here I have neglected energy contributions from transmutation of the cathode material, which are probably small.)

Ludwik (not posted):
I think that this long thread is over. And I hope that theoretical predictions, as from XXX above, will be discussed in future threads. What are the main experimental CMNS issues?

In my opinion they are:**(a)** Is excess heat real? Some say that experimental errors are responsible for excess heat. Most often that heat is generated at the rate of about 1 W or less. **(b)** Is the rate of generation of excess heat commensurate with the rate of producing nuclear ashes? Some say that these two processes are not commensurate. **(c)** What evidence do we have that a chemical process can trigger a nuclear process? This issue is not limited to nuclear reactions responsible for excess heat. Some say that this is not possible because the energy scales are very different (millions of eV versus several eV). **(d)** How to explain unexpected, but experimentally observed, nuclear processes? No accepted theory emerged from the CMNS field, so far. **(e)** What to do to overcome administrative discrimination of CMNS by our scientific establishment? The establishment consists of (but is not limited to): fund granting agencies, editors of peer-reviewed journals, and administrators of national laboratories. Each of these five issues deserves a separate thread.

Bill Collis (quoting with permission):
I appreciate XXX' skepticism. I appreciate YYY honesty. It's people like these who keep us in the real world. We need dissent. We need constant reexamination of our cherished dogmas and beliefs. We need an open mind. The heresies of the early 90s such as light hydrogen anomalies and transmutations have steadily become acceptable. Who knows what the future will bring? So much for the philosophy. Let's get down to specifics. . . .

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278) Tabletop fusion devices

Ludwik Kowalski; 2/16/2006
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After being away for nearly three months I went to a meeting at the university. That was three days ago. Three people who I met independently commented on new tabletop experiments. They knew about my interest in cold fusion and they thought that new reports were confirmations of reality of cold fusion. Each time I explained that these are new "hot fusion" devices. The next day a message of the same kind (see below) was posted on the restricted Internet discussion list for CMNS researchers. The CMNS, by the way, stands for Condensed Matter Nuclear Science. It is a new name for what used to be called cold fusion. Personally I identify three CMNS subfields: cold fusion (CF), cold transmutations (CT) and excess energy (EE). Contrary to what many think, research in these areas did not end after cold fusion was declared to be pseudoscience (more than 15 years ago).

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1) It started when X1, who is an amateur scientists, quoted (see below) what was posted recently (2/13/06) at:

<www.scienceblog.com/cms/ny_team_confirms_ucla_tabletop_fusion_10017.html>

Researchers at Rensselaer Polytechnic Institute have developed a tabletop accelerator that produces nuclear fusion at room temperature, providing confirmation of an earlier experiment conducted at the University of California, Los Angeles (UCLA), while offering substantial improvements over the original design. The device, which uses two opposing crystals to generate a powerful electric field, could potentially lead to a portable, battery-operated neutron generator for a variety of applications, from non-destructive testing to detecting explosives and scanning luggage at airports. The new results are described in the Feb. 10 issue of Physical Review Letters.

"Our study shows that 'crystal fusion' is a mature technology with considerable commercial potential," says Yaron Danon, associate professor of mechanical, aerospace, and nuclear engineering at Rensselaer. "This new device is simpler and less expensive than the previous version, and it has the potential to produce even more neutrons."

The device is essentially a tabletop particle accelerator. At its heart are two opposing "pyroelectric" crystals that create a strong electric field when heated or cooled. The device is filled with deuterium gas -- a more massive cousin of hydrogen with an extra neutron in its nucleus. The electric field rips electrons from the gas, creating deuterium ions and accelerating them into a deuterium target on one of the crystals. When the particles smash into the target, neutrons are emitted, which is the telltale sign that nuclear fusion has occurred, according to Danon.

A research team led by Seth Putterman, professor of physics at UCLA, reported on a similar apparatus in 2005, but two important features distinguish the new device: "Our device uses two crystals instead of one, which doubles the acceleration potential," says Jeffrey Geuther, a graduate student in nuclear engineering at Rensselaer and lead author of the paper. "And our setup does not require cooling the crystals to cryogenic temperatures -- an important step that reduces both the complexity and the cost of the equipment."

The new study also verified the fundamental physics behind the original experiment. This suggests that pyroelectric crystals are in fact a viable means of producing nuclear fusion, and that commercial applications may be closer than originally thought, according to Danon. "Nuclear fusion has been explored as a potential source of power, but we are not looking at this as an energy source right now," Danon says. Rather, the most immediate application may come in the form of a battery-operated, portable neutron generator. Such a device could be used to detect explosives or to scan luggage at airports, and it could also be an important tool for a wide range of laboratory experiments.

The concept could also lead to a portable x-ray generator, according to Danon. "There is already a commercial portable pyroelectric x-ray product available, but it does not produce enough energy to provide the 50,000 electron volts needed for medical imaging," he says. "Our device is capable of producing about 200,000 electron volts, which could meet these requirements and could also be enough to penetrate several millimeters of steel." In the more distant future, Danon envisions a number of other medical applications of pyroelectric crystals, including a wearable device that could provide safe, continuous cancer treatment.
Dan Kawasaki
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2) To which X2 replied: "This is conventional thermonuclear fusion. So, yes, we can again be happy that the hot fusion people are confronted with tabletop alternatives to their otherwise mega projects."

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3) X3 commented: "Actually this is not thermonuclear fusion because the system is not in thermal equilibrium."

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4) A veteran CMNS researcher, X4, made a very interesting observation: "Actually, these folk (and Puttermann at UCLA and Taleyarkhan? at Purdue) need to adhere to the same standards the high temperature folk set for us: measure the yield of both neutrons and tritium (and demonstrate that 4He is sparingly seen) before they claim a hot fusion result. When they do, they might be surprised at the results."

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5) Ludwik Kowalski:
X4 made an interesting observation. Perhaps the authors of the papers know that mentioning a possibility of a CMNS mechanism would be detrimental. Would a paper mentioning cold fusion be sent to objective referees by the Physical Review Letters? Probably not.

Two kinds of standards are worth emphasizing. Why do newspapers describe some discoveries as interesting and scientific while other discoveries are totally ignored? Production of 4He, associated with generation of excess energy, is not less interesting as tiny crystal-based accelerators of deuterium ions. The same for the discoveries of Iwamura. The answer is clear, the newspapers' editors discriminate against the CMNS topics because they have been convinced that research in that area belongs to pseudo-science. The editors of mainstream journals do not even send CMNS reports to the impartial referees. This created a vicious circle. The claimed CMNS discoveries are criticized on the basis of not being confirmed by other scientists while, at the same time, other scientists are prevented from seeing CMNS papers.

Illustrations of unreasonable rejections of CMNS papers are described in items #33, #88, and #153 at my website at:

http://blake.montclair.edu/~kowalskil/cf/

Please send me other illustrations and I would be happy to append item. Exposing unfair discrimination should be an important part of our public relation activities. What else can we do to break the vicious circle? Most people are fair and they want others to be fair. That is why I am optimistic; fairness will prevail.

X5 wrote:

This is sad, profound and hilarious -You all know how initially cold fusion got off to a bad start because people thought it was a "colder" form of thermonuclear fusion? And how the name cold fusion stuck, regardless of the mistaken identity? Well, judging by the popular culture out there -- believe it or not -- hold onto your seats -- history is repeating itself again, this time in favor of cold fusion. The recent attention about the pyro and bubble fusion is being called "cold fusion" by many people. I think we all know that it's not, but consider the spill-over positive attention the field -and the name -- is getting! I realize the *real* cold fusion is not getting this attention, but that makes no difference. The NAME cold fusion is losing its stigma through this process. Thank you, Seth Putterman and UCLA !"

6) X3 again: "I remember when Taleyarkhan et al. did compare tritium and neutron yields in 2002 paper, the neutron yield was lower, which caused a controversy at Oak Ridge. The discussion was all about the efficiency of the neutron detector. Do you think there's more to it? I thought we had settled that Pyro and Bubble Fusion is definitely hot. But your point is quite good and I'm glad I hear it."
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7) X4 again: "It is just a hunch, and I am being mischievous. But the so-called "heat" from hot fusion is actually measured from neutron flux (and then back-calculated). They never saw any heat or tritium. And the neutron could come from lots of sources other than d-d or d-t (i.e. wall interactions or semi-coherent fusion in the so called plasma instabilities - which are actually stabilities). I guess my point is the same requirements and standards of proof are not held for the hot and cold operating regimes. But I do not believe it has been definitely shown that Pyro and Bubble fusion are hot. It would not upset my world view if they were and were proven to be hot fusion. My hunch is they are not."

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279) The iesiUSA company seems to be in trouble. Will it hurt the reputation of CMNS researchers?

Ludwik Kowalski; 2/2/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

In unit #274 I wrote that even a single case of fraud associated with CMNS might "deliver a devastating blow to the entire discipline. The consequences of such episode would be difficult to repair." When this was written I was not aware how bad the iESiUSA business situation was. What follows was posted by Steven Krivit at the New Energy Times website:

http://newenergytimes.com/SR/IESI.htm

Steven is a journalist who has been investigating many aspects of CMNS field for at least four years.

New Energy Times Editorial Commentary (Feb. 1, 2006)

I've been investigating iESi since June 4, 2005. Most of the references on the "DIY" blog listed below appear to agree with my findings, though I have not yet looked at every reference in detail.

Since the start of my investigation, I have been, at various times, in direct communication with many iESi personnel including; Patrick Cochrane (CEO), Tom Bugg (President), Norman Arrison (Staff Scientist), Mark Boocock (U.K. Managing Director), Hywel Rees (U.K. Sales & Partnerships Director), Terry Dingwall (ex-President), Stephen Monaco (ex-Vice President), Hyunik Yang (ex-Senior Scientist), Nahm Cho (ex-Senior Scientist), Bill Harrington (ex-Staff Scientist), Alaine Liberty (Technician), and Ken Pierce (Technician).

I have seen several court documents pertaining to pending actions from the company against ex-employees, and also actions from the ex-employees, who are shareholders, and who are also acting on behalf of many of the other shareholders.

The "company," in this regard, is the board of directors; namely, Patrick Cochrane, Ronald Foster, Fred Dornan and Tom Bugg. They, and not the shareholders at large, appear to be fully responsible for the state of the company and the alleged misdeeds.

The former senior scientist, Hyunik Yang, told me last fall that he had been hastily appointed to the Board sometime around August. According to Yang, he subsequently resigned from the Board a month or two later, after starting to see the management problems. On Dec. 1, he informed me that he was no longer working with iESi.

The story behind iESi is certainly a fascinating one. It may not please many people. Certainly, the several hundred private investors who cumulatively contributed a few million dollars to the company may not be pleased to finally start learning about the troubles with the company. Some of the good-hearted employees of the company, like Norm Arrison, may be shocked and disappointed to hear about the mess the company is really in and some of the alleged

wrongdoings on the part of the remaining directors.

There is no certainty at the moment as to what will become of iESi, or its licensing claim to any intellectual property potentially conveyed by Hyunik Yang. I am told by several sources that there are 20 separate agreements between Yang and Cochrane, and Yang and iESi that govern the licensing of the intellectual property. I am also told that several of these agreements contradict each other. These matters are now in the hands of half a dozen attorneys on behalf of Yang, and probably as many on behalf of the iESi board.

Will the true owners of iESi, the hundreds of shareholders, ever see the development of the intellectual property and see a return on their investment? It's hard to say at the moment.

The iESi story is about wildcatters; bold businessmen willing to take high-risks, and like sharks smelling blood in the water, swarming to what they think is a vein of gold in an apparent cold fusion gold rush.

Is it fool's gold? I'm still undecided. The underlying science reported by Yang and Vysotskii at ICCF12 has multiple inexplicable characteristics. But they've been highly-secretive and have refused to reveal any hard data.

Has the iESi leadership contributed toward the progress and the development of this technology? For a while, at least, they most certainly did. Can they be credited for their willingness to take a risk on a new source of clean energy? Absolutely. Did they bring research funds into the field and promote progress? Yes, they can be given that credit as well.

However, does the current board of directors have what it takes to salvage this company? That is a question every shareholder should be asking right now. Unfortunately, it's too late to get your money back. That offer, made by the iESi board in the Dec. 8, 2005 shareholder's update, expired two hours ago, on Jan. 31, 2006, which coincidentally, was the same time my embargo agreement expired with iESi as well.

Is the leadership at iESi typical of businesspeople who are investing in cold fusion? Fortunately, no, but this is not the first, nor will it be the last of companies who are willing to make possible indiscretions on the way to hopeful cold fusion profits.

I am pleased to say that there are, in fact, several companies in the U.S., and some in Europe that are making careful, conscientious efforts to develop cold fusion. However, they are remaining private, and staying under the radar for now. They are not putting themselves in the predicament of taking money from public investors. This seems wise to me. I have little uncertainty that eventually cold fusion will be commercially viable, but which company, or companies will figure out the answers? That's anybody's guess.

As much as I'm a proponent of cold fusion, I would say that this is not a time to invest your life savings or your IRA in cold fusion. This is a time for angel investors who can afford to take risks and who want to possibly contribute to a great alternative to fossil fuels.

-Steven B. Krivit
Do-It-Yourself Due Diligence Blog (Feb. 1, 2006) New Energy Times Photos of iESi "Corporate Offices" (Feb. 1, 2006)

On Jan. 22, 2006 a New Energy Times (tm) reader informed us that the www.iesiusa.com Web site went dark.

"The website appears to be password protected. I checked and it appears that the password message that used to appear only for the private investor section now inhibits access to the entire site. Do you know what is going on?"

New Energy Times started investigating Innovative Energy Solutions Inc. (iESi) in June, 2005. At that time we signed both a nondisclosure agreement as well an embargo agreement. The embargo agreement expires in February and we will therefore be able to report our findings in our March 10 newsletter. We concluded the major part of our iESi investigation in November, 2005. New Energy Times does not have an ongoing interest in iESi at this time.

New Energy Times is continuing its interest and investigation of Hyunik Yang and the work he has reported at ICCF12.

As a courtesy to interested parties, we captured several of the key pages from the iESi Web site and we present them here, along with photos we took of their "corporate headquarters."

Appended on 11/21/06

I think that the following piece of information is worth appending. It was posted by New Energy Times journalist Steven Krivit on a restricted Internet discussion list for CMNS researchers in response to a prominent list member who had chided him to "Loosen up!" in response to Steven's vigilance of another "cold fusion" company. It a good reminder that waters surrounding scientists are infested by unscrupulous sharks. Steven has an in-depth investigation at this Web location:

http://newenergytimes.com/SR/IESI.htm

Many people were harmed from the actions of a few with regard to iESiusa. Millions of dollars from innocent, though perhaps somewhat eager and somewhat naive private investors were apparently lost. The reputations of some good people were harmed. One of the conscientious employees formerly involved with iESiusa spent what would have amounted to a year's tuition for his children's college fund to extricate himself from a meritless suit by the iESi executives. But for this fellow, he didn't know the facts until it was too late. He had encouraged the iESiusa board to operate within the law and when they refused, he blew the whistle on them to federal investigators. Even though he was protected under the U.S. whistleblower act, this action still cost him thousands of dollars in legal defense and much of his personal time.

The immediate collateral damage is that the CMNS community lost a potentially good ally. There is more collateral damage. It may have discouraged other potential allies from the business community to set foot into the CMNS community. I do not know the current status of iESiusa at the moment, but it appears that it is nothing but a skeleton. Their former Web presence http://iesiusa.com/ has now disappeared without a forwarding link. I learned that they have a new Web site at http://www.iesica.com/. Cochrane appears to be involved in several outstanding related legal challenges, Yang told me today that he is still fighting his legal battles with iESiusa [Cochrane and other board members]. I know of no scientists who remain with iESiusa.

Terry Dingwall, the former iESiusa President initiated a legal action against the corporation on behalf of the bulk of the shareholders in the fall of 2005 to protect the interests of the shareholders. According to Dingwall, the company that was formerly based in Nevada, USA, now exists as an empty shell, and at least some of the iESiusa board members have now started a new company. An Alberta Registrar of Corporations document shows Patrick Cochrane and Ronald Foster as directors of a company called IESI Energy Solutions (Alberta) ulc.

The web site says Arrison and one other fellow are part of their "team." I spoke with Arrison last week - he tells me me left the company almost a year ago. What about the other fellow still listed on the iESiusa site, Dumitru Fetcu? The one whose name iESiusa couldn't even spell correctly? This morning I obtained the following information from a European colleague, "Fetcu is no longer working with iESiusa. He had sent them a termination letter on June 30 this year because they had not paid him according to the contract."

When companies make claims such as “has amassed the best collection of cold fusion scientists in the world," they should back that up with fact. If they cannot, the public should know. When the owners of companies engage in shady business practices, the public has a right to know. And it is the moral and ethical obligation of the press to be the informant, if necessary, of bad news. Fraudulent and deceptive business practices hurt everybody; investors, scientists

and eventually even those who are guilty of such indiscretions."

It is clear to me that Steven is a very gifted journalist and a dedicated defender of general public. In fact, suspecting something inappropriate, he is now investigating another company. I do not think that this will turn out to be anything comparable with the iESi episode. Another way in which his online journal, New Energy, serves general public is publishing summaries of laboratory results. A good example of this is his report #7 at:

http://newenergytimes.com/news/2006/NET19.htm

My own unit #314, to be posted soon at my website, was prompted on that report. What a coincidence, just now, as I was about ready to append this piece, another message from Steven Krivit was posted on our restricted CMNS list. He was responding to someone who referred to unpleasant aspects of critical investigations and to "personal factors." Part of his reply is shown below. I hope Steven will again give me permission to be quoted. He wrote:

". . . I will continue to investigate and report as I see best, on behalf of the public's interest. What I report will not always be pleasant, but that is the way of the world and it is my choice to keep this community, and the world at large, informed of all matters pertaining to CMNS. The field is not the same as it was two years ago. Money is coming in. A journalist (Bruce Gellerman) recently got an award for covering the field -- the first in over a decade. New branches of government(s) are becoming interested (again.) National security concerns are being triggered. Secret meetings are being held. We are in for changing times, and to borrow a term from electrochemistry, a fair amount of disequilibrium. . . ."

Appended on 11/27/06

The paragraph below is from a paper that Robert W. Bass publishd in the Infinite Energy Magazine (Issue 67, May/June 2006).

http://www.infinite-energy.com/iemagazine/issue67/apsmeeting.html

The title of the paper, dated March 16, 2006) was: "An Afternoon to Remember: Cold Fusion Session of APS Meeting."

Investigative reporter Steve Krivit has been diligently exploring every plausible development in the entire CF field, and surprised me by having persuaded Hyunik Yang and his former Russian collaborator Vysotskii to disclose at ICCF12 some of the formerly closely-held information regarding Innovative Energy Solutions Inc., where in June 2005 Fleischmann, McKubre, Hagelstein, Beaudette, Krivit, and myself were privileged to witness what purported to be the first large-scale (20 kilowatts!) CF power demonstration. (Steve has a continuing interest in following developments at www.iesiusa.com, which, sad to say, has lately become distracted by intellectual property ownership issues and from which their former chief scientific manager Yang has now departed.) However, Steve refrained from mentioning iESi in his well-illustrated current-activity-survey slideshow, which I hope he makes available on his website cited above.

I would be very much interested in what other witnesses the "first large-scale (20 kilowatts!) CF power demonstration" think about the iESiusa episode today (especially Peter Hagelstein, from whom I first heard about iESiusa, and Martin Fleischmann). More specifically, it would be interesting to know at what stage they become suspicious that someone might be manipulating them, and other scientists, to impress naive investors. The issue is not simple. Business people are not scientists and many of them think that risky stock market operations are accepted parts of healthy economy. They are willing to lean on claims made by scientists; their only concern is to avoid doing things that are not legal. Scientists, on the other hand, often have good reasons for taking some claims seriously. Working in a field that has been discriminated against, they are willing to accept any promise of financial support for research.

Appended on 11/29/07

The original 2004 claim, recovering heat which would otherwise be wasted, was not unreasonable, as illustrated below. This piece was published at: http://entrepreneur.com/tradejournals/article/119368190.html

INNOVATIVE ENERGY FORMED TO DELIVER CLEAN ENERGY.
Worldwide Energy • August 1, 2004 •

Dr. Hyunik Yang, Dr. Dumitru Fetcu, Patrick J. Cochrane and other industry experts have launched a new venture to deliver innovative and clean energy solutions. The new , Innovative Energy Solutions Incorporated (iESi), has developed unparalleled patented technologies in the areas of hydrogen generation, low-cost heat generation and energy conservation. The company's first available solution, iESi Heat Pipe, is a unique patented heat delivery system enabling end users to recycle waste heat and dramatically reduce energy consumption.

iESi is organized into three broad divisions to maximize return on its intellectual capital and intellectual property. These divisions include hydrogen generation, heat generation and waste heat recovery.

In order to remain competitive in today's global market, businesses are forced to address the uncertainly of rising and volatile energy costs as well as concerns regarding the dependence on oil from the Middle East. Government controls have also added additional regulations and at times impeded exploration and the development of new energy sources. iESi solutions offer a viable alternative to traditional energy sources.

"The application of our patented technologies allows our customers to go from dependence on energy providers to energy self sufficiency," said Terry Dingwall, president, iESi. "Dr. Yang and our team are continually conducting research to enhance iESi's offerings. We plan to unveil revolutionary new technologies in the third quarter of 2004."

iESi's Heat Pipe product has established a reputation as a practical technology to utilize waste energy. The product is an innovative solution that recovers heat energy from a variety of industrial and commercial processes. Waste heat that is normally dissipated into the atmosphere can now be recovered and reused, enabling end users to reduce overall energy costs as well as greenhouse emissions. The patented technology of the Home Heat Exchanger saves the average consumer about 20-30 percent of their total heating bill and can easily be installed by homeowners on their fireplaces, gas stoves or furnaces.

As concern grows for the world's natural resources, energy efficiency is gaining worldwide attention. iESi is poised to be the leader in the development of innovative energy solutions through its safe and patented plasma processes which includes low cost hydrogen generation and low cost heat generation.

Dr. Yang holds a PhD in the field of mechanical engineering from Columbia University in New York. Dr. Yang has spent the last 17 years researching quantum energy, in addition to his responsibilities as an associate professor at Hanyang University in South Korea. Dr. Yang has been included in the Who's Who of Science and Engineering since 1998.

About iESi

Innovative Energy Solutions Inc. (iESi) is a leader in developing next generation clean energy technologies. iESi owns several patents related to heat pipe systems and hydrogen generation technology. Headquartered in Las Vegas, iESi also has offices in Canada, Europe and South Korea.

For more information, visit http://www.iesiusa.com or call. 913/707-2776.

Appebded on 4/17/08

This was fetched by Google today.
Also Dated as January 27, 2007. Here is a quote from this webpage:

. . . . Dr. Philipp Kanarev, chairman of Kuban State Agrarian University's department of theoretical mechanics, Krasnodar, Russia, developed a method of water plasma electrolysis that he sees as the best way to get cheap hydrogen from water. He tells why his 1987 report on it didn't reach news media nor the open literature about patents. Since his device was developed "at the enterprise of the military industrial complex", his certificate of invention was stamped "for service use only" and its content was not published openly.

At that time, the focus was on purifying and disinfecting water with the help of the plasma in his reactor. Two years later Drs. Stanley Pons and Martin Fleischmann announced they had excess energy output during a type of electrolysis. This renewed research efforts behind the former Iron Curtain as well as in the West. In 1996 one of Kanarev's co-authors on the 1987 certificate published results on the excess energy output from the plasma process. The next year they applied for a patent. Then a full group of Russian scientists tested the device and documented its output. Technical people can read more about Kanarev's theory in his books, such as Crisis of Theoretical Physics. Results of the plasma-electrolytic experiments were predicted by the theory.

The bottom line for us non-techies is that hydrogen can compete with fossil fuels as an energy carrier. Previously, existing methods of getting hydrogen from water required more power than is produced when hydrogen is burned. Kanarev says it is possible to get ten times more energy output than input, and he calculates that ideally more than 1,500 ampere-hours of electrical energy could be produced from each litre of water. Engineering the lab models into industrial models has been delayed by lack of money, he said this year. He was apparently too wrapped up in his research to attend this year's Berlin conference, but sent a paper.

Hal Fox of Utah has interviewed scientists in the former Soviet Union. He points out that the high-density charge cluster (HDCC) phenomenon first discovered by Ken Shoulders was also independently found by several others. When the process is refined into a reliable product, HDCCs may be used to harness 30 times more energy than is pulsed into the devices. Fox said his Emerging Energy Marketing Firm is promised enough funding to develop such technologies. They can be used to transmute both liquid and solid high-level radioactive wastes into safer substances. Shoulders received the US patent for HDCC methods, and later Alexander Ilyanok in Belarus and Russian scientists Mesyats and Baraboshkin also discovered charge clusters.

At the Berlin meeting Fox reported another patented Soviet-based invention. A.I. Koldomasov's device piezo-electrically vibrates a mix of waters through a special dielectric material to produce heat energy in more abundance than the energy which powers the oscillator. The device is reported to put out 40 kilowatts of heat energy with only two kilowatts of electrical input. Last year Dr. Josef Gruber described a visit to the research institute where Koldomasov is managing engineer. Koldomasov discovered the new energy source while observing cavitation – implosions in water such as found in "water hammer" in pipes. Gruber showed a photograph of the small device, filled with pure water mixed with only one per cent deuterium (heavy water). Although there are no spark plugs or similar equipment, electrical discharge could be seen. Energy comes out in the form of both heat and electrical current. "Depending on kind and location of the magnets, DC or AC (electricity) may be observed." Gruber said testing revealed 2000 per cent excess energy.

Don't more-output-than-input machines violate a law of physics? A few speakers said that "law" is invalid if a machine releases trapped potential energy from nature in previously-unrecognized ways. They cited zero-point fluctuations of the vacuum of space, vacuum spin or aether as the source of excess energy. The word "aether" is loaded with baggage from the nineteenth century, when aether was thought to be a static substance filling all space, just sitting there. In contrast, a new understanding is of a non-material dynamic primary background out of which the material world is created. This aether is seen as being incredibly dense with energy in constant motion.

Dr. Harold Aspden from England (www.aspden.org) is both a theoretical physicist and practical. In 1972 he was 30 years ahead of his time with his book Modern Aether Science. As patent director of IBM Europe and then a professor of electrical engineering, he could have taken the easy route of accepting consensus science. But he instead came up with a physical theory that applies to new energy technologies including magnetic motors and plasma discharges. . . . .

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280) Another rejection

Ludwik Kowalski; 2/17/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

lllustrations of rejections of cold fusion papers and notes by editors of mainstream journals can be found in items #33, #88 and #233. Below is another illustration. In typing this new item I am thinking about the Soviet Union where research in genetics was considered to be pseudoscientific. Enormous administrative pressures were created to make sure that minds of people are not poisoned by degenerated Ph.D. scientists and academicians. The system was very effective.

The editors of our mainstream papers are certainly not subjected to the same kind of ideological pressure. But, somehow, they also act in unison when it come to rejections of information about research in the "forbidden" CMNS field. The CMNS, by the way, stands for Condensed Matter Nuclear Science, a better (but not perfect) name for what used to be called "cold fusion." What is the mechanism behind the unison action of editors and other administrators of "mainstream science?" They seem to follow some kind of a "party line." And this happens in many countries.

The social phenomena of organized discrimination of large groups of highly qualified researchers are probably worth studying. I do understand the mechanism of discrimination of geneticists in the Soviet Union but the mechanism of discrimination in the USA is not at all clear to me. Something is not right somewhere. What is it? The text below was e-mailed to the editor of a magazine on January 29, 2006.

To: Yale Scientific Magazine
P.O. Box 209117, Yale Station
New Haven, CT 06520
ysm@yale.edu

From: Ludwik Kowalski
341 Brook Avenue
Passaic, NJ 07055

Date: January 29, 2006

Dear editor: Please consider the following note for inclusion in your magazine.

Still proto-science -- but not pseudo-science.
And not a fraud.

By Ludwik Kowalski

About 40 years ago, as a postdoctoral researcher at Columbia University, I often participated in nuclear science experiments at Yale University. In 1968 my note entitled "The Use of Mica in Understanding Nuclear Reactions" was

published in Yale Scientific. I am now a retired professor emeritus, Montclair State University, NJ. The purpose of writing this short note is to make the readers aware of how I reacted to a Washington Post article by a Yale University historian, Dr. Bettyann Kevles. Although I found the article interesting I strongly disagree with what she wrote about "cold fusion" (CF), as explained at:

http://blake.montclair.edu/~kowalskil/cf/254fraud.html

Like most scientists of my generation, I was highly excited when the CF discovery was announced 17 years ago. And like most of them, I accepted the "official" Department of Energy position that CF is pseudo-science. But about four years ago I accidentally discovered that the field is still active. I started reading recent publications <www.lenr-canr.org>, participated in four CF experiments and attended three International CF conferences (ICCF10, ICCF11 and ICCF12). The evolution of my ideas about CF can be seen at the above website. I am optimistic that something good will emerge out of CF research. Unfortunately, instead of being encouraged and rewarded, honest researchers in the area of CF are often viewed as pseudo scientists. Dr. Kevles even compared the announcement of CF with the well-known fraudulent "Piltdown Man" episode.

Be aware of these facts, remain open-minded and protect honest scientists, even those who explore controversial claims. Researchers should be protected from those who discriminate against them, by denying access to peer reviewed journals, etc., and from fraudulent commercial manipulators. In my opinion CF will become science when at least one of its effects is "officially" recognized as truly reproducible. My personal attempts to contribute to such a transformation are described in items #267 and #270, at the above website. For the time being let good people benefit from your doubt.

This note was submitted about three weeks ago. Today, assuming that my message was lost, or forgotten, I resubmitted it. I wrote: "Dear editor: I am sending the attached file again. It was first e-mailed on 1/29/2006 but I did not hear from you. Assuming my message was lost I am sending the file again. Please confirm that the file was received. I want to know if my note is going to be published. Ludwik Kowalski Professor Emeritus, Montclair State University, Montclair, NJ, 07055"

Will the editor reply or will this professor emeritus be ignored again? This question will be answered here in about ten days, if you come back. Please describe your experience in dealing with administrators of mainstream science, editors, government officials, laboratory directors, fund granting agencies, etc. I will be happy to "advertise" your stories here. What can be done to eliminate a vicious circle: The claimed CMNS discoveries are criticized on the basis of not being confirmed by other scientists while, at the same time, other scientists are prevented from seeing CMNS papers.

To be appended; probaly in about ten days:

. . . . . .

No reply so far, 10/29/06

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281) Reporting negative results

Ludwik Kowalski; 2/23/2006
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Thanks to Tom, Scott and Dennis for permissions to quote from private messages. Yesterday **Tom Benson wrote**: "Hello Ludwik and Scott, How are you doing? I've just finished reading your (Ludwik's) unit # 267 in which your non-confirmation of the electrolytic experiments is shown. I'd like to thank you personally for publishing a good solid NEGATIVE report...CF people need to double-check each other's work more frequently and point out problems. ....."

Replying to the above, **Scott Little wrote**: "Thanks Tom. Both Ludwik and I agree that there is indeed value in publishing negative results. I just wish we had the opportunity to publish some positive results once in a while. .....To date, I have never really seen (i.e. confirmed) the excess heat effect in the Earthtech labs. Oh, there have been times when something positive-going occurred in our experiments but it was either an artifact that we subsequently identified or something very small near the detection limit that was never adequately explained and could not be repeated.

Over the years we have gradually improved our calorimetry to the point where we can now achieve +/- 0.1% relative accuracy with our latest instrument. We have run a small variety of CF experiments in this new calorimeter and none of them have shown even 0.25% excess heat. It is a great frustration for me to see the CF community claiming over and over again that there can be no doubt as to the validity of CF, yet I can't find/make an experiment that shows the basic effect.

But I have not tried Dash's recipe, nor the Szpak approach. We've worked a lot with Dennis Letts who takes the standard F&P cell and excites it with laser light. So far that has not shown excess heat in our new calorimeter. . . . Please keep our calorimeter in mind. It is currently available for work and we offer to test any viable CF cell in it for free."

The issue of reporting negative results was raised in my ICCF12 poster, as shown in the unit #270. The only person who commented on the issue was Ed Storms. He wrote "I think publishing negative results is a waste of time unless these results reveal a useful pattern." I disagree. If a positive result is an interesting fact, such as generation of excess energy, then the failure to confirm such result is also important. What is a "pattern?" It is something about how things are arranged. Right? An experiment describing a discovery and another, apparently identical, experiment confirming it, form a useful pattern to be aware of. The same should be true for a pattern consisting of a pair of reliable experiments contradicting each other. Withholding one kind of results creates an illusion of success; it is not scientific. I am certain that Ed will agree with this. The only facts not worth sharing are those which are suspected to be unreliable, for example, after discovering that an instrument became faulty, that a solution was contaminated, or something else of that kind.

Another issue on which I have mixed feeling, as far as Ed Storms is concerned, is the concept of NAE (Nuclear Active Environment) that he invented. It is something that remains to be identified. The rate of progress toward control and understanding of CMNS phenomena would certainly accelerate if the main cause of such phenomena were known. But what evidence do we have that all CMNS phenomena have the same cause? And how can we be certain that the cause is "nuclear?" How do we know, for example, that it is not electromagnetic or gravitational? Yes physicists did identify

three kinds of interactions (gravitational, electromagnetic and nuclear). But why should this be the end of the story? Perhaps the CMNS phenomena are due to something that remains to be discovered. But that is a separate issue.

Replying to Tom I wrote:
" If you are reading item #267 then go to item #270 as well. But this is not the end of the story. The experiment described in #270 is going to be repeated. The chance of success is higher this time. Pierre Clauzon, and other people at the ICCF12 (including Mizuno himself), identified something that was not done correctly. Our protocol did not allow for sufficient loading of Pd with hydrogen. As you know, a Mizuno-type experiment can be relatively simple and relatively inexpensive. I hope it could be used to produce excess heat on demand. That is why we are anticipating another attempt to replicate the French experiment.

In replying to Scott, I wrote:
I am glad that your highly sensitive, and very accurate, calorimeter is now available to researchers. But it does not seem to be appropriate for a Mizuno-type cell in which excess heat is measured by the amount of evaporated water. But the instrument is probably highly appropriate for measuring excess heat resulting from diffusion of deuterium through palladium. Several CMNS researchers reported on reality of such heat. In one case a Pd tube (with thin walls) was filled with the gas at the initial pressure of several atmospheres. Did you try to experiment with such setup? I did write to Dennis Letts and he provided additional details about what you mentioned.

As you know, Dennis was successful in triggering excess heat with a laser beam. This was reported at ICCF10. But the effect was observed only with palladium that he originally had. Last summer he told me that attempts to reproduce the effect failed with other stocks of that metal. So I asked Dennis what the present situation is. He wrote: "In November 2005 the effect returned in my lab with experiment 641--the laser triggered about a 1 degree cell temp increase with ambient temperature controlled to within +/- .05C and cell input power controlled to within +/- 10 mW on a 10 Watt input power. I started experiment 642 Jan 10, 2006 in MOAC [Scott's calorimeter] and so far the laser has had no effect on cell temperature beyond laser heating of about .1 degree C. "

Should such mixture of positive and negative results be known to other researchers? I thing so. Reporting about the 641 experiment and not saying anything about the 642 experiment would be highly inappropriate. In this case the new origin of Pd is the 'meaningful pattern.' But suppose the negative result is obtained in an experiment in which everything seems to be the same as before. Why should negative results not be reported when "meaningful patterns" cannot be identified? Nothing is strictly identical in nature. Unfortunately, we still do not know which differences are significant and which are not.

Anticipating a possibility of success, in the next set of Colorado experiments, I would like to know who would be interested in building similar devices in order to either confirm or refute reality of large amounts of "excess heat on demand." Once we have reliable Mizuno-type cells we can start demonstrating them at local high schools and universities. Experiments performed at scientific conferences, for example, during poster sessions, can also be very helpful. Concentrated effort of many might turn out to be very productive at this stage. Please write to me, if you are interested. Even 80% reliability would be a giant step forward toward the reconciliation with mainstream science.

I am glad that Dennis gave me a blank permission to quote. Here is how he responded when I asked for the permission yesterday. "You may use any material that pertains to me in any way you wish. I only require that you tell the truth as you see it. I think it is wrong for me to tell others what to print or withhold; I don't believe that represents academic freedom, freedom of the press , or my idea of freedom in general." Let us hope that Scott's calorimeter, called MAOC, will confirm reality of excess heat in Dennis' experiments. MAOC, by the way, stands for Mother Of All Calorimeters. It took Scott several years to build and test this superb instrument. I am honored to communicate with people like Dennis and Scott. How do some physicists dare to say that CMNS researchers are voodoo scientists. Failure to reproduce experiments does not turn a researcher into a pseudoscientist. But what should one think about an author who writes about something without making an effort to study it? The answer is obvious; such an author does not act as a scientist. Yes, I am thinking about Dr. Robert Park, the author of the Voodoo Science book. His crusade aganst real pseudoscience is highly valuable. But labeling cold fusion as voodoo science is not. Why didn't he come to recent ICCFXX conferences to learn about what is going on? Why are his arguments based on what was known in 1989 and not on what is known today.

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282) Velikovsky's controversy

Ludwik Kowalski; 2/25/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

The CMNS controversy is certainly not the first and not the last in physical sciences. In 1950 Immanuel Velikovsky hypothesized that Venus became a planet about 5000 years ago. This happened when big chunks of matter, presumably ejected from Jupiter, were progressively captured into a nearly circular orbit. As far as I know, the flame of conflict created by the publication of Vilekowsky's books extinguished itself, little by little. I hope that the CMNS controversy will not end in the same way. In 2005 a large archive of Velikovsky's papers and correspondence was donated to Princeton University Library. Will physical evidence supporting his hypothesis be found by future generations of scientists? I do not think so. And I hope that the CMNS controversy will not end in the same way in which the Venus controversy ended. But our fascination with CMNS topics might also become a forgotten episode. The only way to avoid this would be to find at least one truly reproducible effect in the next ten or twenty years. Most currently active researchers will no longer be available after that time.

Here is what one proponent of Velikovsky's theory, Dr.E.W. Mac-Kie, published in the British scientific journal, the New Scientist (January 11, 1973). Mac-Kie was referring to the unjustified negative reaction of mainstream scientists to Vilekovsky's hypothesis. ". . . Nevertheless, such understandable skepticism hardly explains the violence of the reaction against the book and its author, from certain quarters of the scientific establishment in the United States in the years following its publication. So outrageous were some aspects of this reaction when compared with the strictly rational ethics which is supposed to govern the scientist in his evaluation of new work that the situation eventually attracted the attention of psychologists. An entire special issue of the American Behavioral Scientist (September 1963) was devoted to this extraordinary story and this material was later expanded into a book, "The Velikovsky Affair (1966)."

And here is what somebody wrote about all this in wikipedia: "The plausibility of the theory was rejected practically unanimously by the physics community. Both the cosmic chain of events, and the fact that they left no known trace on Earth except as myths, were described as simply contradicting the basic laws of physics. More recently, the absence of supporting material in ice-core studies (such as the Ice-3 and Vostok cores) has removed any basis for the proposition of a global catastrophe of the proposed dimension within the later Holocene period.

Supporters contend that a few of Velikovsky's predictions have been validated. Amongst the most prominent is the prediction that Venus would be very hot. This argument has been countered by noting that Venus is indeed very hot, but not at all for the reasons proposed by Velikovsky. Critics further claim that the vast majority of Velikovsky's predictions turned out to be far from correct. His supporters have continued to claim counter-refutations, but space-probe results do not support the proposal.

In fact, Velikovsky started from myths and traditions of ancient peoples and cultures, postulated that they are based on actual events, including a chain of worldwide global catastrophes, and then he constructed an account to physically explain these events. He made no attempt to analyze his theory from a physical point of view — he was not a physicist — (a fact underscored by the content of Cosmos Without Gravitation) and often remarked that if they contradict the theories of physicists, then the physicists must correct their theories."

It is important to be aware of an essential difference between the hypothesis made by Velikovsky and the experimental discovery announced by Fleischmann and Pons. Velikovsky's ideas were developed to produce a scientific basis for several Biblical episodes. They were only speculations, not a theory derived mathematically from the known laws of nature. Velikovsky was not a physical scientist; he was a medical doctor and a biblical scholar. Fleischmann and Pons, on the other and, were recognized experts in the area of physical science in which the discovery was made.

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283) A set of issues worth discussing?

Ludwik Kowalski; 2/28/2006
Department of Mathematical Sciences
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What follows was extracted from a short power-point document. That document was used to discuss a set of selected topics. The discussion was limited because time was limited. But it did take place on November 28, 2005, during a meeting of ISCMNS members who attended the 12th International Conference on Cold Fusion in Japan.

Slide 1 Reproducibility is the soul of science!

Scientists take nothing on faith, they are skeptical. They like to check new claims in laboratories. Then they say "do as I did and you will see the same." That is the essence of the scientific method. But in the CMNS fields things still do not work that way. Why is this so?

Slide 2 Four steps of the ideal scientific method:

1) Discovering facts (reproducible observations).
2) Hypothesizing (tentative explanation).
3) Testing the hypothesis (verifying predictions).
4) Universal acceptance of the proposed model. Where are we in this process?

Slide 3 Why do we have problems with reproducibility?

It often takes time to learn how to control new phenomena:
1) Think about electrostatic experiments, before the role of humidity was recognized.
2) Think about first transistors.
3) Think about first light bulbs.
4) Think about first flying machines.
5) Think about cloning.
Please mention other cases. We need such cases to discuss CMNS issues with honest skeptics.

Slide 4 On the other hand we know that:

CMNS will not be recognized as science, unless at least one claim becomes 100% verifiable (by any qualified scientist, anywhere in the world). Is it not strange that 16 years and not a single 100% reproducible phenomenon has been identified?
WHY IS IT SO ?

Slide 5 My answer is: "because we work in isolation."

Meeting once a year, even if everyone is able to attend, is not a replacement for what is available to scientists working as teams, for example, in national labs and at universities. Coordination of efforts would help us to be more efficient.

Slide 6 Fortunately, we now have a discussion list.

A team of ~130 brains? Not really. Why not? Why is the number of active participants limited to about 10% of

subscribers?

Slide 7 I see two possible reasons:

a) linguistic barriers
b) technologically-oriented researchers prefer secrecy.

But at least 75% of subscribers know English very well.
Would electronic translators help?
What is the main reason of limited participation?

Slide 8 Peer reviews?

a) Yes, for obvious reasons we are deprived of benefits of peer review.
b) Can the CMNS list be considered a substitute for the formal peer review process?
c) What should happen to make this possible?

Slide 9 Why not?

Some are not aware of the list.
Some might think that only positive results are worth sharing; sharing negative results can hurt the CMNS reputation.
It is not polite to criticize others in public.
Fear of retaliation? What else ? ? ?

Slide 10 Mini-conference using the CMNS list

For example, every 6 months. Would this help?

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284) Black Light Power calorimetry
Underground energy states of hydrogen atoms

Ludwik Kowalski; 3/1/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Introduction:
Occasionally I receive messages from readers who I do not know. Here is a message received two days ago: "Dear Sir, I have been following this field since I first acquired Internet access in 1993. Having read both these items on your web site, I would offer the following as IMO the most likely scenario. The Russian device [see items #216 and #226] works as follows:-

The cavitation reaction works to produce a strong vibration in the dielectric material which is probably piezoelectric. This can produce initial charges which can indeed be separated by a flowing dielectric fluid (very pure water). The resulting high voltage at
the exit results in a plasma as stated. Now comes the new information:-

In the plasma, O++ ions are created which function as Mills catalysts for the shrinkage of the Hydrogen atom to hydrinos, or in the case of heavy water of Deuterium atoms to deuterinos. In either case, considerable amounts of energy are released. Occasionally a few deuterinos will shrink to the point where they may undergo a fusion reaction with a relatively short half life. However most of the energy probably comes from the shrinkage reactions. The Russians can't admit this without infringing upon Mills' patent(s).

Additional info:(1) The ionization energy of O+ is 35.117 eV, so a hot plasma is required to produce it, preferably combined with a strong electric field. The Russian invention meets this requirement. (2) Mills' website is www.blacklightpower.com. ..... (3) The major problem with the Russian device is likely that the dielectric material eventually self destructs as a consequence of the severe vibration. This may result in too short a lifetime to be commercially useful. Nevertheless, it could well serve as an illustration of what is possible, and may be capable of commercialization with some more work." I asked Robin van Spaandonk, about his background and asked for the permission to quote. He gave the permission and added: "I dropped out of physics after 2 years at the University of Leiden about 25 years ago, and fell into computer programming, like so many drop outs. I'm interested because it's going to be about a million times more important to humanity than the discovery of fire. How can I not be interested? You can find my own "take" on Mills' work at: <http://www.users.bigpond.net.au/rvanspaa/New-hydrogen.html>

The above massage prompted me to go to Mills' company's website

http://www.blacklightpower.com

I has several interesting items. I was glad that the company is still in business of conducting research and promoting promising technologies. I clicked on the "recent press" link and saw The Wall Street Journal article (1/9/06) about the company's business. The second item was an article from Guardian Unlimited (11/4/05) about fuel's paradise. Most downloadable papers on Mills' website deal with theoretical physics (from which the concept of hydrinos is derived) and with experimental spectroscopy (presumably confirming theoretical predictions). What attracted my attention was calorimetry and excess heat. That is what I want to describe here. But before doing this I would like to share some

observations.

Unlike the CMNS research the hydrino investigations seem to be based on a theory. Here is how this is described in the above mentioned Guardian Unlimited: "Randell Mills, a Harvard University medic who also studied electrical engineering at Massachusetts Institute of Technology, claims to have built a prototype power source that generates up to 1,000 times more heat than conventional fuel. Independent scientists claim to have verified the experiments and Dr. Mills says that his company, Blacklight Power, has tens of millions of dollars in investment lined up to bring the idea to market. And he claims to be just months away from unveiling his creation. The problem is that according to the rules of quantum mechanics, the physics that governs the behavior of atoms, the idea is theoretically impossible. . .

What has much of the physics world up in arms is Dr. Mills's claim that he has produced a new form of hydrogen, the simplest of all the atoms, with just a single proton circled by one electron. In his "hydrino", the electron sits a little closer to the proton than normal, and the formation of the new atoms from traditional hydrogen releases huge amounts of energy. This is scientific heresy. According to quantum mechanics, electrons can only exist in an atom in strictly defined orbits, and the shortest distance allowed between the proton and electron in hydrogen is fixed. The two particles are simply not allowed to get any closer."

It is unfortunate that critics focus on interpretation of facts rather than on the claimed reality of facts. In that respect the situation is not very different from that in CMNS. That field does not have an accepted theory and researchers were originally guided by a hypothesis of Fleischmann and Pons that excess heat has "nuclear origin." Instead of focusing on reality of excess heat critics focused on the fact that the hypothesis is not consistent with what is known about nuclear fusion. The history of CMNS would probably be very different if the authors wrote "we discovered excess heat of unknown origin."

As it stands, the CMNS is based on the assumption that excess heat has nuclear origin. The HS (hydrino science) does not make such claim; it is based on the idea of atomic origin of excess heat. Excess heat, according to Dr. Mills, comes from new kind of chemical reactions. These reactions are much more exothermic than reactions with which we are familiar. And here is another quote from the same article: "According to Dr Mills, there can be only one explanation: quantum mechanics must be wrong. 'We've done a lot of testing. We've got 50 independent validation reports, we've got 65 peer-reviewed journal articles,' he said. 'We ran into this theoretical resistance and there are some vested interests here. People are very strong and fervent protectors of this [quantum] theory that they use.' Rick Maas, a chemist at the University of North Carolina at Asheville (UNC) who specializes in sustainable energy sources, was allowed unfettered access to Blacklight's laboratories this year. 'We went in with a healthy amount of skepticism. While it would certainly be nice if this were true, in my position as head of a research institution, I really wouldn't want to make a mistake. The last thing I want is to be remembered as the person who derailed a lot of sustainable energy investment into something that wasn't real.' " Theoretical idea Dr. Mills are presented in a book he wrote; it can be downloaded (in the pdf) form the company website. I wish I were qualified to comment on the content of that book. But I can show the notes composed while browsing other files at that website.

My Notes

a) How does the blacklight process work? The H_2 molecules are broken into atoms. Ordinary hydrogen atoms are then reduced in size by interacting with catalysts. Transitions from the hydrogen ground states to the below-ground states liberate energy. "Products [are hydrinos] lower-energy H atoms, molecular ions, molecules, and hydride ions." The animation of the sequence is played in the <http://www. Hydrino_Animation.avi>

b) Explaining the release of energy (in the animation captions) they say: "First , energy is resonantly transferred from hydrogen to catalyst, which ionizes. Then the hydrogen atom shrinks to form hydrino and either releases a specific frequency of light or creates 'fast' (very hot) hydrogen." In the Step 4, labeled "disproportionalation' they say "A hydrino atom may also serve as the catalyst." The smallest hydrino shown is H/4.

c) What does the term "resonantly transferred" refer to? It refers to transitions from more excited to less excited states. In this case the "more excited" state is the ground state of an ordinary hydrogen atom while the "less excited" is a presumably existing state of lower energy. The role of the catalyst, an atom able to temporary absorb the energy released in the transition, is not clear to me. The catalyst, they say, must have excited levels matching the energy

released by the hydrogen atom. That is what the term “resonance” refers to. If the underground energy states exist then single hydrogen atoms placed in a vacuum should also be able to undergo transitions that are said to be energetically favorable. What kind of selection rules control probabilities of transitions to the Mills’ underground states?

d) The company will offer “a new energy source based on a breakthrough in hydrogen chemistry with paradigm-shifting applications.” Hydrinos are said to be hydrogen atoms whose binding energies, BE, are larger than for ordinary hydrogen atoms.

BE=13.6 (eV)/(1/p^2)

where p is an integer between 2 and 137. They are formed reacting with catalyst atoms whose “net energy of reaction is about m*27.2 eV, where m is an integer. The reaction results in release of energy and in the decrease of the size of hydrogen atom.

“Fractional Rydberg states” (underground states) have energies: E=13.598 eV /n^2 where n=1, 1/2, 1/3, 1/4, etc. . . These are states characterized by fractional quantum numbers. Emission lines corresponding to underground sates were presumably observed.

n=1 r=1.8660 A ioniz_energy=0.7542 eV lambda=1644 nm
n=1/2 r=0.9330 A ioniz_energy=3.047 eV lambda=406.9 nm
n=1/3 r=0.6220 A ioniz_energy=6.610 eV lambda=187.6 nm
n=1/4 r=0.4665 A ioniz_energy=11.23 eV lambda=110.4 nm

The wavelengths shown in the last column refer to the ultraviolet light light or to soft (not very penetrating) X rays. I suppose the the name “BlackLight process” was chosen to emphasize emission of the invisible electromagnetic radiation. Turning hydrogen into hydrinos, they say, releases much more energy than burning that amount of hydrogen (by a factor larger than 100).

e) The “in-depth tutorial” defines plasma as charged gas. Is it a mistake? Plasma is normally neutral; its positive and negative ions exactly balance each other on the macroscopic scale. They also say that “only certain atoms or ions are predicted to serve as catalysts for the BlackLight process, such as helium, potassium and strontium ions. The catalyst absorbs energy from the hydrogen atom (resonant energy transfer) which shrinks to become a hydrino atom. . . . Hydrino atoms then react to form new compounds. ..... These compounds are highly stable due to the higher binding energy of hydrinos. Once formed, hydrinos may sustain the reaction. For instance, two one-half hydrinos may react to form the one-fourth hydrino, a normal hydrogen atom and a net energy release of 68 eV.” Photons of such energies have been observed. They are, according to the tutorial, associated with dark matter.

f) In a section devoted to the business aspects of Mills’ scientific discoveries one can see the following statement: “ With water as the fuel, the operational cost of BlackLight Power generators will be very inexpensive. Moreover, rather than air pollutants or radioactive waste, novel hydride compounds with potential commercial applications are the by-products. The BlackLight Process offers a prospectively efficient, clean, cheap, and versatile thermal energy source. Two of the potential applications of its technology are in the heating and electric power production.

Heat generating prototypes have indicated the BlackLight Process to be competitive with existing primary generation sources over a range of scales from microdistributed to central power generation. ..... The BlackLight Process is a new primary energy source that has unique competitive advantages in all energy markets: electricity, heat, cogeneration (electricity production with waste heat recovery and utilization), and motive power.”

g) Should one worry about accumulation of hydrinos (and of their compounds) in our environment? I am thinking about a possibility that large-scale power plants, based on blacklight technology, will be used. What should we do with the “non-radioactive ashes” produced in such plants? The ashes are said to be very stable. But can they also be dangerous? Pure hydinos would probably be escaping from the atmosphere but molecules of hydrino compound would be gravitationally retained on our planet, even if they were gasses.

h) One obvious difference, between the situation in the CMNS and in HS (hydrino science), should be mentioned. CMNS researchers (experimentalists, theoreticians and inventors) act as independently working individuals. The HS researchers, on the other hand, seem to be highly organized under one leader, Dr. R. Mills. Organizational structure, and coordination of research, should be an advantage, provided the leader is knowledgeable, flexible and willing to learn from criticism. An effective leader should not act as dictator, s/he should listen what the coworkers suggest and coordinate their initiatives. Progress in the field of CMNS would probably be more rapid if an effective organizational structure could be established.

Calorimentry.
Those who read items #264, #267 and #270 know that last summer I participated in two investigations of excess heat. That explains why the 2003 paper of R.L. Mills et. all, "Plasma power source based on a catalytic reaction of atomic hydrogen measured by water bath calorimetry" attracted my attention. The article I downloaded was published in Thermochimica Acta, vol 4006, 28 November, 2003, pages 35-53. It describes a setup in which excess heat was measured calorimetrically. The electric power was delivered to the setup at the rate of 8.1 W while the heat was generated at the rate of 30.0 W. The excess power is reported as 21.9 W. That corresponds to a much higher COP (coefficient of performance=3.7) than reported by Fauvarque et. all. "The sources of error were the error in the calibration curve (+/- 0.05 W) and the measured microwave input power (+/- 1 W)."

The setup consisted of a quartz tube (inner diameter half-inch) that could be filled with a desired gas, for example, a mixture of 10% H_2 and 90% of He, at a desired pressure, such as 10 Torr. The tube was placed into a microwave cavity delivering electric energy to the gas at the frequency of 2.45 GHz. That resulted in the ionization of gasses. The electric energy, in the form of microwaves, delivered to the gas, for example, at the rate 30 W, was known. Measuring the input power was performed with power diodes, identical in all experiments. Rates at which thermal energy was generated, in a setup whose heat capacity was known, were determined from the rates at which temperatures were changing. Small thermal power corrections had to be made to account for the small conduction and convection losses.

The main purpose of the described experiment was to measure the temperatures of ionized gasses (plasma) on the basis of Doppler thermal broadening of spectral lines. The interesting conclusion was that, under identical conditions, "the average hydrogen atom temperature [in the presence of 98% of He] was measured to be 180-210 eV versus ~3 eV for the pure helium. The electron temperature for helium-hydrogen was 30,500 K compared to 7400 K for pure helium." Ionized helium is believed to be a catalyst for production of hydrinos, the process presumably responsible for excess heat. Hydrogen mixed with Kr and with Xe was also studied under identical conditions (input power 85 W).

P.S. (3/3/06)
I also looked into a more recent publication of R. Mills (March, 2005) downloadable from the above website. The link to this article is located at the end of the list of publications that pops up after one clicks on the "what is new" link. The long document (over 100 pages) is entitled "The Hydrino: Lower-level States of the Hydrogen Atom Which Have Remarkable Consequences." Near the end (why are pages not numbered) Mills refers to the Calvet calorimeter. The description states: "Calorimeter wall consists of thousands of thermoelectric elements in series. Thermoelectric elements transduce heat gradient to voltage which is proportional to energy released in cell. Highly sensitive and reproductive. The calibration curve, voltage (0 to 4 V) versus wattage (0 to ~16 W), is a nearly perfect straight line. The intercept is zero and the slope is 0.233 V/W.

That calorimeter was used to measure excess heat from electrochemical cells when potassium, rather than helium, was used a s catalyst. That is interesting in terms of what has been reported in items #267 and #270. We also used the K_2CO_3 electrolyte. If hydrinos are produced in Mills' cell then they should also be produced in our setups, as suggested by Robin van Spaandonk. By the way, contrary to situations described in the item #270, Richard is now reporting (so far in private) the excess heat of ~10% at 350 V. Mills seems to be studying low voltage electrolysis. Comparing two electrolytes he reported: "0.6 M K_2CO_3 electrolysis produces 0.4 watts of thermal energy above the control Na_2CO_3 during pulsed electrolysis." The wattage is small but the COP is ~2. All this is very interesting and worth confirming. Were calorimetric studies of Mills et. all confirmed by other researchers?

As I mentioned at the beginning, the excess heat from formation of hydrinos is not claimed to be nuclear. And it has

nothing to do with condensed (crystal-like) matter. But a small fraction of excess heat reported by Mills can be nuclear. I am thinking about H_2 molecules formed from two hydrinos. The atomic nuclei in such molecules are closer to each other than in ordinary hydrogen molecules. Thus a probability of under-barrier fusion of two hydrinos must be much higher than in common hydrogen molecules.

P.S. (3/4/06):
A friend who saw the draft of this item informed me that last year The Institute of Physics (London) has published a short article on hydrinos.....You can see it at: <http://physicsweb.org/articles/news/9/8/4> But this refers to the theoretical aspects of hydrino science only. I was disappointed by not seeing a single words about experimental facts discovered by Mills and his coworkers.

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Another friend informed me that Mills discovered cold fusion in a cell whose cathode was Ni and whose electrolyte was K_2CO_3 dissolved in ordinary water. The reference sent was: Mills, R., and Kneizys, S. P. 1991. "Excess Heat Production by the Electrolysis of an Aqueous Potassium Carbonate Electrolyte and the Implications for Cold Fusion", Fusion Technology 20, 65. I went to the library at <www.lenr-canr.org> and discovered that it has not a single paper submitted by R. Mills.

Intrigued by this I wrote to Jed Rothwell and asked him why is it so. Here is his reply: "This is because Mills has not submitted anything. We are not excluding him. All of the papers in our library come from the authors, except in rare cases such as when the author is deceased. I believe Mills was the first to observe CF in a cell with the K_2CO_3 electrolyte in ordinary water and with the Ni cathode. I would like his paper, so if anyone is in contact with him, please ask him to send it to us." <JedRothwell@mindspring.com>

P.S. (3/6/06):
Another reader supplied this information: "Dr. Reiko Notoya of Hokkaido University gave presentations on her experiments with light water electrolysis setup with nickel and potassium carbonate giving excess heat and transmutation (this is just off the top of my head). These prsentations were at the ICCF-4, ICCF-5, and ICCF-6. As I understand it, she had an active light water demonstration table set up at the ICCF-4." Did Dr. Mills attend these conferences? I do not think so. He was probably no longer part of the cold fusion group.

P.S. (11/29/07):
Discussion of hydrinos started again at the CMNS list, as illustrated in unit 338.

This website contains other cold fusion items.
Click to see the list of links

285) Private correspondence before Colorado-2

Ludwik Kowalski; 2/22/2006
Department of Mathematical Sciences
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Introduction

The outcome of Colorado-2 experiment was described in the unit #271. While waiting for the outcome of the Texas-2 and Paris-2 I decided to prepare and post this unit. It is essentially nothing more than private Internet correspondence between participants of Paris-1, Texas-1 and Colorado-1 experiments. If findings described in the item #271 are confirmed then the correspondence leading to these findings might become historically important. I will assume that readers of this unit are familiar with contents of units #252, #267, #270 and #271. The appendix following the correspondence is about Mizuno's protocol, as described at the ICCF12 (12th International Cold Fusion Conference, November 2006, Japan.

= 1

Richard --> Ludwik+ Pierre + Scott + George (9/30/05)

Just to let you know that my CF setup is running. No excess heat yet. As you can see from the picture the reaction is a lot different then what we were seeing with the Pinnacle. This is more like what the other researchers were seeing. I'm convinced that the low stored energy of the Pinnacle changes the reaction. Hope the conference is interesting. If you get a chance you might ask other how long they took to ramp up the voltage. I can't really tell from the reports.

= 2

Richard --> Ludwik (12/11/05)

I thought you might find it interesting. Last night I had one test run that indicated excess heat COP=1.40. It seems to be important to ramp the voltage slowly to the critical region. I'm using two methods of measuring power. One method is a Kwh meter, that give a pulse every 2250 joules. The other is measuring volts and amps about 600 times a seconds and calculating joules. The two methods agree within 5%. This is just between you and me for the time being. Hope your experiments are going well.

= 3

Ludwik --> Richard + Pierre + Scott + George (12/31/05)

As you probably know, from postings at CMNS, I am still in Japan. There is nothing earth-shaking to report. But, for the first time in my life, I experienced an earthquake. Not a big one, fortunately. But my bed (on the 11 floor of a 13 floor building) was shaking strongly enough to wake me up in the middle of the night.

My understanding was that Richard continued exploring excess heat in a Mizuno type cell. What are your findings, Richard? People who heard our CMNS report said we did not load the cathode with hydrogen long enough. Can five of us come to a common conclusion about the possible reality of excess heat? Pierre said that he might be able to come to the US to perform an experiment with us, and to convince us that the effect is real. Another possibility is to work together in his lab in Paris. What do you people think about these suggestions?

= =

Richard --> Ludwik + Pierre + Scott + George (1/1/06)

The last couple of weeks I have not had a chance to try any new experiments. I've not seen any excess heat in the 30 test I've run. I would be willing to go to France and observe the experiment. I would like first to work closely with Pierre to try and replicate the experiment here. As a diversion I tried to replicate the APE experiment of J.L. Naudin. This experiment has just two tungsten rods in a solution of K2CO3. I sent Naudin a couple of emails asking several questions and I've not received any response. I'm back to working with an experiment closer to Clauzon. I have a couple of questions for Pierre:

How often do you start with a fresh K2CO3 solution?
I've been heating my solution with the CFR electrodes is this wrong?
Should I be using an ohmic heater?
Do you think the current density of the cathode/anode is important?
How do you raise the voltage to the operating point? Speed rate steps
How long does it take to reach the operating point?
How do you know your at the operating point?

I know you have already answered some of these questions. Since I've not been able to reproduce the results maybe a second explanation will help.

= 5
Richard --> Ludwik + Pierre + Scott + George (1/2/06)
Pierre, Could you put together a timeline that shows your voltage adjustments and the voltage/current response of the cell for a successful experiment? I'm sure that I'm not using the correct voltage profile. You also indicate a delay in beginning the measurements as the current stabilizes. I would like to duplicate this profile if possible.

= 6
Richard --> Pierre + Ludwik (1/25/06)
I thought you might like to look at this data. I'm not sure what it means!! I'm very skeptical of these results. I'm using two methods of measuring input power one is a commercial watt meter that gives a pulse every 2250 joules and the second uses the average voltage and current reading. The graph below is several hours of testing. The yellow line is the electrolyte loss and gain (I added more water).

= 7
Richard --> Pierre + Ludwik (1/25/06)

Pierre, I'm still looking for definitive results. I had some data last weekend that looked promising. I'm just not high enough about the noise level to be positive.

I have a lot of noise in my input power measurement where I'm measuring voltage and current to get power under plasma conditions. The commercial watt meter I'm also using gives a pulse for every 2250 joules delivered. These two methods correspond to within 5% when I'm using an ohmic heater. When I am measuring the plasma input power I see a much larger difference so I'm not sure which is the most accurate. A lot of my data shows COP = 1 +/- 5% regardless of which input power measurement I use.

I'm having trouble maintaining the yellow plasma for a long time. The plasma may start out being yellow but in a short time turns mostly white. The papers seem to indicate that the proper operating point is when you have the yellow plasma for an extended time. Do you agree with this statement? I see all the changes you mentioned as the voltage is raised. I plan on doing a lot more test!!! Hopefully I'll get some definitive results soon.

= 8
Pierre --> Richard + Ludwik (1/25/06)
By the way Richard, where are you in the CFR tests ? I am anxious to know more about your experiment. Friendly yours Pierre. PS: we are trying also to duplicate the experiment of Koldamasov and Yang....

= 9

Richard --> Pierre + Ludwik (1/25/06)
" I ran a dozen experiments tonight all negative. I . . " Thanks for sharing. It seems to me that Richard has reached the point at which the best way to solve the COP puzzle would be to work with Pierre. Both of you indicated willingness to travel and work on the experiment together. Perhaps it is time to organize this; I would be happy to join you. Are you ready? When and where should we meet? Attached is the photo of Pierre

= 10
Ludwik --> Richard (1/27/06)
If going to Paris is a problem for you do not hesitate to suggest that Pierre comes to Boulder. He told me in Japan that this would not difficult for him. I would also prefer to use your setup in Pierre's presence rather than the other way around. We should not miss this opportunity to come to a mutual agreement, one way or another. That could become an interesting report at ICCF13, probably at washington DC next summer (I hope).

= =12
Pierre --> Ludwik (1/26/06):
When back from JAPAN to your home, are you always planning to see your dear aunt in PARIS ? If so, I would be pleased to arrange tests in my lab (CNAM) but also at " les Renardières" with Didier NOËL (EDF), and even with Jean-Louis NAUDIN at GIFNET (Fontainebleau). We will see after all these tests if we have to decide Richard and me to join our efforts somewhere...

= 13
Ludwik --> Pierre + Richard (1/26/06)
Pierre: My participation is much less important than that of Richard. I have not been in touch with Scott Little. Perhaps Richard was. What his attitude toward additional experiments? Since you and I are retired finding time (a week?) for an experiment in Paris is probably not a problem. But Richard has a full time job. So it is he who should suggest when we can possibly travel to France?

= 11
Richard --> Ludwik (1/27/06)
I would like very much to go to France. I had the impression from Pierre's email that he was suggesting I wait until you had review all the test. I did not want to invite myself. I would be able to go the France in late February or early March.

I think it would be better to review his working system than try to make mine work. I could bring my measurement system and we could add it to Pierre's system. This would give us great confidence in the input power measurement.

= =14
Ludwik --> Pierre + Richard (1/27/06)
Dear Pierre,
In my opinion it would be better if you could help us to confirm your results in Richard's lab than for one of us to go and witness your experiment. If you come and help us to demonstrate the COP > 1.2 in Colorado then it would be a second independent setup confirming your findings. It is not the same as one of us saying "I did not see anything wrong with the experiment performed in front of me in Paris." I am not an electrochemist. My testimony would not add much to what has already been described by you. Let Richard perform the experiment, using an independently built replica of your setup. You will watch and tell him what to do and what not to do. I assume he will be successful after a couple of days.

Suppose his results also become 100% reproducible. We would then describe the experiment with sufficient number of details and offer help to anyone who wants to be convinced that excess heat (at least several tens of watts) can easily be generated in a Mizuno-type cell. You would do this in Europe and we would do this in the US. Perhaps we can, after a year or two, convince honest mainstream scientists that the excess heat claims of Mizuno, Naudin, etc. can now be measured by any qualified researcher in any laboratory. That "missionary work" could transform the situation radically. One truly reproducible experiment will change the CMNS from proto-science into recognized science. That would be a fantastic accomplishment.

Are you able to come in February? Tell us at once and we will start talking about practical things. For example, how to minimize costs, etc. One possibility is for two of us to stay in the Youth Hostel in Boulder. That is where Richard's own home laboratory is. Boulder is less then one hour (by bus) from Denver; a major international airport. Any time interval between now and 3/2/06 or after 3/8/06 is OK for me. But we should start planning as soon as possible.

= 15

Richard --> Ludwik (2/2/06)

Have you any word from Pierre about visiting Colorado? Did you get my last email? Below is a copy. I don't want to loose the opportunity to work with Pierre on this experiment! I'm willing to go to Paris if that's what it takes!

= 16

Ludwik --> Pierre (2/2/06)

Assuming the message I sent you about one week ago was lost I am writing again. The more I think about it the more am I convinced that we would accomplish more (in terms of convincing others that COP>1 is real) if your results are reproduced by Richard. Another success with your setup will not add much to what has already been reported by you. I hope that the absence of the reply from you does not mean that you sick, or something else of that kind. Please reply at once, even if you need some time to decide about coming or not coming to Colorado.

Richard, thanks for offering us your house for staying. Where is your lab now? Is it at your home or is it in another location? I will order the airplane ticket as soon as I know that Pierre is coming, and when? To get a cheap ticket I must buy it at least two weeks ahead of time.

= 17

Richard --> Ludwik + Pierre (2/2/06)

This is great news. I'm looking forward to working with you both. My lab is now in my basement. I have a setup that will give us a little over 300 Vdc at 20 Amps on the cell. I'm using a variac, transformer and full wave bridge for rectification. It would be a good idea for Pierre to bring his electrode and anode. I still wonder about the Pt on NB for the anode. This is one common thing about Scott Little and my experiment.

= 18

Pierre - Ludwik + Richard (2/2/06)

After discussing with my friends (Pr Fauvarque, J.P Bibérian and some others), I think I will go to Denver and try to help Richard to succeed with this experiment .First, Ludwik, are you always in Japan for the time being? Do you plan to go home via Paris (France) ? If yes, it will be for us very interesting to show to you our experiment at the CNAM laboratory. Second, I thanks also Richard for offering us his house for staying. I am a retired nuclear physicist and I will go to Denver at my own expenses, so I appreciate very much this Richard's offer. Now for how long and when.? I must ask for a visa from the American Consulate and it may take a few weeks, as our french passports are not yet in the new required shape. I will try to do my best by the end of this week.

I will also see air companies for their best fares, and I think that one week of stay in the USA is required in this case. I suppose that this will be a sufficient time for our experiment, as Richard must have already the necessary equipment. I think that the possible period could be for me between March the 6th to the end of March. Tell me please, both of you, if this proposal seems reasonable and, of course, as soon as possible. As you can see, I believe in cold fusion....

= 19

Ludwik --> Pierre + Richard (2/2/06)

I returned home about ten days ago. And only now is my biological clock more or less normal. I am glad that you are able to come to Boulder. If you are a skier take what you need and I would then do the same. But this is not as important as our experiment. I did not know that French citizens need visas to visit the USA; is this something new? Richard will probably tell us when is the best time to come, considering his regular work. Any time after March 8 is OK with me.

Can you bring your Pt anode; this would eliminate one possible uncertainty ("anodes are not identical")? Also one or two French tungsten rods, just in case this makes a difference. We should probably start discussing everything that can help us to succeed. My mind was away from this topic for while but I will review what I knew.

= =

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This is great news. I'm looking forward to working with you both. My lab is now in my basement. I have a setup that will give us a little over 300 Vdc at 20 Amps on the cell. I'm using a variac, transformer and full wave bridge for rectification. It would be a good idea for Pierre to bring his electrode and anode. I still wonder about the Pt on NB for the anode. This is one common thing about Scott Little and my experiment.

= =

Pierre --> Ludwik + Richard (2/3/06)
You were right, Ludwik ! I have no problem with my passport, quite new, and I don't even need a visa. So now, we have to agree for the week at Boulder. Are you OK, Richard and Ludwik, for the week starting from March the 6th? Thanks in advance for a quick reply. Friendly yours........PS: of course, I will bring with me anode, cathodes and wattmeters…and maybe clothes for skiing!

= =

Ludwik --> Pierre + Richard (2/3/06)
I can come on Wednesday, March 8, or later. I suspect that Richard would probably prefer us to arrive on Friday because he has a regular job. . . Richard wrote "I will bring with me anode, cathodes and wattmeters. ..and maybe clothes for skiing!" That is good. What else would be worth bringing, Richard? You wrote that the highest voltage of your homemade power supply is 300 V. Is this enough? The COP was 1.35 at 350 V, and much less at 300 V. We certainly do not need 20 A. But going up to 370 V would very desirable. (Possible systematic and random errors of ~5% could prevent us from accurate measurements of COP below 1.10 or so.) What limits the voltage of your power supply? Can the limit be pushed up if we sacrifice the amperage? I am thinking about going down to below 4 A, as in many Naudin's setups. What about adding another front-end variac?
.

= =

Richard --> Pierre + Ludwik (2/3/06):
The week of the 6 is ideal for me. One of my sisters will be skiing a Breckenridge starting the 11. She has a condo and we could stay one night there and ski a couple days. Ludwik I think you can ski free since you are over 70. I have additional family coming in starting March 15. Since Ludwik can't come until the 8th lets plan to run the experiment from the 8th to the 15th. If you need to come a day earlier or leave a day later that's OK. My basement is not really a lab but I have all the equipment we should need. I have plenty of K2C03 500g. I've been using just distilled H20 I've thought maybe we should use de-ionized distilled H20? I have 6 mm tungsten rod 0.3 meters long. It is very pure tungsten rod. I was burning so many of the 2-4 welding rods I purchased some larger diameter rod.

= =

Ludwik --> Pierre + Richard (2/4/06)
(3) I just spoke with Richard and he said that the power supply goes to 450V, not only 300V, as he indicated below. That is good, Pierre's COP at 300V was small but it was ~ 1.3 at 350V.
(5) I am very happy that we will be working together in about a month.
(6) Attached is a file with our recent correspondence. Please give me permission to attach it as an appendix to unit #271. That unit will be devoted to our upcoming experiments.
(7) Let us hope it will go to a museum (Palais de la Decouverte in Paris?) one day. But the precondition for this is to produce a protocol that can be used by any qualified researcher to demonstrate reality of excess heat anywhere in the world. That would certainly change our proto-science into science.
(8) We know what we wish to accomplish. But, hopefully, this will not prevent us from being objective. Let us hope for the best.
(9) We will have a lot to do after excess heat results become reproducible. The next step, both in Paris, Texas and

Colorado, will be to optimize excess heat, one parameter at a time. Right?

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Richard --> Ludwik (2/9/06):
Looks like Scott doesn't think it will work. [He wrote]: “Thanks for the invitation, Richard. Frankly, I just don't think the chances of any change in the outcome of that experiment outweigh the expense and time of the trip, despite the favorable location you have. Of course, if you guys do get o-u results with Clauzon's assistance, I would be honored to have the opportunity to perform the first independent replication of those results. Our apparatus is all still in one place and could be resurrected quickly and easily. Good luck. Stick to your principles and demand good measurements.”

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Ludwik --> Richard + Scott (2/10/06):
I am not surprised by Scott's reply. He has spent much more time on trying to confirm excess heat from Mizuno-type cells than we did. And each time the excess heat was nearly zero. In his place I would probably feel the same way. But I am glad that Scott remains open-minded. Let us hope that our results will be different. I am glad that he is ready to try again, if we succeed.

Pierre (and his friend Paul Biberian) think that our failure to succeed was due to insufficient preparatory loading. Paul is an electrochemist. My reaction to this (during a dinner in Japan) was “but we followed your protocol. If loading (by increasing the current gradually) is so important then why was it not even mentioned in your paper?” They smiled and said that such things should be obvious. Our description of the protocol, if the COP>1 is confirmed, should be much more detailed.

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Richard --> Ludwik + Pierre (2/9/06)
Ludwik, I found out today that there is a way to run the Pinnacle on 220 single phase power. I’m going to borrow another Pinnacle so that if we are successful with the variac system we will be able to use the Pinnacle and confirm our results. This will also let us go to much higher voltages. I think now that there was something wrong with our first experiments.

Pierre,The Pinnacle is a programmable power supply that can deliver 0-800 Vdc in single volt increments. The supply can be programmed to regulate the voltage current or power. The supply is designed to deliver continuously 20 kW yes 20,000 Watts. In our case we will be limited to about 4 kW. It also reports joules deliver to the output. We had some difficulties with it during our first experiment but I’ve learned a lot since then.
Richard

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Richard -->Ludwik (2/10/06):
This is just between you and me. Tonight for the first time I saw the current drop when it’s suppose to and the reaction inside the beaker became more placid. These are thing that have been described by other researcher but I’ve never seen. I’m very optimistic about the upcoming experiment. Lets all have very positive thoughts. This is going to be a lot of fun.

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Ludwik --> Pierre + Richard (2/10/06)
I think that it is not too early to discuss the strategy we would follow if the COP>1 were definitely confirmed in our Boulder experiment. Presenting our results over the Internet, and at the ICCF13, is worth the effort but this is definitely not enough. I am thinking about many good reports that have been published in that way but had very little impact on the overall situation.

What should we do to convince mainstream scientists that a truly reproducible experiment now exists to explore a puzzling “excess heat” phenomenon? Our weakest point would be the inability to rule out a possibility that a chemical fuel might be responsible. Reviewers will not fail to notice that non of us is a chemist. I suggest that we ask Fauvarque, or another recognized electrochemist, to write a detailed appendix entitles “Excess heat cannot be of

chemical origin." He can probably do this now, on the basis of what has been done in Paris. Our paper would be stronger with an author who is an electrochemist. Samples of materials we used (pieces of electrodes, salt and water) should be preserved for future examination, if necessary. The paper should be sent to a mainstream journal. But it should not be a journal known for its unfair attitude toward CMNS. Any suggestion? Our paper should not mention cold fusion; it should refer to a "puzzling 100% reproducible, and simple to demonstrate" phenomenon. What else can we do to produce favorable attitude of mainstream scientists toward our findings?

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Richard --> Ludwik + Pierre (2/11/06):
When we have positive results that are reproducible I'm sure that we can get the help of several people at Fermi Lab to help publish this in the manner that will get the attention it deserves. My sister Jean Slaughter will be here later in March. If we can show her positive results she will be a big help at Fermi Lab. I've had emails from several others at Fermi that show real interest in CF.

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Pierre --> Ludwik + Richard +his colleagues: (2/13/06)
A message from Naudin is shown and Mizuno's ICCF12 paper is attached. Naudin wrote: "Pour info, regarde en PJ, le compte-rendu complet de Mizuno sur l'explosion de son CFR... C'est très intéressant, je pense qu'il a eu une nano-explosion nucléaire sur la pointe de son CFR... Soyez donc prudent dans vos manip à Denver..."

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Ludwik --> Pierre + Richard (2/13/06):
1) Thanks for the attached file (Mizuno). Very interesting. I do not recall him presenting this at ICCF12; perhaps I was too tired to pay attention.

2) Here is my description of Mizuno's protocol, based on this ICCF12 report:

Mizuno's Protocol (ICCF12 paper) K2CO3 at 0.2 M (700 cc in 1000 cc beaker).

a) Wait 3 minutes at V=0 (to show that cell temp=constant=30C
b) At t=180 sec start increasing V for about 30 sec. At t=210 s the voltage becomes 40 V.
The current during that 30 s time interval goes up with voltage and becomes 1.9 A.
The cell temperature is essentially constant 30 C.
c) Keep the voltage constant (40 V) till t=300 s. The current remains 1.9 A. The cell temperature goes up to ~31 C.
d) Sudden change of voltage to 65 V at t=450 s. The current starts growing rapidly, up to about 3.5 A.
e) Another sudden change of voltage (to 65 V) at t=550 s. The current becomes 4.7 A and then it starts going down rapidly. At t=700 s the current is 3 A; at t=2000 s I=2 A. The voltage is still 65 V at t=2000 s. The cell temperature at t=2000s is about 65C. This is far below boiling.
f) At t=2000 s the voltage is suddenly changed to 100 V. It remains 100 V till t=2500 s. The current also drops suddenly from 2 A to 1.6 A and then continues to decrease slowly. The cell temperature stabilizes at about 70 C. It never goes much higher.
g) Between t=2500 s and t=4500 s the voltage is changed, in eight small sudden steps, up to 250 V. Each step is by about 20 V, Once the voltage is changed it remains constant for about 6 minutes. The current keeps going down till t=3000 s; then it remains nearly constant, about 0.7 A.
h) At t=4500 s the current starts growing and the voltage is turned down. That is the end of one experiment.

3) This protocol is not good for us because only very small (hard to measure) amount of water will evaporate at 70 C, even if we wait for 2 hrs. Non-evaporative heat losses (conduction and convection), instead of being small, will become dominant.

4) I suggest that before coming to Colorado Pierre describes the boiling water protocol, more or less as Mizuno did (two graphs describing time dependence of three parameters). Also all qualitative observations, such as color and size of the plasma region at different times, relative strength of light emitted (going up, going down, remains about the same, etc.) We are going to replicate your boiling water experiment, not Mizuno's experiment with two calorimeters.

Protocol details will be of great help to us. Unless you already have them, I think that repeating the experiment (to collect as much information as possible), say at 350 V, is worth the effort.

5) What do you two think about my suggestion (in another message) to have a reputable electrochemist, for example, Fauvarque, on our team? He does not have to be present during the experiment. But he will write a detailed appendix about the upper limit of chemical energy that might possibly be in the excess heat we measure.

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Richard --> Ludwik + Pierre (2/13/06):

Your right the protocol is not good for our test setup. It is confirming what I've noticed recently that I've been rushing the test. Up until a few days ago I thought that each test would only take fifteen to twenty minutes the time it takes to boil off 30-40 g. Recently I've be giving the test a much longer soaking period at lower voltages. This changes the appearance of the plasma at the higher voltages.

I'm looking forward to trying the Pinnacle again. It will let me program the exact function described below. If we can setup a repeatable function that produces excess heat that will be a great help. Pierre if you could provide a similar step description then I can have the Pinnacle programmed before you arrive. The other advantage of the Pinnacle is that it is designed to maintain plasma in gasses. It has a feature that suppresses arc when they are generated in the plasma.

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Ludwik --> Pierre + Richard (2/14/06):

Plotting Mizuno's protocol as amperage (vertical axis) versus voltage (horizontal axis) one gets an interesting curve. Up to about 70 V the current grows with voltage, up to 4.7 A and after 70 V it decreases with voltage, down to 0.7A at ~200 V. How can this be explained? At low voltages the electrolyte is like a resistor (about 20 ohms). But R becomes much larger when a column of gas is formed around the cathode. At higher V the column becomes thicker and R goes up accordingly. That is the glow discharge mode. Above 250 V the current starts to go up because the arcing mode starts to develop.

I see no reason why what happens at 70 C should not happen at 100 C. The only difference is that the removal of the electrically generated heat at 70 C is done by cooling water (of their flow calorimeter) while at 100C this is done via rapid evaporation (boiling). Our last summer protocols were very different from the one described by Mizuno. That is why I am optimistic about the next experiment.

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Pierre --> Richard + Ludwik (2/15/06):

I did receive your recent e-mails about Mizuno experiments, but I have to think more about that ... During the two last months, the Fauvarque lab was very busy with works to do for external contracts and my friend Gérard was not free to work with me ... But, yesterday, in the course to prepare my Boulder trip, we operate again our CFR experiment. We have to confirm the results obtained, but I am very glad to tell you that at 300 volts we got a COP value of 1.13 and at 350 volts a COP value between 1.35 to 1.40...(as in our YOKOHAMA paper)

Of course, the real problem is the validity of the measurement of the inlet energy. To morrow, we will try to have in front of all the experimental set up a second wattmeter in order to confirm the value of the measured watt-hours ... One point important, a good calibration with the auxiliary heater. Richard, do you have two electrical supplies? We think that the continuous water feeding does not work very well. So, now we do not use it any more and we just readjust the water level between two following tests ... I will bring with me cathodes, anode, the wattmeter used, ... OK, Ludwik, I will ask Pr Fauvarque for the appendix we need concerning the chemical energy reactions .. See in picture joined my friend Gérard preparing our experiment with me...

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Ludwik --> Pierre + Richard + Gerard (2/15/06):

1) Thanks for the picture of Gerard. I am glad that he and you are performing confirming experiments. Please send us details of the protocol -- for example in the form of a table with three columns (time, volts and amps). Also additional

details about the plasma column (color, intensity, etc.). As you certainly know, many details are usually forgotten later. The best time to describe is immediately after experiments. This will allow us to comment on your observations.

2) One way to be certain the your input energy measurements are reliable is to deviate from the protocol drastically. Try to use pure water. At ~10 volts you will see a lot of bubbles; this is ordinary electrolysis. They will probably be ionized at high voltages and plasma will be formed (perhaps a different color?). If this produces the COP=1 then the electric energy is measured properly. Another way to drastically deviate from the successful protocol would be to change polarity (making Pt negative and W positive). P.S. I see that Richard (in a message that just arrived) also suggests deviating from the protocol.

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Richard --> Pierre + Ludwik + Gerard (2/15/06):
Yes I have two electrical supplies. I've been doing my calibration using an ohmic heater and the same supply I used to drive the cell. The ohmic heater is a 250 Volt 400 Watt device. I get the electrolyte boiling as vigorously as possible and measure the evaporated loss and input power. The non-evaporated loss always seems to be about 11 percent of the evaporated loss. Using the 11 percent in my calculations always gives me COP=1.0 +/- 0.03. Occasionally I've had anomalies where the COP has not been 1. The COP results have been both above and below 1. Until I rule out other explanation for the results I can't claim excess heat. I'm encourage by my latest data. I also tried the continuous water feed and gave up. I felt I could not keep track of the temperature of the extra water.

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Ludwik --> Pierre + Gerard + Richard (2/16/06):
Hi again. Another thought on your electric energy measurement. The fact that your COP is voltage-dependent seems to a reasonably good argument against a systematic error. Why would an instrument have a voltage-dependent systematic error? Why would the COP be 1.0 below 200 volts only? But, considering very high standards imposed on CMNS researchers (extraordinary claims need extraordinary evidence), a demonstration that the COP=1 at 350 V, when pure water and two Pt wires are used, would be extremely useful.

Our goal, at this stage, is to publish a paper in a mainstream refereed journal. We should anticipate all possible accusations and address them before they address them. That seems to be the only way to win. So far we identified two "weak point" (a) chemical origin of excess heat and (b) a systematic error in measuring electric energy. What else should we address?

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Pierre --> Richard + Ludwik + Gerard (2/18/06):
I send you a figure giving you the abstract of our 4 last CFR measurements made in order to prepare my trip. (Ludwik, trying to please to you, I put the power now in abcisses) . As you can see, our tests give good results, values going from COP = 1.14 (300v) to COP = 1.46 (350v)... I am planning to write down our experimental protocol and I hope to send it to you once corrected by Gérard by next monday or tuesday. Richard, as you can see, once over about 300 watts, the thermal losses are constant and we get a straight line parallel to the line with thermal losses equal to zero (2260j per G). I do not understand your results giving thermal losses equal to 11% of the power, whatever the value of the power. An explanation for not finding COP>1.0 ? As the matter of fact, once all temperatures being at equilibrium, the thermal losses must be constant and the slope of the line gives you the value: 2260 j/G. Please do not hesitate to give me your comments... I may be mistaken of course!
[The attached graph shows that four new measurements were made with the following results:

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Ludwik --> Pierre + Richard + Gerard (2/18/06):
1) I am glad that Pierre's new results are consistent with what was shown at ICCF12.
2) And I like to see the 2260 J/g label (no cold water is added during an experiment)
3) It would be easier to make comments if tables with raw data were presented. Here is a suggested format:

Table 1 (Ohmic heater:)

Volts Amps (seconds) evaporated (grams)
25 ....
37 ....
51 ....
62 ....
78 ....

Note that I am using font "courier" to create a table. It usually helps. Just cut and paste each table and replace dots with numbers

Table 2 (electrolysis:)

Volts Amps (seconds) evaporated (grams)
300 ....
300 ....
330 ....
350 ....

If Pierre's protocol is similar to that of Mizuno [see the appendix below] then neither volts nor amps remain constant during an electrolysis experiment. In that case Table 2 would refer to mean values. I would like to see a table with v(t) and i(t) for each of your four experiments. That would be a complete set of raw data.

3) I do not know how to answer the question about the 11%.

4) One thing should perhaps be added. If what was actually measured was electric wattage, P, (not current) then the wattage should be specified in the columns with raw data. And the protocol should be V(t) and P(t). If the protocol was similar to that used by Mizuno then P was probably not changing very much during each experiment. That is because i(t) was going down when v(t) was going up. But the P(t) curve is needed, unless P was exactly constant, which is very unlikely. The best is to measure all three things independently v(t), i(t) and P(t). That what we should try to do in Boulder; do not forget to bring the wattmeter, Pierre.

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Gerard --> Ludwik + Richard (2/20/06):

CNAM CFR EXPERIMENTAL PROTOCOL:

First, we take care of the electrodes, essentially the cathode. We verify that the bottom of the tungsten cathode not covered by the ceramic tube is about 1.5 to 2.0 cm long. The ceramic tube must go at the other end out of the electrolyte level by 3 to 5 cm.

In our case of a beaker of 2 liters volume, the electrolyte level corresponds to about 1200 cubic cm . (0.2 M of K2CO3)

Then, we begin the abacus of the thermal response by operating the ohmic heater. When temperatures equilibrium are achieved , we plot the answer on a graph, 100 watts of electrical power by 100. We send you an example few days ago.

Then, the beaker remaining at about 100°C due to the previous calibration, we put the electrical supplier on. We increase slowly the voltage. We observe pink sparks usually at about 100 v, but when the cathode is brand new the sparks begin at higher voltages (120 v to 150 v).

Then we increase smoothly the voltage and observe the mean current intensity :

150 v 1.6 A
170 v 2 A

200 v 2 to 2.2 A

The light of the cathode is uniformly orange. Then we continue to raise the voltage :

200 v 2 A to 2.8 A

Light becomes milky orange and you can see that the intensity seems to reach a kind of asymptote…

<u>Picture of the setup (showing the orange glow around the cathode) was included.</u>

Then when we arrive to about 300 v, the intensity in A usually begins to decrease . The light is between yellow and orange. Some small explosions can occur and we verify then if there are not short cuts. We verify too if our grid anode is able to control the movements of the electrolyte. If not, we must dispose some Teflon grids in order to calm the electrolyte waves. We verify also if there are not too much droplets out of the beaker…

Once a value of voltage is chosen, we wait for a while to see if everything look stabilized. Then we begin our measurements by 5 minutes runs. Usually, we disregard the first one and we keep the two or three following runs if they give about the same results.

Then we go on for other voltages after verifying the cathode length….

The measurements consist only to get the values of watt-hours used and the weight of the missing water . The watt-hours gives us the mean electric power and the missing water the thermal power… That's all folks

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Ludwik --> Gerard + Richard + Pierre (2/20/06):

Thanks for the protocol description, Gerard.

1) According to Google, "Unigore 390," is a digital instrument to measure ac and dc wattage. Your p(t), i(t) and v(t) keep changing during each experiment. I suppose you record samples of these three parameters. Right?

(a) Is it correct to assume that the averaging of samples is done electronically inside the instrument? In that case each recording is already a mean value from many samples. How many samples per recording?

(b) How often do you record mean values of p, i and v? I suppose that averaging of mean values is performed manually, after each experiment. By am not sure. Perhaps only p(t) is averaged.

c) What do you do to keep the voltage constant, for example 350 V, during an experiment?

2) What do you think about my suggestion to demonstrate that the COP=1.0 at 350V when pure water is used? I am assuming that our Boulder experiment will focus on your 350 V result (because of the large COP).

3) Richard, are you OK? You have been salient for several days. I hope to hear from you after you come home from work today. That would be about 20:00 by New York time. Pierre and Gerard will probably be in beds. I think that discussing details can be useful to all of us. Three weeks from today we will be in the middle of our attempt to succeed.

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Ludwik --> Pierre + Gerard + Richard (2/20/06):

Hi folks: Suppose our Boulder experiments confirm, without doubt, that the COP at 350 V can be as large as in Paris. What will we do with such success?

1) Should I assume that, at this stage, our main goal would be scientific and not commercial? Commercial attitude

usually means secrecy while scientific attitude implies openness. My suggestion is to remain scientific. The goal should be to reverse the current unhealthy attitude of mainstream science toward CMNS. Do we have the same attitude about this?

2) I am not against being involved in attempts to become practical. It would be nice to find a sponsor with "deep pockets" willing to built a large and well equipped lab. Than a systematic investigation of materials and parameters can be organized in order to scale up the COP. But this should come naturally later. Let them come to us when our undeniable success is recognized by mainstream science. How to achieve such success? That is a big question. Having a 100% reproducible demos, one in Paris, one in Boulder and one in Texas, would be our first step.

3) And what should we do next? That is what I would like to start discussing.

(a) How to succeed in publishing a paper in a mainstream referee journal?
b) What "public relation" steps should be undertaken?

I have some ideas about these things. But first I want to know what three of you think about being completely open at this stage of the game. Nothing will be hidden from those who want to replicate our findings. One thing we should not do. You probably guess what I am thinking about. A press release would be highly undesirable. But going to as many scientific conferences, with working demos, would be OK. Nobody will object if an experiment is performed in front of scientists. Demos coordinated with handouts, and with good web pages, can be very effective. And we will be addressing experts, not journalists. In fact we will be inviting them to either confirm or to refute our findings experimentally. Nobody will accuse us of sensationalism, or of bypassing "normal ways" of reporting new findings.

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Richard --> Ludwik + Gerard + Pierre (2/20/06):
Hi All, I'm fine. I've been working mainly to make sure all the equipment is working correctly. I'm also trying to set up a second cell so we can run two experiments at once. Ludwik you mention earlier that you had a triple beam balance. Could you bring that along? I'm watching EBay for a second electronic balance so I can record the data to my computer like we did before. I've also been checking my calibration based on your protocol Pierre. I see some differences from one calibration to the next. It is just measurement accuracy but I would like it to be closer.

What do you think of the idea of getting 5 or 6 publish scientist, that are not part of the CF community, to sign a letter stating that they have examined our methods and data and think the paper should be published. The letter would be written in such a way that they don't have to agree with results only that the methods and procedures are valid and that the paper deserves to be published. We might have to demonstrate our experiment to get their cooperation. The letter would be part of what is submitted for publication. I agree that our paper must not mention CF or make any suggestion as what is happening. We stick closely only to the claim of over unity output to input. Your suggestion of getting an Electro-Chemist to state that it is unlikely to be chemical in nature is a good idea. I'm just afraid that anything submitted that hints of CF, free energy over unity etc. will just be rejected without some traditional scientific support. We might submit the paper to several journals stating clearing that this is what we are doing. If done right we might create just enough competition that the journal feel obligated to publish.

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Ludwik --> Richard + Gerard + Pierre (2/20/06):
Richard: (1) The idea of submitting our paper with a signed letter of recommendation is good. In fact we may send this letter ahead of time and explain why we decided to do so. Then we would ask if the editor wants us to formally submit the manuscript to be reviewed. (2) You forgot to answer my question about being scientific or being commercial. (3) I will bring the triple balance; it will give us the mass lost +/- 0.2 grams.

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Richard -->Ludwik + Pierre + Gerard (2/23/06):
Once again thanks for the protocol. I feel like I'm learning to ride a bike. Each session of running the experiment I feel like I'm getting better results. I still haven't seen the rates in your paper but the data is improving.

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Ludwik --> Richard + Gerard + Pierre (2/24/06):
Hi Folks: How is everybody? I am recovering from a little cold. Better now at home than later in Boulder. Attached is a file that I want to post on my website today. It will open in a browser. Do you want me to say something more about our upcoming experiment?

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Pierre --> Ludwik (2/24/06):
Bonjour Ludwik,

J'espère que vous avez retrouvé une bonne santé et que nous nous retrouverons tous à Boulder en pleine forme... A propos de votre récente question, personnellement, je préfèrerais attendre que nous ayons terminé nos expériences avant d'en parler. Mais ensuite, bien sûr, il faudra en parler, qu'elles soient négatives ou positives. Une annonce faite avant risque de nous mettre sous pression ... et perturber ainsi les expérimentateurs.

Vous savez que la mesure de l'énergie d'entrée est un des points qui fait l'objet d'interrogations... J'ai lancé une expérience décrite ci-dessous qui est censée ne pas faire appel à des considérations électriques pour fournir une réponse..
Dîtes moi ce que vous en pensez... Bien sûr, je vous transmettrai lundi ou mardi les résultats (et en anglais pour Richard !). Excusez-moi de vous envoyer ce mail en français pour gagner du temps. Amicalement Pierre

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Pierre --> Ludwik (2/24/06):
Bonjour, Pour tenter de valider les indications de notre wattmètre, nous avons lancé hier après-midi une expérience avec Gérard en mettant en série la résistance de préchauffage (R=45 ohms) avec le CFR... L'expérience a très bien fonctionné et pour 300 v aux bornes du CFR, nous avions comme prévu 80 v aux bornes de la résistance... Ainsi, la résistance était soumise à une intensité très hachée de type CFR...

Seule variation notée : si nous avions bien au total 380 v constant, les 300 v CFR et les 80 v résistance étaient variables... Il fallait s'en douter ! Pas de difficultés majeures cependant pour comparer l'échauffement de la résistance avec les watt-heures mesurés aux bornes de la résistance dans cette expérience. Les premiers résultats ont montré une bonne cohérence, mais il nous faut confirmer avec un plus grand luxe de précautions pour obtenir une bonne précision... Ci-joint le schéma du montage utilisé. Amitiés PPC

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Ludwik --> Pierre (2/24/06):
I agree. References to our work will be omitted. I like your very clever approach.

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Ludwik --> Richard + Pierre + Gerard (2/24/06):
1) Responding to my message Pierre suggested that we should not advertise our upcoming experiment (as I did in the draft of unit #281). I agree that this would put us under unnecessary pressure. Therefor I remove references to our upcoming work.

2) After the experiment we will share the results widely, either positive or negative. In other words, it is a science project and nothing will be kept secret.

3) Pierre's message is in French. They are performing a very clever experiment whose purpose is to show that the wattmeter can be trusted, even when the current is highly irregular. The detailed description of results will be sent to us (in English) either on Monday or Tuesday.

4) Here is my understanding of what Pierre and Gerard are doing right now. The plasma cell is connected in series with a beaker that contains the ohmic resistor (45 ohms) in water. The power supply voltage is increased to produce the glow discharge in the cell. The only purpose of the the plasma cell is to produce the irregular current through the

ohmic resistor.

5) The beaker containing the ohmic resistor is on the scale and the rate at which water is being evaporated is measured. The wattmeter measures the rate at which electric energy is received by the ohmic resistor. Let us hope that the electric wattage and the thermal wattage will be identical.

6) What is not clear to me is which potential difference should be 350 V, on the plasma cell or on the ohmic resistor. I think that it should be on the ohmic resistor, because that is where the wattmeter is. They are calibrating the wattmeter. The only reason to have 350 V on the plasma cell as well is to be sure that the current waveforms are essentially the same as during the excess heat experiment. Ideally each beaker should be at 350 V. Another possible approach would be to replace the plasma cell by a vibrating contact, for example, from an old doorbell or from a Tesla coil.

7) I still do not know how the wattmeter data are recorded. Do they just read the numbers, for example, every 5 seconds, or do they record numbers electronically at much higher rate.

8) I suppose the distribution of the power-supply voltage between the two beakers is controlled by changing the resistance of the ohmic heater (nichrome wires of different lengths, for example, from an old home heater or from a student laboratory rheostat).

300 V, P_el = 403 W, P_th=459 W) COP=1.14 I=P/V=1.34 A
350 V, P_el = 730 W, P_th=1070 W) COP=1.46 .I=P/V=2.09 A
300 V, P_el = 379 W, P_th=462 W) COP=1.22 I=P/V=1.26 A
330 V, P_el = 439 W, P_th=572 W) COP=1.30 I=P/V=1.33 A

These results are consistent with what was reported by Pierre at ICCF12.]

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Richard--> Ludwik + Pierre + Gerard (2/24/06):
Pierre's suggestion not to advertise is good. I like Pierre' check of the watt meter. I've been using two methods of measuring input power since beginning of January. One is an AC Watt meter just after the Variac. The second is measuring V(dt) and I(dt) at the cell. These two methods have agreed within +/- 3%. I have strong confidence that the plasma current is random about a mean so errors are unlikely. Essentially I've been using the watt meter with highest reading as my input power. During calibration I get very consistent results. During the experiment if every thing runs smoothly the two watt meters agree. The AC Watt meter gives a DC pulse every 2250 Joules. At high power rates it's easy to miss a pulse this leads to some discrepancies. This is one of the items I've been working on to improve.

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Richard--> Ludwik (2/25/06):
I've been using 2260 joules/g for the vaporization of water. This is for sea level. Water boils in Denver at 97.3 degrees. Don't we need to use a slightly lower value for the Colorado experiment? Unfortunately the amount of excess heat I'm seeing is about 10%, if I have the right conditions. This is just to close to experimental error. I've been rechecking my calibration.

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Ludwik --> Richard (2/25/06):
I am happy that you measured the excess heat of 10%. Was this at 350 V? I do not think that difference between your ~10% and the ~35% in Paris is due to the pressure difference. But this is worth investigating. I do not know how L depends on the pressure and temperature. But I know that L does go down when the temperature goes up; it becomes zero at the critical point (374.1 C and 218.3 atmospheres). So at 97 C L is probably only very slightly larger than 2260 J/g.

Viscosity, like L is an indication of how strongly molecules attract each others. That is why I suspect that the temperature dependence of L for water is about the same as of its coefficient of viscosity. Here is a table for the viscosity coefficients:

degr C coeff
==========
0 1.787
10 1.307
20 1.002
30 0.7975
.....
80 0.3547
90 0.3147
95 0.2975
96 0.2942
97 0.2911
98 0.2879
99 0.2848
100 0.2898

As you can see, it is only 1% between 100 C and 97 C.

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From Pierre --> Richard + Ludwik + Gerard (3/1/05):
We made with Gérard the planned experiments with a resistance of 98 ohms on this Monday. The results obtained are quite encouraging. The deviation between thermal power of the resistance (measured with boiling) and the electric power as measured by our wattmeter was in most cases smaller than 2% and in one case equal to 3.7%. The current was the same than the CFR current... with a COP of about 1.12 to 1.14 (voltage on the CFR = 250 volts and total voltage, CFR + R, equal to 407 volts). We try 300 volts with the CFR but we got 460 volts for the total (CFR + R) and then it was too much for the power supply which caused some trouble to the wattmeter (intempestive stops). I will bring with me our technical paper on this subject. I ask again about my passport and both the air company and a travel agency told me that no visa was required for my passport... So I hope that I will have no trouble to go to DENVER...See you soon at Boulder...

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Ludwik --> Pierre + Richard + Gerard (3/1/06):
Pierre: Thanks for good news. I suggest you bring the 98 ohms resistor to Colorado. Richard's Pinnacle power supply will probably have no trouble to extend the calibration of the wattmeter to 350 V. In my opinion we should focus on electrolysis at 350 V because the COP is large and small errors (up to 5%) can be tolerated.

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Ludwik -- Scott (3/5/06):
. . . P.S. I am going to Colorado next Wednesday. Pierre, who is going to work with us, does not want to advertise our work because it will create unnecessary pressure. But he agree that results, either positive or negative, will be published after our work is completed. Hopefully this will be done in one week. Too bad that you are not able to come. We will be staying in Richard's home; his shop is in the basement. I will be happy to bring an additional sleeping bag. I wish you could be part of this combined effort to solve the controversy, one way or another.

Pierre will bring his wattmeter; it has been recently calibrated for the rapidly oscillating current. To accomplish this he used two cells in series. The purpose of one cell (plasma electrolysis) was to produce randomly oscillating current. The current was flowing through the ohmic resistor in the second cell. The rate of water evaporation from the second cell was giving them the thermal wattage. It was essentially the same as the input wattage from the wattmeter. Two weeks ago they measured the COPs again, at several voltages up to 350 V. This confirmed their results reported in Japan (COP =1.35 at 350 V).

= =

Scott --> Ludwik (3/5/06):

Good luck in Colorado. I trust you and Richard will not let Pierre "get away" with any measurement errors. If you do see a COP of 1.35, everything must be checked and double-checked before it can be accepted. In fact, I would say that an excellent test of the validity of such a result would be to repeat the tests in our lab. If WE also get a COP of 1.35 and we cannot find a mistake in the measurements, it will be time to celebrate, no?

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Richard --> Ludwik (3/6/06):
What do you think of one of these as a title for our paper. (a) “Does Plasma Experiment Violate First Law of Thermodynamics?” (b) “Help Plasma Experiment Violates First Law of Thermodynamics!, (c) “Plasma Experiment Violates First Law of Thermodynamics.” Just musing.

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Ludwik--> Richard (3/6/06):
1) I think that the title should more modest. Otherwise discussion will degenerate away from what we want. A sensational title would hurt us. If we are convinced that a really reproducible protocol has been found then the title should be something like "A simple reproducible protocol for observing the unexplained excess heat." We need independent confirmations, hopefully from many people, that our protocol is truly reproducible.

2) In my opinion we should follow the French protocol at 350 V only. Let not even try lower voltages because this would force us to address the issue of small error. The COP=1.3 or 1.4 would be much easier to "sell," and much easier to confirm by others, that smaller COPs. Pierre will be with us for 5 days only. Let us not try to do too many things at once. We will need many control experiments at 350 V. Also at least ten clean experiments to establish the mean COP and the standard deviation. The last day with Pierre (3/13/06) should be devoted to the draft of the article.

3) Do you still want me to bring the second (not electronic) scale? If so then reply as soon as possible; I am going to the university this evening.

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Richard --> Ludwik (3/6/06):
I didn't get a seconds setup running so there is no need for the scale. I suspect you are correct about the title. We want to be taken very seriously. I'm fairly convinced of the presence of excess heat at 350 Vdc. Unfortunately with my setup I only seem able to reproduce about 10% is excess of the input power.

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Ludwik --> Richard + Pierre (3/6/06):
My arrival to Denver, at 16:44, is by Continental (flight 728 from Newark, March 8) I will be waiting for Pierre near the fountain at the center of the main terminal. His arrival is at 18:29 (United flight 919 from Paris, March 8) I will turn my cell phone (portable) on at Denver. The number will be (862)-686-5053. See you both on Wednesday evening.

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Pierre --> Richard + Ludwik (3/6/06):
I did receive your mail and hope to see you soon. By the way, I send to you enclosed an internal technical paper on our wattmeter tests. (excuse me, Richard, it is in French, but I think it is understandable).I will have also others items to discuss with you. See you both on Wednesday...

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Ludwik --> Pierre + Richard (3/6/06):
If it is not too late I would suggest that you also bring some potassium carbonate so that we can use it to prepare the electrolyte. Perhaps it has something that Richard’s salt does not have. Or just bring a bottle with already made electrolyte; perhaps the missing ingredient (Strorms calls it NAE) is in your water. How else can we explain that that at 350 V you have the COP between 1.3 and 1.4 while Richard has only 1.1?

Ideally you should bring the entire setup. But that is probably too heavy. Once we get the COP comparable with what you measured at 350 V then we should try to get the same with the setup prepared by Richard (his salt, his water, his beaker, his electrodes, etc.). You will be in Colorado for only 5 working days. We should use this time as productively as we can. In my opinion we should concentrate on what seems to be the easiest, on 350 V. Working at lower voltages would force us to address difficult issues of small errors while at 350 V the total systematic errors of 5%, or even 7%, would be irrelevant.

Once we confirm your result we will have to measure the COP, at the same voltage, at least ten times. This will allow us to determine the mean value and the standard deviation. Referees of our paper would insist that this is necessary in a paper claiming high reproducibility. Then we must describe the protocol as accurately, and as clearly, as possible and wait for at least several confirmations from mainstream scientists. Turning protoscience into science, by offering a truly reproducible, and simple to implement, CMNS experiment would be the ultimate reward for us.

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Ludwik --> Richard + Pierre (3/7/06):I was going to translate to you the description of the wattmeter calibration. But Pierre is correct; you should be able to figure out what they did. I already described to you the general idea; this file provides numerical results. They are in the last table. It compares thermal wattage (column 2) calculated from the number of grams evaporated (column 1) with electrically measure wattage (column 3). As you can see, the maximum difference was 3.7%. The calibration performed at 250 V and ~ 220 W corresponds to the current of 1.1 A. It is hard to imagine how the thermal power and the electric power could become very different. According to Mizuno, the current goes down when the voltage goes up. Therefore going to 350 V might have a small effect on the power. Fortunately, you have a very reliable power measuring system at any voltage, including 350 V.

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Pierre --> Ludwik + Richard (3/7/06):
I had thought already to bring our potassium carbonate and all our equipment except our beaker (2l), a little too heavy... and of course the balance and the power supply. I joined a picture, taken at the nuclear power plant or Dampierre-en-Burly. Pr Fauvarque was thinking that the neutrinos may play a role in this Mizuno experiment. Unhappily, we were not able to see any effect...You can see in the picture Gerard Lallevé and myself with our guide from the operating staff of Dampierre, Chistine Léger. A bientôt

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Ludwik --> Pierre + Richard (3/7/06):
Great picture Pierre. Let us hope that the EDF (French Electricity Company) will take our results seriously. See you tomorrow.

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Appendix -- Mizuno's protocol

In his ICCF12 presentation (it is downloadable from the library at <www.lenr-canr.org>) Mizuno plots the values of the voltage and current versus time. That is his protocol. French protocol was similar. The only essential difference between the two experiment was in the method of measuring the heat. Mizuno's cell was kept at a constant temperature (about 70 C) by cooling if with water circulating in a flow calorimeter. The cell described by Pierre (nearly identical to ours) was allowed to reach the boiling temperature and the amount of heat released was measured by the amount of water that evaporated. I see no reason why what happens at 70 C should not happen at 100 C.

During the first 20 seconds the potential difference between the electrodes was increased gradually to 40 V. In the next 20 seconds it was increased, in several steps, up to about 80 V. It then remained constant for about 30 minutes. For the next hour the voltage was increased, also in small steps, up to 250 V. The current was decreasing progressively at the same time. Then the voltage was turned off. I suppose that most of the excess heat was generated when the current was decreasing from about 4.7 A to about 0.7 A

Plotting Mizuno's protocol as amperage (vertical axis) versus voltage (horizontal axis) one gets an interesting curve. Up to about 70 V the current grows with voltage, up to 4.7 A and after 70 V it decreases with voltage, down to 0.7A at

~200 V. How can this be explained? At low voltages the electrolyte is like a resistor (about 20 ohms). But R becomes much larger when a layer of gas is formed around the cathode. At higher V the layer becomes thicker and R goes up accordingly. That is why the current decreases when the voltage increases. Above 250 V the current starts to go grow with voltage. This is probably an indication that the glow discharge starts to show a tendency to become an ark discharge. Our last summer protocols were very different from the one described by Mizuno. That is why I am optimistic about the next series of Richard's experiments.

References:

1) Paris-1 experiment:
Fauvarque, J., P. Clauzon, and G. Lalleve, Abnormal excess heat observed during Mizuno-type experiments. 2005, Laboratoire d'Electrochimie Industrielle, Conservatoire National des Arts et Métiers: Paris. Available as: http://www.lenr-canr.org/acrobat/FauvarqueJabnormalex.pdf
2) Texas-1 and Colorado-1 experiments:
Ludwik Kowalski, Scott Little, George Luce and Richard Slaughter, see item #271.
3) T. Mizuno's ICCF12 presentation:
Downloadable from the library of http://www.lenr-canr.org

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286) Colorado-2 in Boulder and over the Internet

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The first message in the "Mizuno type experiments" was posted on March 15. This was only one week ago. Today my In box has 150 messages with that subject line. That means about 20 messages per day, on the average. I did not have time to read all of these messages very carefully in Boulder. But I will do this now, before deleting them from the Inbox. That might take several days. Being retired I have a lot of time. It is good to know that deleted messages will be preserved in the archive of the CMNS list. The purpose of this unit it collect ideas inspired by these messages.

1) X1 asked "Have you tried other metals e.g. steel wire?" That would be an interesting project after a RoD setup (reproducible results on demand) becomes available. Naudin was able to generate excess heat using anodes and cathodes made from several different metals. Iorio's cathode and anode were tungsten rods.

2) X2 suggested that we preserve the electrolyte for future examinations. We did this. he also wrote; " You might consider purchasing some more of the same production lot and keep them intact in a safe place, in case they contain some particular impurity useful for the reaction." I guess we would do this if our resources were not limited. Platinized mesh is expensive. Tungsten cathodes and potassium carbonate from the same supplier are probably identical enough. We would not start this investigation without what we learned from Naudin, and others, about good reproducibility. They said "do as we did and you should have similar results; they did not say that generation of excess heat depends on impurities. Only good chemists should deal with experiments whose outcomes depend on presence or absence of some contaminants.

3) JR wrote (Jed Rothwell gave me permission to quote anything he posts): "I think you need a more rigorous test to exclude the possibility of droplets, but Mizuno has done countless tests with closed cells from which droplets cannot escape, and he has found copious excess heat, so he has ruled out this possibility." In one case we covered the cell with a fine stainless screen and still observed excess heat. But doing this only once is not sufficient. In future experiments the open container should always be covered with such screen. Only steam should be allowed to escape; liquid drops and droplets should not be allowed to splash out of the cell. Why not? Because this creates illusion of excess heat. Fortunately, under favorable red-orange plasma, the amount of water lost due to splashing was less than 2% of what was lost by evaporation. This can be tolerated when the COPs are larger that 1.2 or so. Test with excessive splashing should be stopped at once because they are indicative of unfavorable plasma.

4) As soon as I finished composing the above, a new message appeared in my Inbox. It was from Jed. Here is a quote from his message; I agree with it 100%. "Much can go wrong with this experiment. You should not declare victory until you have done it many times, using many different calorimeter types and configurations." That is what should be done before we try to publish a paper. Today is 17th anniversary of the famous press release about the discovery of cold fusion. As indicated at the end of unit # 271, we should learn from negative consequences of premature publishing.

5) I am really happy that other researchers help us to identify potential errors and suggest methods to estimate them. The escaping invisible droplets was a good example. A message from Michel Jullian (MJ), that appeared in my Inbox at about the same time, as from JR, is another example. Below is his entire message (quoted with permission):

“Richard: Insulating layer not visible!? That layer I have been ranting about is nothing but the conspicuous floating foam of bubbles I can see on all the photographs, what else? Let me explain again my concern, hopefully more clearly: I suspect that when the liquid surface is calm (no heat-radiating splashes) this floating blanket of electrolytic gas bubbles must be a fairly good thermal insulator, much better in any case than the NON-insulation on the naked liquid surface which happily let radiated/conducted/convected heat escape during ohmic heater calibration (an ohmic heater makes no bubbles).

If you now consider that _most_ of the non-evap heat loss from an uncovered hot water tank is emitted by said liquid surface, there is no doubt that the non-evap heat losses will be significantly affected by a foam layer appearing due to electrolysis. They won't be 60W any more, but may be half of that, maybe less, in any case this error should be evaluated, if only to demonstrate that it can be neglected if such is the case. This is the point of the temperature decay measurement I was suggesting, not _instead_ of ohmic calibration, but _in addition to it_, so the error can be evaluated.

Does it make sense now Richard? (Please don't hesitate to criticize or ask for precessions) M.J.

PS Jed, the droplets question must be addressed indeed (a screen as used in Colorado-2 should be fine, two screens would be totally droplet-proof), but the missing mass due to dissociation is not a problem at all, on the contrary it leads to UNDERestimating the COP, as it is more than compensated by consumed dissociation energy (cf earlier posts in this thread by Scott Little and myself)”

It is a real pleasure to read such messages. They are very helpful. We are lucky to be part of a group of researchers (on a restricted Internet list) who are no less interested in transforming our protoscience into real science.

6) And here is another piece from JR. “Actually, the worst thing that can go wrong with this experiment is when the cell explodes. On the plus side, when Mizuno's cell exploded, it produced at least 411 times more than input energy, and probably several thousand times more energy than any chemical reaction with these materials could produce. It was anomalous -- no question about that! On the minus side . . . you can see why Mizuno has largely abandoned this line of research. If the cell can do this, I suppose there is no reason to think it cannot produce two or three orders of magnitude more energy. If that happens to you, you will, at least, die happy. Your last thought will be: ‘Hey, it worked!’ "

7) Let me summarize what has been stated in several messages today and yesterday. Being a retired teacher I want to do this in the form of a formal lecture.

A) Under ideal situation non-evaporative losses are zero. In that case Pt=Pe, provided there is no excess heat. And Pt>Pe when excess heat is generated. The Pe is the average electric power and the Pt is the average thermal power. The Pe can be calculated from the W*h received during a test and the Pt can be calculated from the amount of water evaporated during that test. Note that 1W*h=3600 J and that Pd=L*m/t, where L is the latent heat of water evaporation (2260 J/g), m is the mass of the evaporated water (grams), and t is the duration of the test (seconds). The COP is defined as the ratio Pt/Pe.

B) In reality the non-evaporative losses might not be negligible, for example, 10% of received energy might be escaping through the walls of the vessel (conduction, Pd), with the rising hot air (convection, Pc, and as the electromagnetic radiation (Pr). In other words, Pt=Pv + Pd +Pc+Pr. The COP is still defined as Pt/Pe but determining the Pv and Pe alone is no longer sufficient to to calculate the COP.

C) The sum of Pd, Pc and Pr is what we call the non-evaporative losses. How do we measure such losses? We do this by using the ohmic heater, for several different values of Pe, such as 400 W, 600 W, 800 W and 1000 W. The ohmic heater is inside our cell with the electrolyte. We know that no excess heat is produced in the ohmic heater. We measure two quantities, Pe and Pv. The always positive difference, between the Pe and Pv, gives us the rate of non-evaporative losses.

D) In a large vessel, and without stirring the electrolyte, the temperature is not uniform; it might be 100 C near the

source (between the electrodes or near the coils of the ohmic heater) and considerably lower neat the walls, for example 75 C or 85 C, depending on the Pe, the size of the cell and other factors. That is why the (Pd+Pc+Pr) should be determined for several Pe. The dependence of (Pd+Pc+Pr) on Pe is called the "calibration curve." In the ideal case, the (Pd+Pc+Pr) is zero for any Pe.

E) Next comes an important ASSUMPTION --> The rate of non-evaporative losses (Pd+Pc+Pr), determined by using the ohmic heater, are the same as the (Pd+Pc+Pr) during the plasma electrolysis. That is where a systematic error can possibly be made in the evaluation of COP. Let me consider several specific situations.

a) In Paris-1 experiments the measured (Pd+Pc+Pr) was the same at each sufficiently large Pe. That indicates that the entire electrolyte was boiling. Boiling provided natural stirring and the temperature of the vessel wall was also 100 C, at any Pe. But was the entire electrolyte boiling during the plasma electrolysis? Pierre told me that it was, at least when their heater was turned on during the electrolysis (correcting for the input heat supplied by the ohmic heater is trivial when the W*h delivered to the heater are measured during a test).

b) And what about cases in which the ohmic heater was off during the electrolysis? The answer depends on the size of the vessel and on thermal isolation around its walls. In our case (a beaker whose capacity was 2 liters) the measured temperature of the electrolyte, near the walls, was below the boiling point, for example, 80C or 90C, depending on the Pe. That is why our (Pd+Pc+Pr) was not the same at each Pe.

c) Pierre also told us that many measurements of COP were made without turning the ohmic heater on. As far as I know, the temperature of the electrolyte, near the walls, was not measured. As in the Colorado-2 experiment the electrolyte was not stirred. I suspect that under such conditions the (Pd+Pc+Pr) was not the same at every Pe. Was the dependence of non-evaporative losses on Pe taken under consideration in calculations of COPs? A good referee is likely to ask such question? My question is slightly different. I know that the average COPs at 300 V and at 350 V were essentially the same as in Colorado-2. In Paris-1, on the other hand, the COPs were found to be voltage-dependent. Can this be due the unjustified assumption that non-evaporative loses are the same at every Pe? This question must be answered before we say that COP increases with the applied voltage, as stated in the Paris-1 paper at ICCF12.

d) One of the question asked by Michel had to do with the Pc (rate of convection losses). Is it possible that, for some reason, the Pc during the ohmic heater calibration is not the same as the Pc during the plasma electrolysis. An error in the determination of Pc would lead to an error in the evaluation of COP. A good referee is likely to ask this question, unless we anticipate it and discuss the possibility.

e) Another question asked by Michel had to do with Pd (rate of conduction losses). Is it possible that, for some reason, Pd during the ohmic calibration is not the same as Pd during the plasma electrolysis. An error in the determination of Pd would lead to an error in the evaluation of COP. A good referee is likely to ask this question, unless we anticipate it and discuss the possibility.

F) In the last case Michel speculated about a possible cause of differences in Pd. Suppose, he said, that a thin invisible layer of gas is formed over the electrolyte during the plasma electrolysis but not during the ohmic heater calibration. That would reduce the Pd (during the electrolysis) and would lead to the overestimation of COPs. What kind of experiment can be performed to rule out possibilities of significant errors postulated in (d) and (e) above? Jed Rothwell suggested that other ways of determining the Pt (average rate of releasing thermal energy) should be used, for example, a flow calorimeter or a bomb calorimeter. I think that using a bomb calorimeter is a better idea. The flow calorimeter calls for a closed vessel (with a heated recombiner etc.) in a constant temperature environment. That seems to be more demanding that a bomb calorimeter used to evaluate caloric contents of foods.

G) Another good suggestion made by Jed was to determine the (Pd+Pc+Pr) from the cooling curve. Consider a brick at 90 C. The temperature decreases exponentially according to the so-called Newton's law of cooling. Suppose its thermal capacity is 50 calories per degree. Also suppose that the temperature goes down at the rate 0.2 deg/s. That would mean that (Pd+Pc+Pr)=50*0.2=10 cal/s = 41.9 watts. At a lower temperature the rate of cooling may be, for example, 0.1 deg/s. That would be an indication that the non-evaporative losses become two times smaller. The

thermal capacity of a vessel can easily be determined by using the ohmic heater. It is simply a matter of delivering a know number of w*h and measuring the resulting increase in the temperature.

Jed suggested measuring several values of (Pd+Pc+Pr) at low temperatures, such as 20 and 30 C, and extrapolating to the actual temperature of the cell's wall. I do not think that this is practical; the extrapolation uncertainty is likely to be unacceptable. But measuring the (Pd+Pc+Pr) at several higher temperatures, for example, between 80 and 90 C will offer a reliable extrapolation toward the wall temperature, such as 95 C. Water, however, is also evaporated below the boiling point and this fact must be accounted for, as I mentioned in an earlier message. Ideally the (Pd+Pc) determined from the cooling curve, and the (Pd+Pc+Pr) determined as in Paris-1 experiments, must be identical. In reality a discrepancy of 1 W, or so, would be acceptable because the (Pd+Pc+Pr) are usually close to 50 W. I am thinking about cases in which COPs are larger than 1.2.

The bomb calorimeter test and the cooling curve test must be performed before we are ready to write the paper. I would not be surprised to learn that such tests are being conducted right now in Paris-2 experiments.

Appended on 4/7/06:

A reader of this unit, David Dow, was confused by the paragraph F above. Here is my reply to him: “Dave, you probably confused the Pd, which stands for the rate at which thermal energy is lost by conduction, with the same symbol for the chemical element. A thin invisible layer of foam, suspected to be formed on top of the the liquid during the electrolysis, might decrease conductive losses. Presumably, no such additional layer is formed when the ohmic heater is used. Under such scenario the COP > 1 might be an illusion. I will now make a little change in the unit #286, to avoid another confusion. In fact I will probably add an appendix to that unit, but not today. Thanks for your help."

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287) Social aspects of our controversy

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1) Responding to a comment posted by Charles Baudette, at the 17th anniversary of the famous press release about the discovery of cold fusion, **Steven Krivit** wrote:

I just looked at the very first CF news story from the Financial Times just now. What is interesting to me is that they accurately represent F&P's primary basis (excess heat.) "The two scientists are convinced that they have achieved nuclear fusion, rather than a conventional chemical reaction, *because very large amounts of heat are released* and because some of the expected products of fusion - tritium, neutrons and gamma rays - are formed. Even so, it is not clear what fusion processes are taking place."

Well, I guess this, in itself, is not so profound. What I find profound is that this key actually did appear -- at least once -- but everybody else afterwards missed it and got caught up in the "It sure don't look like fusion to me" mess. You've mentioned this before how the books by Huizenga, Close, etc. totally neglected heat. It seems the identical case for the newspapers too: All the subsequent reporters neglected to mention the heat - at least in a way that represented F&P's perspective. Is this your observation and recollection of the popular press too?

2) **Charles Baudette** responded:

. . . All that the outspoken physicists wanted to talk about was "cold fusion."

1. The subject of "excess heat" would require them to develop some proficiency in calorimetry --- an unpleasant prospect.
2. When the engineering group at NSF (along with EPRI) called a meeting in the fall of 1989, their purpose was to bring together those who claimed success at excess heat generation for a comparison of experimental work and discussion of how to improve the experiment. Huizenga led the charge to claim that those who denied the neutronic evidence for "cold fusion" had not been invited. He converted the meeting (for the press) to one about cold D+D fusion.
3. When these physicists went to ICCF meetings (with J. Bishop in tow), they were looking only for reports of neutron (of a certain energy) emissions. When they did not see that, they would report to the press that there was still no progress in the field.
4. When Morrison reported to the newsgroup science/physics/fusion his take on the ICCF8 (2000) meeting, he mentioned my book. He gave it the title "Why cold fusion prevailed." He did not want the physics community to learn that "excess heat" was continuing to be studied, and he did not want the topic to be broadened from "cold fusion" to "cold fusion research." This is typical of the criticisms of our field.

In discussion with physicists and the press, try to get them to study videos of the original press conference so that, in their criticism, they can get started off on the right foot. Most have a distorted notion of what was announced.

2) **My reply**, on the restricted Internet list for the CMNS researchers:

I agree with Charles; excess heat is one thing and ways of explaining it is another thing. Let qualified theoretical people discuss possible explanations of experimental data, after such data are validated. Premature speculations are tempting and exciting -- but they can be dangerous. It would be better if Fleischmann and Pons did not start speculating about nuclear origin of excess heat prematurely.

3) **On another thread I wrote**:

Michel also asked: "is it ok to speculate about explanations between us people already convinced of the reality of the

excess heat?" Why not? I was referring to our anticipated formal publication of experimental data. In that paper we should avoid theoretical speculations. That would divert discussion from the main issue -- the reliability of experimental facts. Interpretation of facts will appear naturally after such facts are recognize as valid. That would already be a big step forward. We should discuss what happens in the kitchen and learn from what more qualified people have to say in the dining room. We badly need an accepted theory to guide future experiments.

In my opinion the history of cold fusion would have been very different if Fleischmann and Pons have had simply said "we observed excess heat but we do not know where it comes from." That what we should say in a publication describing results from recent Mizuno type experiments [Paris-1, Colorado-2, Paris-2, Texas-2 and (?) Colorado-3], provided we all agree that the measured excess heat is not an illusion (caused by experimental errors, or due to well known processes). Results from experiments now in progress, or in preparation, should be decisive. It will probably take at least several weeks before we are ready to start writing a paper.

4) **Scott Chubb**, responding to the message that started this thread, wrote:

I think the hype that the press conference was shameful evolved from two factors:

1. As opposed to looking at the original sources, with time, members of the press relied upon accounts, provided by "purported experts" and other members of the scientific community.

2. Resentment within the scientific community was wide-spread and almost immediate because F&P called the press conference and went public with their results prior to communicating their results (even within their own department) to the scientific community.

A hidden problem, associated with the second factor, resulted from the use of FAX machines. In particular, David Lindley pointed out that their original paper had an obvious error (associated with the energy of the neutrons) that would have been corrected through the review process. But the review process broke down because their paper, de facto, was published in pre-print form (through FAX machines), and this error was not corrected prior to publication. This further compounded a bad situation. In any case, most scientists, at an early point in the debate, got the impression that by calling the press conference, in the first place, F&P had seriously undermined their own credibility and this action, by itself, was viewed as being shameful.

5) **Jed Rothwell** wrote:

This was outrageous hypocrisy, especially on the part of the plasma fusion researchers. Back then and still today, those people routinely call a press conference after a major experimental run. They ballyhoo their results and declare a breakthrough months before they even write a paper, or publish one. They even go the Congress and demand more funding on the strength of unpublished experiments. As I recall,
some years ago Members of Congress got fed up with this practice and told them to stop it.

Researchers in every field do science by press conference. They always have, and they always will. This is another case of demanding cold fusion meet a standard that no other area of research has ever been called upon to meet. "Skeptics" demand 100% reproducibility, even though that is absurd and no practically other breakthrough has ever been so reproducible (except perhaps the x-ray). They demand commercial products before they accept the reality of the phenomenon. They demand a theoretical explanation, even though nearly every important breakthrough in history has been important precisely because it did not have an explanation. It is all stuff & nonsense.

6) **Scott Chubb** added:

I agree. Complicating the issue, however, was the confusion that resulted because at the time Jones's group "seemed" to be doing the same kind of experiment when (as we know, now), this was not the case at all.

7) **Jed Rothwell** added:

The problem did not begin on May 1, or after the press conference. The problem was inevitable. Martin Fleischmann knew full well that he and Pons would be booted out of the establishment. He knew this long before he made the announcement. See the letter from him quoted in Beaudette's book, page 147:

After the press conference, [Arrhenius's granddaughter] Dr. Caldwell came up to us and said, "Well, when my grandfather proposed electrolytic disassociation he was dismissed from the University. At least that won't happen to you." I said
to her, "But you are entirely mistaken. We shall be dismissed as well."

Scientists have always rejected radical discoveries. The problem has grown worse since the Manhattan Project, which politicized basic research, and put it under the control by Washington. The postwar peer review system has also corrupted the process. The skeptics were pathological all along, and if the press conference or the May 1 meeting had not occurred, they would have seized upon some other event or excuse to reject the findings.

People sometimes blame the name "cold fusion" for the opposition, saying it is was a misrepresentation. Of all the nonsensical objections to cold fusion, this is the most absurd, as any linguist or philosopher knows. There is such a thing as confusing terminology, and technical words are sometimes revised to avoid confusion or standardize vocabulary. But fundamentally, the notion that a word can "misrepresent" something goes back to the bronze age, when people believed language has magical power, and a word embodies the thing it represents. All words in all languages are approximations. Words are tokens. They are often based upon some faulty notion that was current when the thing was discovered. Everyone knows that weather is not caused by meteors but we still call it "meteorology." "Manufacturing" is not work done by hand (manus = hand). It now means just the opposite: work done by machine. But these terms are not inaccurate. "Meteorology" and "manufacturing" mean what we say they mean, and nothing else. Words are nearly always obsolescent, because we have no term when something is first discovered, and we must reach back to our older stock of words. Horses no longer dash in front of the "dashboard," and it is not a wooden board, but we still call it that. We say that computer files are "collected" in "folders" even though nothing folds, and nothing is physically collected.

8) **Scott Chubb:**
Scientists frequently reject radical discoveries, but there are also counter-examples; in particular, in 1987, a radical discovery (High Tc superconductivity) was made, where the system functioned. I would argue that the peer-review process is prone to mistakes in situations involving radical changes. But a major reason for this is Postwar funding (as opposed to the peer review system).

In the case of Cold Fusion, there were many factors that entered that corrupted the process. A subtlety in all of this (that was brought out in the Accountability in Research collection of articles--which, as you know is available at <www.lenr-canr.org>) was
the role of information era technology in disrupting the review process. This (not the postwar review system) was at the heart of the underlying problem. Martin might or might not appreciate this. It was only through talking to Lindley face-to-face and by looking in detail at Dave Nagel's and David Goodstein's contributions to the collection that I realized this.

Dave Nagel's contribution (which I highly recommend) pinpoints, nicely,
the key aspects of the communication process that subsequently broke down (after the press conference and after the 1 May 1989 session). David Goodstein's contribution identifies the turning point (the 1 May session), beyond which the scientific process was doomed to failure. It can be argued that the breakdown was inevitable. But these two articles (by Goodstein and Nagel), and the interview I conducted with Lindley, provide significant evidence that until 1 May 1989, there was a reasonable chance that the process could have continued to function.

An important point to keep in mind is that scientific conferences can seriously help or impair communication. Communication can be seriously impaired especially when information is presented in an unstructured way and the appropriate individuals are not present. This is precisely what happened in the 1 May 1989 session. At that time, although Pons and Fleischmann were not even in attendance, they were accused of carelessness and sloppy work by Lewis, in particular, (as well as by others) at a mass gathering of physicists. This took place in a hostile setting, and neither Pons or Fleischmann had the opportunity to respond. In many respects, the associated ritual can be likened to a

"spiritual event", in which physicists became convinced, as a result of serious broaches in scientific protocol, that purported "experts" (like Lewis), who actually knew basically nothing, had demonstrated that Cold Fusion was bogus.

In fact, the real problem pre-dated both the Press Conference and the APS session. It occurred because of the review process that was adopted by Ryszard Gajewski to evaluate the grant proposal that P&F had submitted to DoE. In particular, Gajewski sent the proposal to Jones to be reviewed. Then, as opposed to forcing Jones and P&F to work together, competition between the two groups resulted. This triggered the Press Conference.

[Jed Wrote:] The skeptics were pathological all along, and if the press conference or the May 1 meeting had not occurred, they would have seized upon some other event or excuse to reject the findings.

I disagree. This is speculation.

[Jed wrote:] People sometimes blame the name "cold fusion" for the opposition, saying it is was a misrepresentation. Of all the nonsensical objections to cold fusion, this is the most absurd, as any linguist or philosopher knows.

Jed, you have not talked to mainstream physicists about this. It may seem absurd, but it is a fact that the name has caused a problem. For them, cold fusion had a meaning, that the effect was the result of a colder form of conventional fusion. Along with this meaning came the assumption that high energy particles, and the conventional products of fusion, would be present. Complicating the fact that this name has caused many physicists to have these preconceptions, in fact, Jones may have very well discovered the kind of "colder version" of fusion that physicists thought was required.

[Jed wrote:] There is such a thing as confusing terminology, and technical words are sometimes revised to avoid confusion or standardize vocabulary.

This is what has happened with cold fusion. I would suggest that the terminology "Condensed Matter Nuclear Science" is considerably more appropriate. I have found that using this terminology has been quite useful in alerting physicists to the fact that they may have been wrong in their assumptions about cold fusion. Technically, Jones's work is closest to the definition that physicists would associate with cold fusion. Pons and Fleischmann's work should also be associated with cold fusion. But I believe that the terminology has been applied inappropriately when it has been used in conjunction with the transmutation work. In particular, I do not believe a single effect is at work in many of these effects and to lump all of them into a single category, called cold fusion, seriously misrepresents the physics behind the underlying phenomena.

[Jed wrote:] But fundamentally, the notion that a word can "misrepresent" something goes back to the bronze age, when people believed language has magical power, and a word embodies the thing it represents. All words in all languages are approximations. Words are tokens. They are often based upon some faulty notion that was current when the thing was discovered. Everyone knows that weather is not caused by meteors but we still call it "meteorology." "Manufacturing" is not work done by hand (manus = hand). It now means just the opposite: work done by machine. But these terms are not inaccurate. "Meteorology" and "manufacturing" mean what we say they mean, and nothing else. Words are nearly always obsolescent, because we have no term when something is first discovered, and we must reach back to our older stock of words. Horses no longer dash in front of the "dashboard," and it is not a wooden board, but we still call it that. We say that computer files are "collected" in "folders" even though nothing folds, and nothing is physically collected.

These are all interesting points. Eventually, the terminology "cold fusion" might stick. For now, I think it should be abandoned, and the terminology "Condensed Matter Nuclear Science" should be used, at least in situations involving mainstream scientists.

9) **Jed Rothwell**:

One of the lessons of history is that when extremists make threats, denounce people, and when they call you a fraud and a criminal, you should take them seriously. They mean what they say. Such people do not kid, and they seldom

exaggerate for effect. The Nazis really did intend to exterminate the Jews. The extreme Islamic fascists really do want to impose Sharia law on the US and all other nations. They flew airplanes into buildings, they killed Salman Rushdie's Japanese translator, and given half a chance they would kill the Danish cartoonists. The professors who brought legal proceedings against Taleyarkhan intend to shred his reputation and crush his research. They will stop at nothing. They do not consider themselves unethical. They see themselves as the defenders of science against the rabble. They would be happy to see Taleyarkhan serve time in jail, if it came to that. They will make any accusation that sticks, including larceny. Similar people tried to destroy Bockris with these tactics. If they could, they would put cold fusion researchers in jail too. They have told me this and I have no reason to doubt they mean it.

Some people have suggested that Robert Park may be softening his stand. This is nonsense. It is dangerous nonsense if you fall for it, or lower your guard against him. Here is what Park wrote in 1996, in a Washington Post review of Close's book: "Close asks in the first chapter, 'Was this a delusion, an error, or a fraud?' By the end of the book, it is clear that cold fusion progressed through all three. What began as wishful interpretations of sloppy and incomplete experiments ended with altered data, suppression of contradictory evidence and deliberate obfuscation.

After a while, the objective seems to have been just to prolong the inevitable--but Fleischmann and Pons were no longer alone. Inept scientists whose reputations would be tarnished, greedy administrators who had involved their institutions, gullible politicians who had squandered the taxpayers' dollars, lazy journalists who had accepted every press release at face value--all now had an interest in making it appear that the issue had not been settled. Their easy corruption was one of the most chilling aspects of this sad comedy. To be sure, there are true believers among the cold-fusion acolytes, just as there are sincere scientists who believe in psychokinesis, flying saucers, creationism andthe Chicago Cubs. The lesson from "Too Hot to Handle" is that a PhD in science is not an inoculation againstfoolishness--or mendacity."

He repeated the gist of these comments in 2002. I have not read a single word by him that hints he has had a second thought or a moment's doubt. If you wonder about this, ask him. If he bothers to respond he will probably tell you that he sticks to every word. He has never been reticent about expressing his views.

I know of only one hard-core, anti-cold fusion campaigner who does not frankly own up to his views, and does not respond to such inquiries: Steve Jones. As you saw the other day when I asked him whether he really believes the excess heat results, he cuts and runs. He evades the issue. I suppose he is trying to play both sides and build up support among cold fusion researchers, as well among the people who despise cold fusion. I cannot imagine why he thinks he needs help from us, since we have no political power, funding or influence. Based on all of his published papers and on countless conversations with him, I am certain he does not believe the heat is beyond the limits of chemistry (anomalous). He may want to give the impression that he is willing to examine that idea and take it seriously, but I am sure he cannot bring himself to do that. For one thing, he has not published a single word admitting that might be the case. For another, if he were to confront the truth he would have to admit that Fleischmann and Pons were right from the beginning, and he was wrong, and this has been obvious all along. I doubt he is capable of admitting he made such a drastic mistake.

10) **Ludwik Kowalski** (not posted):
Jed has been trained in humanities and in linguistics. But he is very knowledgeable in many areas of CMNS (condensed matter nuclear science is the new name for what used to be known under the name of cold fusion). Scott, on the other hand, is a theoretical physicist . Each of them believes in reality of excess heat and in its nuclear origin. I suspect the the friendly exchanges of points of view between Jed and Scott will continue. But I have to stop somewhere. There were one more message from Jed, one more from Scott and an additional one from Jed. All of them are interesting. But my goal, in composing this unit, was to show what some CMNS researchers think about social aspect of the 17-years controversy.

11) More from **Jed Rothwell:**

The people who oppose cold fusion went on record within days of the announcement. At MIT they held a party to "celebrate the death of cold fusion." Professors called for the arrest and imprisonment of Fleischmann and Pons for fraud. The US secretary of energy said he could tell cold fusion is wrong because he saw Fleischmann on television, and

"Fleischmann looked stupid." These people never gave cold fusion two seconds of consideration. To this day, they have never looked at any evidence. They are not shy about their views. The editor of the Scientific American brags that he has never looked at a paper.

Whenever anyone mentions cold fusion they assert with unshakable confidence that all cold fusion researchers are frauds or lunatics, all are a disgrace to academia, and should all be hounded, ridiculed and fired -- tenure are not. They are as sure we are wrong as I am sure the Flat Earth society is wrong. Perhaps they do not say this to you in person but that is what they say elsewhere, and their actions prove that is what they think. They are not mincing words, kidding, or exaggerating their own views, so you should believe them. There was never any question that these people are bitterly opposed and they will fight to the end.

12) More from **Scott Chubb**:
It is sad to say that people do pay attention to stature and background. So, I don't doubt that in many cases, particular editors might say one thing to you and something else to me, simply because I am a mainstream scientist and you are not.

[Jed wrote:] There was never any question that these
people are bitterly opposed and they will fight to the end.

Many people have become bitterly opposed to the subject. But in most cases, this is because they either have not formed their own opinions (and have based their opinions on the opinions of "experts"), or they have not bothered to learn the facts.

13) Another quote from **Jed Rothwell**:

Scott, I think you gravely underestimate the opposition. You do not understand the severity of the problem. You seem to think these are reasonable people who mainly "fail to understand." Perhaps they are polite to you when you meet at a conference. I have met Huizenga, Park and and Morrison, and I was polite to them as a matter of course. I would consider it beneath my dignity to insult them in public. I would not want them to think they have got my goat.

These are not nice people who have made some sort of miscalculation or forgot to read a paper. It is not as if they have been meaning to do some fact checking but it has slipped their mind every month for the past 17 years. They have made their goals and their intentions crystal clear, time after time, in statements to the press and to me
and to others. They despise you. They do not believe a word you say. They think you are a crazy, lying, criminal fraud. They do not give a fig about academic standards or freedom or fairness -- they say such standards do not apply to lunatics. Their goal is to destroy you and they will stop at nothing to accomplish this goal.

If you do not think these people hate you, ask yourself: Why do they keep shouting that they hate you?!? Why do they take legal action against Taleyarkhan? What more will it take to convince you? Do they have to come and burn your house down?

Seriously, you might as well doubt the intentions of Bin Laden. You might as well wonder whether he hates you and wants to do you harm. Yes, he does. People are not all sensible, kind and reasonable. Our dispute with Bin Laden is not caused by a failure to communicate, or by good intentions gone awry. Bin Laden is communicating loud and clear, and there is no doubt what he wants to do. He has repeatedly declared that his goal is to use a nuclear bomb against a U.S. city. For months he has been asking the leading Muslim radical imams for a fatwa (permission) to proceed. They say fine, go ahead, as long as he kills fewer than 10 million people per attack. (According the ex-CIA section chief who wrote "Imperial Hubris.") So if a stolen Russian bomb goes off, will you be surprised?

Our professional journalist, **Steve Krivit**, who triggered this thread wrote:

Jed and Scott, You are both have logical and correct-sounding viewpoints. It seems that you're just looking at it from different perspectives. And insightful ones, each.

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288) CMNS researchers discuss chemistry

Ludwik Kowalski; 3/29/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

1) This evening I posted this message: "Dear Francesco [Celani]: Suppose a Pt cathode produces the COP=1.00 while a W cathode produces the COP=1.30, in a Muzino type cell, at the same voltage. Would this not be a sufficient proof that the excess energy is real when a W cathode is used? I think that this would be a valid argument against your suspicion that the inductance of the capacitor might be causing an error in the COP measurements.

It seems that the chemical origin of excess heat, in Mizuno type cells, becomes the central issue. I hope somebody can summarize what has been posted about that topic so far. The K2CO3 disappears when the salt is dissolved in water; it is replaced by + and - ions. An experiment begins when the electrolyte temperature reaches a constant temperature ~100 C. The K2CO3 no longer exists; it cannot be a fuel. If there is a fuel then it consists of ions. Which reactions are dominate and how many joules of energy can these reactions possibly deliver, per gram of the initially dissolved potassium carbonate? Significance of Paris-1 and Colorado-2 results depends on how is this question answered by chemists."

Fauvarque is an electrochemist and I expect that he will write an appendix to our anticipated publication (see unit #271). Pierre, who works in his lab, took back a bottle with the electrolyte we used in the Colorado-2 experiment. Let me gather messages that people wrote about the possibility of chemical origin of excess heat. Even Jed Rothwell admitted that the excess heat in Mizuno type experiments (plasma electrolysis) might have chemical origin. Several days ago he wrote: ". . . but this open cell glow discharge is quite different, and one cannot as easily rule out conventional explanations." In other words it is possible that some chemical reactions are responsible for what was interpreted, prematurely, (by Mizuno, Naudin, Fauvarque and Iorio as liberation of nuclear energy. High voltage plasma electrolysis is very different from the low voltage electrolysis used by most CMNS researchers.

2) On Michel Jullian asked:

"The mass loss corresponding to this large quantity of liberated O2 and H2 is attributed wrongly to evaporation isn't it? Has anybody worked out how significant this effect is, and which way (under or overestimation?) it affects output energy estimation? Responses would be appreciated."
P.S. I forgot to include an earlier important observation made by X1. He wrote: I suggest that exothermic chemical reactions involving the electrolyte salt or its ions could be releasing heat in glow discharge experiments, on the bases that it is the only possible chemical reactant present in sufficient quantity (tens of grams) to explain observed excess heat, and that the color of the plasma "flame" depends on the nature of salt used.

3) The immediate reply from X2 was:
"First, the report of 80 times Faradaic efficiency for dissociation of H2O comes, I believe, only from Mizuno et al. The literature on Contact Glow Discharge Electrolysis (i.e. a section in the big multi-volume electrochemistry compendium edited by Bockris) says that up to 8 times has been reported. In our own experiments (http://www.earthtech.org/experiments/Inc-W/2ndtry/run6.html) we have observed only 2.5 times the Faradaic efficiency. I will use our observation in the calculations below.

Ignoring the fact that some mass is lost due to dissociation of H2O into H2 and O2 results in an underestimation of the heat energy produced by the cell. The error is around 1% for the cells I have run. Consider a cell running at 300 volts and 1 amp. Pin = 300 watts. The fraction of this power that goes into electrolysis is 1.48 volts (the thermoneutral dissociation potential) * 1 amp * 2.5, the latter factor from our own observations. So Pdissoc = 3.7 watts. That leaves 296.3 watts for boiling the water. Assuming no excess heat, if you measured the mass loss correctly and came up with 296.3 watts of heat power, then compared it to the input power of 300 watts, you would conclude that the process is only 98.8% efficient.

If the Faradaic efficiency did go as high as 80, things would be very different. Then Pdissoc would be 118.4 watts, leaving only 181.6 watts for boiling. Using mass-loss calorimetry the process would appear only 60.5% efficient." Let my mention that, according to Mizuno, the amount of H2 produced in plasma electrolysis is often much higher than what is expected by the Faraday's law of electrolysis, for example, 80 times higher. Hydrogen is fuel and its excess must be counted as excess energy.

4) Ludwik Kowalski wrote:
Here is a little rough calculation worth sharing. In our typical 5 minutes test (input electric energy = 400 W * 0.0833 h = 33.3 Wh) the amount of excess heat is about 6.5 w*h (if the COP=1.2). This translates into 1.46*10^23 eV. The estimated mass of tungsten lost during the same time is about 0.5 grams. That translates into 1.63*10^21 atoms. Thus the excess heat per atom of tungsten seems to be close to 100 eV. That is about two orders of magnitude larger than what I associate with common fuels.

What happens to our spent tungsten? Part of it probably melts and then solidifies in the electrolyte. If so then pure tungsten must be found in the cell (in the deposits we see at the bottom, after some waiting). Suppose that 50 % melts and the rest combines with other atoms or ions. Not being a chemist I might be allowed to postulate that all possible chemical reactions are equivalent to one representative exothermic reaction. Suppose that our excess heat is due to that reaction. What would the Q value of the representative reaction be? It would be 178 eV. That is a lot !

The Q value would be two times larger if only 25 % of tungsten was used in chemical reactions while the rest was simply melted. This shows that Q increases when more tungsten is melted. At the other extreme the Q value would be 89 eV if nothing was melted (all consumed in exothermic chemical reactions). Even this number is unusually high. Did I make a wrong assumption, or a numerical mistake, somewhere? . . . Tungsten electrodes should be weighted before and after individual tests. Fortunately, a possible 50% error (in the estimated mass lost) would still support the conclusion -- too much energy per atom of tungsten.

I wish our real chemists, such as R. O. or Mike McKubre. and his team, were performing Mizuno type experiments. Confirmations made by them would be much more significant than by amateurs, like myself. I would be in a much better position to argue about nuclear particles and nuclear detectors than I am in arguing about chemical reactions. That is because I was trained as a nuclear physicist.

5) M. Jullian asked:
" Dissolving solid K2CO3 in water is an endothermic reaction isn't it? (absorbs heat)

6) X2 responded:
My old 1948 "rubber bible" says it is exothermic. ....6.5 kcal is released when 1 mole of K2CO3 is dissolved in 400 moles of water.

7) X6 wrote:
Whilst it is perfectly true that atomic hydrogen can give off lots of heat on reforming H2 (precisely 4.5 eV per molecule) this same energy is required to form it in the first place. Consequently any such reaction would produce no net energy.

8) M. Jullian wrote:
[Somebody asked] "As for hypothetical reactions with the electrolyte, what could they be?" I don't know. Whatever the high temperatures and the active species in the plasma can do to it. If you bring material past a plasma, like the

electrolyte is brought past the plasma sheath around the cathode in a Mizuno cell, it will be processed happily and non reversibly. This phenomenon is called plasma processing, used to e.g. oxidize the material to be processed, exhaust gases, plastic film etc.

In any case it is beyond doubt that the electrolyte intervenes in the plasma reactions, have a look at all the beautiful different plasma colors one can get by trying different electrolytes

<:http://www.geocities.com/spfaile/plasma/Plasma.html>

Note that the above excellent experiment report (couldn't find authors names in LENR-CANR library) also confirms that initiating a plasma is a matter of electric field, not of voltage. They managed to generate plasmas with only 50V, using fine enough cathodes (thin wires, or sword shaped sharp plates). It was pointed out by Jed recently that Mizuno obtained the right kind of plasma at lower voltages than the 200-400V used in recent experiments, I submit that is because the lower radii of curvature of Mizuno's thin W plates' edges induced the required electric field for a lower voltage than with the relatively thick welding rods used in recent experiments.

Back to chemistry, my suspicion as a non-chemist familiar with only the _physical_ aspects of plasmas, is that the field effect ionization effect giving birth to the plasma, combined with the >3500°C temperature of melting tungsten, may be able to break the electrolyte into more active potassium species able to react exothermically with oxygen and/or water with a positive net energy balance.

But there is no need to know exactly what such reactions could be for now or if they are plausible, let's first check the hypothesis experimentally as discussed with Ludwik: is the electrolyte consumed at all in a _successful_ run? If not we can happily eliminate the electrolyte as a possible chemical fuel. Will remain your solvent hypothesis :)

9) Mike McKubre responded:
I have seen this thread bouncing around and hoped that there was a subtle meaning that I was not getting. I may have missed the point but let me address the obvious. Potassium in the electrolyte is in the oxidized state (K+) while oxygen is in its reduced state (O--). If by some means potassium becomes reduced to K and oxygen oxidized to O2, and they somehow are allowed to react, the results is oxidized K and reduced O --- exactly as we started with. Thermodynamics has the wonderful virtue of being promiscuous --- it is completely insensitive to pathway. If the initial and final states are the same --- then no energy is absorbed or released. I am hoping that this is not the theme of this thread. If it is, then let this answer be an end.

10) John Coviello wrote:
. . . Of course I agree with your obvious assessment but would like to include consideration of the possibility that the cell ingredients would undergo reactions that would result in different final compounds. For example, the plasma might stimulate the reaction

$K_2CO_3 + H_2O \rightarrow 2KOH + CO_2$

The CO_2 would leave the cell as a gas. Adding up the heats of formation and solution of these compounds, this looks like a slightly endothermic reaction..... about 25 kcal per mole of K_2CO_3 converted. Compare this to 275 kcal/mole for the formation of K_2CO_3 from its elements. I suppose it is safe to assume that any "rearranging" reaction, i.e. one that leaves the metals oxidized and the oxidizers reduced will not have a great energy production or consumption.

Regarding the W cathode, I have examined this reaction:

$W + 4H_2O \rightarrow H_2WO_4 + 3H_2$

for which we presented evidence in this report:

http://www.earthtech.org/experiments/Inc-W/2ndtry/run6.html

and found it to be almost non-thermic. The energy required to dissociate 4 moles of H2O is about 274 kcal and the energy released when the H2WO4 is formed is 280 kcal. Plus, the total mass of W consumed in a typical run is quite small. In conclusion, it appears that there is no possibility that chemical energy could be the source of the apparent excess heat from the W-K2CO3-H2O system.

11) My own reply to Mike McKubre
Thanks, your comments, concerning the ongoing Mizuno type experiments, are highly desirable. Most of us performing, or planing to perform, these experiments, are not even chemists. The only way to compensate for this handicap would be to receive critical input, and help, from real electrochemists, like yourself, E. Storms, R. Oriani, S. Szpak, etc. Please help us to confirm reality of excess heat from plasma electrolysis cells.

12) Michael McKubre responding to X2:
The first reaction is endothermic as you state. In fact it is the bane of electrochemists running KOH electrolytes as it spontaneously reacts with CO2 from the air to form carbonate. I did not look up the numbers but your assessment of the W + H2O reaction seems right, and I agree with your general conclusion. For non chemists the heats of formation of all these materials are easily looked up and added up. You can answer these questions yourself, easily. In fact, thermodynamics is trivial - like nuclear physics. A few simple rules and a lookup table.

13) M. Jullian wrote:
Thanks M.McKubre Assuming the first reaction proposed by X2 occurs in a significant manner, we will have a significant mass loss via CO2 won't we? Since this mass loss will be wrongly attributed to evaporation, we will have to make sure each gram lost this way takes away as much heat, or more heat, than a gram of evaporated water. Haven't time to do the computation right now, maybe someone will beat me to it.

Ok so X2's first reaction K2CO3 + H2O + heat -> 2KOH + CO2 (well
spotted X2 BTW, you made me look less foolish with my speculation of electrolyte chemistry ;) is endothermic, requiring about 25 kcal per mole of K2CO3. That's 25 kcal per mole of escaped CO2, i.e.25000*4.186 = 105000 J for 12+16*2 = 44 g of lost mass.

This works out as 2380 J/g, which seems to be slightly higher than H2O evaporation heat 2260J/g, but it would be worth double-checking the exact figure for the reaction heat (anywhere online where the formation heats you mentioned can be looked up?)

Pending confirmation of the figure, as far as this reaction is concerned we can happily keep counting all lost mass as evaporated water as this amounts to underestimating the output energy and the COP (it would have been bad if it had overestimated it!). But this result needed to be justified didn't it?

Since the outcome of the calculation had no particular reason to be favorable, I still think it would be a good idea to analyze the electrolyte after a successful run, to check that no other unexpected chemical reaction involving the electrolyte has occurred to mess up our results one way or another.

Note such analysis can be tricky though, as for example the above reaction could not be detected if the electrolyte was left to cool down in ambient air long enough: as Michael taught us the KOH would reabsorb the CO2 from the atmosphere and return to its initial K2CO3 composition. . . . .

Er... group, how about this new silly speculation about chemical heat from the electrolyte? (very basic this time, would probably also escape post-run chemical analysis depending on how the electrolyte is collected) Dissolving solid K2CO3 in water is an endothermic reaction isn't it? (absorbs heat)

So some electrolyte drying up on the cell walls after some splashing, the reverse reaction, would generate heat wouldn't it? Real heat this time, not an artifact of the measurement method. This effect could be far from negligible but it's getting quite late over here so I'll go to bed and ask a charitable soul to do the homework this time: - How many joules of excess heat per gram of dried-up salt on the cell walls? - How many grams end up this way, experimenters? - Couldn't this also affect P&F cells to a lesser extent?

14) X2 wrote:
You asked "Dissolving solid K2CO3 in water is an endothermic reaction isn't it?" My old 1948 "rubber bible" says it is exothermic..... 6.5 kcal is released when 1 mole of K2CO3 is dissolved in 400 moles of water.

15) M. Jullian wrote:
Thanks X2, my mistake, and a very good news: again the outcome is favorable! I submit that a thorough paper on the subject would mention all the chemical reactions that have been thought of, and why they can only underestimate the COP or make it much lower than measured: - Faraday and thermal dissociation of water (underestimates COP) - reactions involving the tungsten (100 times not enough mass to explain the COP) - KOH and CO2 formation from K2CO3 (underestimates COP) - dried up electrolyte (underestimates COP) - ???

Other reactions with the electrolyte might be possible, is there any way at all to predict all possible chemical reactions that can occur in a given system? Analyzing the electrolyte before and after a successful run may reveal reactions we might have missed, and hopefully will give more consistence to the far from obvious claim that excess heat is not chemical in Mizuno experiments.

Such chemical analysis would also quantify the lost droplets/mist phenomenon as has been discussed: just saying it is negligible because some filtering means is used, or because no mist is seen at the surface of the covered cell on the photo (if we "see" water vapor in a covered cell it is not water vapor but means there is some nebulization occurring) may not be sufficient proof.

In any case the dream I had of a simple cell demonstrating reality of cold fusion to any skeptic in a few minutes is getting more remote every time a new artifact is found and ruled out. There is stronger and stronger hope that a simple cell (insulated and covered Fauvarque) could be sufficient proof, but it will also take a lot of reading!!! Unless of course it produces significant excess heat for days and months, but we are not there yet.

16) M. Jullian also responding to what I wrote in (1):
X has just responded (he may be right about ceramic being better than plastic). My own rantings are not about the reality of electric power measurements either (like all or most people here I now have a high level of confidence that they are valid) but about their indisputability from the point of view of a reviewer.

About the COP depending on the cathode material, it would be an interesting result in itself and would certainly increase the level of confidence of the measurement, but would not yield indisputability, why should it?

As I have been stressing, no electrical measurement made in a high EMI environment will ever be indisputable. Please let us never forget that skeptic or even neutral reviewers will need EXTRAORDINARY PROOF for our extraordinary claims. Any reason to doubt, especially a good one such as abundant EMI, will be jumped upon to dismiss the whole stuff, and HAS BEEN actually. Simplicity, transparency, indisputability, should be maximized if we want to stop being considered as self deluding wishful thinkers, agreed?

Talking about EMI, it occurred to me that it might be a good idea if the anode itself, which could easily be made to surround entirely the discharge area (including top and bottom), could be used as a Faraday cage. This would mean connecting it to ground and having the cathode float below ground, Scott Francesco and all what is your opinion on such an arrangement?

P.S. About the chemical effects discussion, I would find it outrageous if the real chemists here did not get more actively involved in this, leaving amateurs such as Scott or myself do their homework as they have effectively done up to now:

1/ What are the possible reactions apart from the ones discovered (by chance more or less) by Scott and by myself?

2/ What can happen or not happen chemically speaking in the plasma at temperatures ranging at least from 100°C (outside of plasma sheath) to melting tungsten temperature (inside), and at high electric fields? Any missing

parameters to answer this question? Why does the plasma color depend on the electrolyte? Real chemists please ENLIGHTEN US (if you have any clue that is).

17) Peter Gluck, who is a chemist, wrote:
. . . From the point of view of engineering, we need first of all, complete ENERGY and MATERIAL BALANCES. Has somebody a complete material balance for all the phases? Otherwise unknown data plus hidden parameters put any positive results in doubt; there more unknowns than equations of balance.

SOLID- e.g. W electrode at start + ? --> electrode at finish + solid precipitate (composition?) I have told that my guess is that W is destroyed by spark erosion (as does Zr in the Cincinnati Cell) i.e. it does not react. This need a bit of analytical chemistry, however a simple balance: initial weight of the electrode vs final weight of electrode plus the **washed and dried** precipitate can tell something about the fate of W- is it broken, is it eroded, is it reduced, is it burn?

LIQUID- initial K2CO3 vs final K2CO3 plus KOH, K balance, CO3 minus 2 balance alternatively dissolved carbon can be measured

GAS- the most difficult part and the most critical, I dare to say! Energy is consumed in order to get gases; Gases are coming out the cell and add to the weight loss;
Extremely hot gases are bubbling through the liquid that is already close to the boiling point and get saturated (supersaturated?) with water vapors. Was the volume/weight & composition of gases plus the weight of the water after condensation and separation measured?

By the way is there a connection between the depth of immersion of the electrodes and COP? This is only an ad-hoc sketch of the chemical aspects of the material balance in these experiment.

18) John Coviello added:
. . . I don't think anyone would find anything exciting about what is going on in these cells if the heat measurements fell within the ranges of chemical reactions.

19) Ludwik Kowalski observed:
I am surprised that neither Mizuno nor Fauvarque, who are electrochemists, presented a complete discussion outlined by Peter Gluck. Why didn't they do this? I am less surprised by the absence of such discussion on Naudin's website; as far as I know he is not a chemist. Naming his device CFR (cold fusion reactor) he showed what he believes in nuclear origin of excess heat in Mizuno type cells. We have no evidence that the amount of "nuclear ashes," discovered by Mizuno, matches the amount of excess energy, as it seems to the case in some Fleischmann type cells. Fauvarque et al, by the way, also refer to their setup as CFR.

20) M. Jullian responded:
Ludwik: the possibility of such chemical reactions may have escaped Mizuno or Fauvarque. X4 you wrote: "Isn't that exactly what Ed Storms and Mike McKubre have been doing on other threads regarding the Colorado replications regarding questions raised about the amount of heat produced via chemical reactions?" The short answer is no. Mike first implied that no chemical reaction with the K2CO3 electrolyte can affect excess heat. Then two effects with the K2CO3 which do affect excess heat or it's measurement by evaporated mass were found by non-chemists, one by X2 and one by myself, fortunately it was worked out that they can only lead to underestimating excess heat, and only slightly, which is not a problem.

My questions were about the possible reactions . . . Of course Mizuno excess heat would not be exciting if it could have a chemical origin, and the fact that two hitherto unthought of reactions REDUCING apparent excess heat have suddenly sprung up suggests that there might be more waiting to be discovered, and there is no guarantee they won't CONTRIBUTE to excess heat. It's awfully hot in the plasma, and it contains hydrogen, oxygen, carbon, and potassium, in various combinations, probably all ionized by high electric fields and temperature and in highly reactive forms. Can we be sure no reaction affecting excess heat can occur in this furnace, other than the ones mentioned? We count on chemists to suggest a detailed protocol to ascertain this.

21) J. Coviello wrote:
I agree with the spirit of your inquiry. There is no reason why we should just accept the excess heat measurements without considering all the possible causes for the observed excess heat and the potential that the excess heat measurements are the result of error. It is correct to explore alternative explanations and only when those have been exhausted to entertain the idea that the excess heat is of nuclear origin.

People like Mike McKubre and Ed Storms have been working on cold fusion for over a decade (or longer) and they have certainly considered at times whether the heat is of chemical origin or not. The arguments I've read against the heat being of chemical origin is the simple fact that the observed excess heat is just too high to be the result of chemical reactions. If a chemical reaction has the potential to cause a 5 milliwatt increase in heat in a cell and the observations are in the 20 to 50 milliwatt range for excess heat, then the chemical explanation quickly falls by the wayside. I believe you are reinventing the wheel here, as the chemical explanation has been considered and dismissed many times over.

The one thing cold fusion research really needs at this point is a larger experimental setup, one capable of outputting heat in the watt range consistently. That would put chemical explanations and error questions to rest forever. I'm not sure what exactly is preventing scaling up cold fusion cells, besides perhaps simple economics, there isn't all that much money for cold fusion research. Whatever money has been available, such as the SRI work funded by EPRI that Mr. McKubre performed, was spent on basic research just to determine if the cold fusion phenomenon is real and is worth pursuing. Now that it has been established as real, we need massive funding for research efforts that determine the optimum operating environment to produce the cold fusion reaction and to scale the reaction up to levels that might be useful for energy production.

22) Ludwik Kowalski wrote:
X4 wrote: "People like Mike McKubre and Ed Storms have been working on cold fusion for over a decade (or longer) and they have certainly considered at times whether the heat is of chemical origin or not."
That is true. But I do not remember them analyzing chemical reactions in Mizuno type cells (along the lines suggested by Peter Gluck). These cells are very different from Fleischmann type cells that were used by most CF researchers. But Mike, Ed, P. Boss, S. Szpak and R. Oriani are electrochemists; their opinion would mean much more than opinion of numerous amateurs. I suspect that our experts have had reservations about Mizuno type cells but preferred to remain salient. Was it because the chemical environment is too complex or because they decided not to create a conflict in our own "army?"

23) P. Gluck wrote:
Dear X1, Excuse me please for adding that some physical effects have to be also considered. An example: The solution has a concentration of 0.2m K2CO3 i.e. also 0.2 m in CO2. If the CO2 is released in the very hot reaction zone as CO2 gas, that is 4.5 liters in normal conditions (1 mol is 22.41 liters). The conditions are far from normal, it is very hot there! If CO2 is released as a very hot gas, it mixes with the also hot hydrogen and oxygen, the gases are bubbling through hot water and get charged (saturated or not, it is a problem of kinetics) with water vapor -- and this leads to extra weight losses. Therefore such problems as: -- is K2CO3 decarbonated during the tests? -- what is the total volume of gases during an experiment and can it be correlated with COP? have to be considered too.

24) Richard Slaughter wrote:
The reason I decided to try the Glow Discharge experiment was that it did produce excess heat in the 10 to 100 watt range or 10 to 30% of input power. [That is much more than fractions of watt, as in many experiments involving the Fleischmann type cells.] I'm not a chemist but it seems to me that we should be trying to figure out what remains chemical after a successful experiment.

I can think of several test we should be doing. I'm sure the chemist can add several more. Find out if there has been a change of mass in the system. If all we are doing is evaporating H20 the total mass should be constant. Find out what amount of K2C03 still remains. Find out how much pure W is still the beaker and how much has gone into solution. Find out what compound is holding the W in solution

If we answer these question don't we have a complete picture of the energy balance of the system? Do we really need

to know the complex multistage relations that might have happen to reach this point? With this data can't we now calculate energy production per/atom and show that this production is outside normal chemical reactions and a potential energy source? Chemist, is there an inexpensive way to answer these questions?

25) Ludwik Kowalski wrote:
I think that this is good way to begin a discussion of "Mizuno cell chemistry for dummies." Suppose the water of used electrolyte is totally evaporated after an experiment. Then the recreated K2CO3 should be extracted from the deposit. No, I am not going to this; I do not know how to separate the K2CO3 from the remaining solid deposit. But a chemist can do this; I am sure Richard will be happy to send used electrolyte to a chemist willing to help us. Suppose that nearly 100% of the initial K2CO3 is recovered. This would show that no mass was lost in the form of the escaping CO2. The other extreme would be total absence of K2CO3 in the deposit. That would mean that concentration of the electrolyte was not constant in our experiments. Does this suggestion makes sense? I do not know which ions are formed when pure K2CO3 is initially dissolved in distilled water.

26) M. Jullian responded:
You wrote “no mass was lost in the form of the escaping CO2.” Not necessarily, as I explained in a recent post: “Note such analysis can be tricky though, as for example the above reaction could not be detected if the electrolyte was left to cool down in ambient air long enough: as Michael taught us the KOH would reabsorb the CO2 from the atmosphere and return to its initial K2CO3 composition."

27) Jed Rothwell wrote:
Ohmori and Mizuno are electrochemists, and they have done very thorough investigations of the cell chemistry.

28) Ludwik Kowalski wrote:
I do not recall anything of that kind in their papers downloaded from our <www.lenr-canr.org> library. It would be very useful if somebody who has access to Japanese papers, and who knows Japanese, could translate for us their analysis of the bulk chemistry in the hot plasma environment. In the context of this debate the bulk chemistry seems to more important than chemical impurities that might be confused with products of transmutations.

27) Mike McKubre wrote:
. . . My position is that the magnitude of the excess energy reported in this thread (and elsewhere) from this class of experiment (as others) is far too large to be chemical in origin. Actually I have been saying this for nearly 17 years. This is a matter of eV vs. MeV. It is a matter of quantification - something I see too little of in this somewhat airy discussion. . . . The Mizuno effect is of interest to me, but not one of my primary foci. In my view there are also far too many posts. I am a little disturbed by the new tone of Chemist/Physicist specialization. If you are going to do experiments or discuss them in this field you need to develop competence in both areas. They are not so different, or difficult. Do not retreat behind your ignorance or ask others to train you or do your work for you. . .

Appended on 4/1/06

28) Peter Gluck wrote:
I think that material and energy balances based on measurements, chemical analyses and search for cause-effect are necessary in order to distinguish between successful (say, COP>1.20) and failed experiments. Without balances for the solid, liquid and gas phases we will not be able to understand what happens and to know if the effect is real - and adequate for intensification and scale-up. The problem is that we have a **metastable system --** plasma, extremely hot gases, boiling water, great temperature gradients etc., and in these systems very small changes can have disproportionately great effects -e.g. fast evaporation, loss of mass. We have to establish if the difference between the successful and failed experiments is: - **thermodynamic** i.e. a real heat effect/excess - **kinetic** - in this case we need further investigation to decide if the effect is real.

29) Peter van Noorden wrote:
I want to stress that it is very important to look for the amount of non evaporated water/ evaporated water during the contact glow discharge experiments, because I think this can have a big influence the energy calculations of the cell.

As I earlier mentioned I did a lot of experiments with a system which has a long vertical condensor (about 1 meter). I used thallium chloride as an electrolyte to study remediation of radioactivity. I did not expect to see any electrolyte in the condensor flask but when I used radioactive thallium 201 I measured about 15-20% of the original activity in the condensor solution. This was very surprising so I assumed that 15-20% of the solution is carried away by (non evaporated) droplets. Further I noticed that during gentle electrolysis the thallium concentrated on the kathode surface (which is ofcourse normal), but as soon as the glow discharge started ,the thallium was rapidly expelled from the electrode and could be found at the bottom of the cell attached to the W debris. Probably this was the result of fast corrosion of the W electrode.

30) Ludwik Kowalski (not posted):
That is a very significant contribution. The COP close to 1.2, that we measured in Colorado-2 experiment would have to be reduced to 1.0 if only 80% of the water were known to be lost via vaporization and 20% in the form of tiny drops.

31) Johnny Coviello wrote:
I came across this critique [by Alexi Tekhasski] of the Mizuno evaporation measurements (see below) . The writer is saying that Mizuno and others who have found positive excess heat results from his experimental design are not accounting for liquid that is lost by the glow discharge cell due to mist created during the boiling process when bubbles violently break the surface of the liquid. The critic is saying that Mizuno and others are assuming that all the missing water has evaporated, and they are not accounting for mist created during the boiling process, which would reduce the amount actually evaporated during the experiment. I assume that Mizuno and others took this variable into consideration in their calculations, but this person says otherwise.

32) M. Jullian wrote:
Indeed. Mist becomes a central question obviously, it will have to be elucidated in a very definitive way, and I don't think an open cell will allow solving the controversy. . . .

33) Peter van Noorden:
This contact glow discharge reaction is very violent at 350V so you have a lot of mist which escapes from the electrolyte surface and is forced through the condensor..... The fact that the radioactivity is transported through the tubing (1 m high) and into the condensor vessel made me conclude that the mist above the electrolyte must be a combination of vapour and droplets

34) M. Jullian wrote:
Yes you are righ, it must be a combination. Indeed there is no way to tell condensing vapor from mist as they are the very same thing: liquid droplets. . . .

35) Peter Gluck wrote:
. . . The Peter's experiment shows you that the very hot gases are very fast too and they carry microdoplets of solution at relatively great distances in fragments of a second..... I made Mizuno type experiments - very preliminary, with stainless steel electrodes (in Gene Mallove's lab in 1998) and have observed that above the plasma zone there is splashing and it is very hot. But have not measured HOW hot and this is an important unknown parameter. We also do not have the slightest idea now, if potassium carbonate is converted in hydroxide and in which extent.That is we have CO2 too.

I am an engineer (chemical, bien sur) and I am not very fond of experiments in which the electrodes are destroyed so fast. (the Cinci Cell has the same fatal flaw), therefore I have not continued this work. The system is not so simple as it seems and therefore the new experiments have to be planned carefully. In case the group needs my know-how, I am glad to help. Please read what I wrote re. the material balance.

36) Jed Rothwell wrote:
Look up "wet steam" and "dry steam" and you will see that engineers have been dealing with the enthalpy of steam and the issue of moisture in steam since the late 18th century. Measuring enthalpy by weight loss alone is a crude

method, as I said here several times. Perhaps it proves there is excess heat but I would not trust it +/- 10%, so the significance of 20% excess is low. The other methods used by Mizuno -- such as bomb calorimetry and flow calorimetry combined with mass spectroscopy -- are much more reliable and convincing. Since there is no jet of steam leaving the cell it is not likely that droplets will be carried a long distance, but it is still better to condense the steam and let the hot water drip back into the cell, the way Mizuno has done. This eliminates the issue altogether.

37) Ludwik Kowalski wrote:
Yes, the COP=1.2 is probably the smallest one can trust by using a simple technique. But, according to Pierre, the COP close to 2.0 are constantly reproducible in Naudin's lab. Was it an exaggeration, Jean Louis? Please describe your most efficient excess heat generator for us. What metals are used as electrodes? Are you also using the 0.2 M K2CO3? What is your average current? At what voltage are you operating? Numerous reports about reproducible experiments with the COP>1.3, coming from students, teachers and amateurs, would have an effect on mainstream scientists. Simple and inexpensive techniques should be encouraged. More sophisticate techniques are for those who can afford them.

38) M. Jullian wrote:
. . . indeed there is no reason to expect the mist's droplets to have a given minimum size, most of it's mass could well be invisible, and it would have no particular reason to be entirely caught by a screen of any arbitrary fineness. . .

39) J. Rothwell wrote:
[The simple evaporated mass method] is wonderfully nice and simple, but not precise, and not very reliable. To make it precise you would have to bring in all kinds of complex instruments and measurement techniques. The same goes for any method of calorimetry, except the Seebeck method. Flow calorimetry is conceptually nice and simple, but Mike McKubre will tell you, beyond the conceptual stage it becomes messy and complicated in real life. The same is true of the evaporated mass method. If you want to make it better than 10% accurate you will have to spend a few thousand bucks and a few months I suppose. Why bother?

There is a reason why I kept saying this "wet steam" issue makes me nervous, and why I have not expressed overwhelming confidence in the results reported here. As I said, if you improve your calorimetry you may well find your excess heat results vanish. There is also a reason why Mizuno stopped using this technique many years ago.

40) Ed Storms wrote:
The plasma method dissipates generated heat mainly by boiling water. In addition, this is how the excess energy will eventually be used, i.e. by making steam. Rather than fighting this condition, why not use it to measure the heat produced? This can be done by surrounding the plasma cell by good insulation (no calorimeter), have water of known temperature delivered to the cell at a rate required to keep the level constant, and allow the steam to exit through another tube. Place a plug of glass wool in the exit tube, within the plasma cell, to trap any droplets. Measure the temperature of the steam as it exits and weigh the condensed water. You can measure the pH of the condensed water to determine if any droplets get through the filter. Of course, some H2, O2 and CO2 will be in the exit gas. The amounts of these gases are easy to determine. The H2 and O2 can be combined back to H2O using a catalyst and the resulting water weighed. Once steady state has been achieved, you have an easy measurement of produced power. In addition, the apparatus can run as long as you wish to show that the output is stable. If significant over unity is indicated, the arrangement would be easy to scale up. Almost all errors will result in a lower value for produced power, hence believable OU if it is claimed. The main challenge would be to make the size of the plasma and the exit pipe compatible to prevent too much pressure buildup in the cell. Eventually, this pressure can be increased to give steam at a higher temperature. Why is this approach not used?

41) Richard Slaughter asked:
Ed, I like your proposed approach. I know there are chemical catalyst to recombine the H2 and O2. I have the impression that these can be dangeroue if the system is not running at the right temperature and there is no visable way to know the catalyst is working. Would it be ill advised to use a spark generator to recombine the H2 and 02? I like the idea of a spark generator because I can tell instantly if it is working. You are proposing a semi-closed cell so I'm concerned about gas build up.

42) M. Jullian wrtote:
In terms of indisputability by a careful reviewer, even if Pt was cheap, a unity COP with Pt as Ed and you suggest wouldn't help in the least because conditions would be too different. Plasma temperature would be different for one thing (different melting temperature). As I suggested before, even a unity COP obtained with a _tungsten_ cathod in a negative run isn't proof that a 1.3 COP in a positive run isn't an artefact (it could be due e.g. to the right regime of discharge nebulizing a particularly large amount of mist).

Group, it occurred to me that the simplest and least expensive method to _quantify accurately_ the mist phenomenon would be to condense each run's exhaust gases as has been suggested, _measure the total salt content_ of the condensates(*), and if not zero use it to correct the run's COP precisely (the maths are easy). This would eliminate altogether the need for a mist filter and would be skeptic-proof I would think. Your opinions welcome.

43) Ed Storms wrote:
a) Wet or dry, the steam leaves as a gas and the droplets join the liquid phase that slowly drips back into the cell. Droplets do not result from the steam or form within the steam, but form as bubbles burst on the electrolyte surface.

b) You wrote: “a way to renew the cathode continuously must be found for this.” This is easy to do by feeding a W rod through a glass tube into the cell. You might need to drain the cell periodically to remove the tungsten, tungsten oxide, and transmutation particles that would accumulate.

c) You asked about the mist filter. As I described above, the steam would push through any liquid in the plug. Because the plug is in the cell, the hot water returns to the cell where it adds its energy to the generation of more steam. Of course, the plug needs to be designed so that steam can leave at a slow rate, to avoid creating additional bubbles, and to allow the water to drain away. Such a design is easy to create.

44) L. Kowalski wrote:
a) Condensing escaping gasses (if practically possible) and vapors, makes sense to me. Please elaborate. (see the P.S. comment below).
b) Using a bomb calorimeter, as suggested by Jed, also seems appropriate when excess heat is generated at the rates of tens of watts, or more.
c) I wish to see more people performing experiments and reporting results.
d) Exploring the same type of phenomenon, and discussing the results on this list, could be very effective.

P.S. Suppose the mass of the water escaping from the condenser is measured. How do we determin what fraction of that mass is from the vaper and not from tiny drops escaping with it? The fraction must be known to calculate the COP correctly.

45) Ed Storms responded:
I don't understand why this is an issue. Removing the droplets is trivial. A resistively measurement of the condensate would give a good analysis for the amount of electrolyte that was not removed.

46) L.K. (not posted):
I am confused. If tiny droops are removed somehow (sparging ?) then what condenses should be pure water. Ed is referring to the study of the resistively of removed droplets. Obviousely, I did not understand some messages.

47) M. Jullian wrote:
. . . Ludwik I disagree with your objection that the condensate salt contents measurement method I suggested cannot measure escaped mist. Let me explain by using an example: if we find say 5g of salt in the condensate, along with _any_ amount of water, and if the electrolyte concentration is 50g per liter, then we can conclude 0.1 liter of electrolyte has escaped as mist. So 100g should be subtracted from the mass loss to determine evaporated mass, agreed? You also said the scale would not be needed anymore, why is this?

48) Jed Rothwell wrote:
. . . Conventional instruments and experimental techniques work fine. People have doing calorimetry and dealing with wet steam for 220 years. Find out how it is done and stop fretting about it. There are plenty of things that actually go wrong with this experiment, so stop inventing imaginary problems.

49) Ludwik Kowalski responded:
1) I disagree; it is better to anticipate what honest referees might say and to address the issues before submitting a paper. This debate was highly instructive to me. It showed that the issue of tiny droplets should be taken very seriously.

2) My last message showed that I did not understand the essence of Michel's suggestion. Only now do I do understand what he, and Storms, had in mind. It is a very clever method of determining the fraction of water escaping in the form of tiny drops. Yes, the scale would be needed, as in Paris-1 and Colorado-2. Am I the only one who did not understand the original proposal? Probably not. The Marseilles-1 experiment will probably produce more convincing results on reality of excess heat than earlier experiments.

50) M.Jullian wrote:
. . . As a lower cost alternative to a condenser, the mist collector could be an _external closed paper filter_ with a _large surface area_ (for low back pressure), airtightly connected to the cell's vent tube (a large vacuum cleaner bag with it's rubber gasket would do the trick), one numbered filter per run. The filter would be weighed once dried, and its new mass minus initial mass would yield the run's escaped salt mass (a more precise version of Mizuno's paper towels disposed around the cell as described by Jed).

Note the filter once dried and weighed would be reusable e.g. the following day ; successive dry weights, increasing with salt accumulation, could be handwritten on the filter itself vs the run numbers, e.g.:

0 -> 28.1g (initial mass)
1 -> 33.2g (+5.1) (run 1 has expelled 33.2-28.1=5.1g of salt)
19 -> 37.5g (+4.3) (run 19 has expelled 37.5-33.2=4.3g of salt)

IMPORTANT: the mist collector must exert no force on the cell: we mustn't count its weight with the monitored cell weight because at least some of the vapor will condense in the mist collector, which would make the evaporated mass erroneous. If we use a vacuum cleaner bag it must be independently suspended, with its opening on top, and the cell's rigid vent tube bent twice to connect airtightly but with zero vertical force with the bag's soft rubber gasket.

51) Ed Storms commented:
a) I get the impression that you have never seen glass wool and think it looks and acts like cotton. If it acted like cotton, it would not work as a filter because, as you say, it would block steam flow. Glass wool has a very open structure and it does not mat when it gets wet. Before you reject this method, I suggest you look at glass wool. I suggest a tube 2cm in diameter lightly packed, which is connected to a tube 0.5 cm in diameter that exits the cell.

b) Also, the paper filter method will not work. You need to have the filter where it remains hot so that steam will not condense. Otherwise, you will get a lot of water that did not come from droplets.

51) M.Jullian replied:
a) I personally trust your word that it would block all the mist without any back pressure, but this claim would be hard to prove indisputably, especially if the reviewer has never seen glass wool as you say. The only way actually would be to have external capture AS WELL, and then there would be no point in having an internal filter at all. So my point is that external mist collection is all that is needed, and we have plenty of room to do it.

b) You will indeed, but it doesn't matter as only the dry weight matters! As I wrote earlier, and as Ludwik may be the only one to have understood in fact, "if we find say 5g of salt in the condensate, along with _any_ amount of water, and if the electrolyte concentration is 50g per liter, then we can conclude 0.1 liter of electrolyte has escaped as mist. So 100g should be subtracted from the mass loss to determine evaporated mass"

52) L. Kowalski
I posted a long message (see in blue below) to which M. Jullian responded. He quotes my entire message and comments in different places.

53) M. Jullian:
You see I too think that mist is not really a problem, I don't even think chemical heat is a problem, but again what we think doesn't matter in the least, we need hard facts to put to rest any doubt a honest reviewer could have. Mist is one of those doubts, and chemical heat is another.

Hi Ludwik, my comments inserted into your blue text.

Let me try to review the situation, as i see it. Please correct me, if necessary. The goal is to reach an agreement on what is the best way to proceed. The bomb calorimeter, suggested by Jed, seems to be a good approach. But like in everything else, the devil is in details. I hope somebody will use a bomb calorimeter to confirm reality of OU ("over unity" COPs). Here I want to focus on the boiloff cell as used in Paris-1 and Colorado-2.

1) Use of the thermos is desirable but not essential because a large fraction of heat seems to be lost via convection above the open cell.
2) Layers of foam outside the cell walls are desirable (to keep the nonevaporative losses low).

I would think 1/ is better than 2/, both as a thermal insulator, and in terms of optical transparency (I suggest we use a transparent Dewar flask).

3) The cell should not be too large. In that way the electrolyte will boil and its temperature will be uniformly ~100 C, even when the wattage is low.

Agreed. It just has to be large enough to hold the electrodes and the heater.

4) A small ohmic heater (say 200 W) should be inside the electrolyte all the time. It should have its own W*h meter. In that way one should be able to turn it on and off at any time (to keep the electrolyte boiling, when plasma current is too low for this.

Agreed.

5) The COP is calculated as:

COP == Et / Ee (thermal energy released/electric energy received)

The electric energy is the sum of W*h (or joules) recorded by electric instruments (including the ohmic heater W*h measuring device) during a test. There are nuances in measuring the Ee, for randomly oscillating currents, but this is not our main problem. The thermal energy is:

Et = Ev + Ed + Ec + Er = Ev + En

It is the energy used to evaporate water (v) plus the energy escaping by conduction (d), escaping by convection (c) and escaping by radiation (r). The nonevaporative loss En, is simply the sum (Ed + Ec + Er). The method of measuring En with an ohmic heater, used in Paris-1, Texas-1 and Colorado-2 experiments is highly appropriate. Next we come to the

main problem. The Ev was calculated as 2260*dm1, where 2260 J/g is the latent heat of evaporation while dm1 is the mass of water evaporated during an experiment. Unfortunately, what we measure with a scale is not the dm1 but the dm=dm1+dm2, where dm2 is the mass lost in the form of tiny droplets. Up to now we assumed that dm2<<dm1. But, as indicated in the discussion, this assumption must be experimentally validated. If the assumption is not valid then Ev must be calculated as 2260*(dm-dm2), where dm is that we measure with a scale and dm2 is the amount of water escaping in the form of tiny drops. Ignoring the dm2 we overestimate the COP.

Ejection of large (visible) drops can be practically eliminated by covering the cell with a fine wire mesh. Large drops are stopped be the mesh and fall back into the cell. Under favorable plasma conditions large drops are rare and they contribute to a small fraction of En (which we measured). The main problem is how to measure the dm2, or how to show that it is indeed negligible in comparison with the dm1?

Michel first solution was to collect all water escaping from the cell and to condense it. Suppose that the scale tells us that dm=10 grams while the mass of condensed water is 7 grams. It means that ~30 % was not condensed. It is important to know the percentage. Michel suggested that condensed water must be evaporated in order to collect solid deposits. The water that was evaporated did not contribute to that deposit. Only the tiny drops bring the dissolved chemicals with them.

We know how much deposit is produced when we evaporate one cubic centimeter of the electrolyte (taken from the cell). Suppose it is 0.3 grams. If the amount of deposit per cc of condensed water is 0.003 grams we can say that dm2 is indeed negligible (when the accuracy of 5% is sufficient). But what would we say if the measured amount of the solid deposit, per cubic centimeter of condensed water, turned out to be 0.1 grams. We would say that about one half of water lost was due to the dm2. Why one half? Because not 100% of lost water was condensed. This shows that the value of dm2 can be determined. Once determined it could be used in the calculation of COP.

Condensers, cooled by tap water, are routinely used by chemists. My guess is that the cost could be about $250. Please correct me if you can; I do not have any catalogues at home. What is not clear to me is a method to catch the escaping water (dm1 and dm2) into the condenser.

A thin rubber diaphragm connecting the cell output tube to the condenser input as I suggested should do the trick (think of the vacuum bag gasket if you have ever seen one of those ;)

Vertical leads to the anode and the cathode, in our cell, would interfere with efficient capturing of the escaping water into the condenser.

We need airtight seals for the leads and everything that goes across the lid. The lid itself must rest on top of the Dewar via a rubber gasket. The inverted U shaped output tube should be sealed to the lid.

So much for a condenser idea, as I understood it yesterday. Simple in principle but not easy to implement in practice, unless one is well equipped for custom-made devices.

6) I am not sure I understand your glass wool filtering method, Ed. How does it help us to determine the dm2? I can visualize a layer of glass wool above the cell, for example between two plastic screens (used on window). The screens have holes to insert electrodes. Suppose the scale tells that 10 grams of water was lost during an experiment. What else do you have to measure to determine the dm2?

7) Michel's method is conceptually clear to me. But it might indeed lead to a large underestimation of Ev (energy used to evaporate water), as indicated by Ed.

No! Wrong, as I have explained to Ed, and I thought you had understood! Dry mass of condensate is all that matters. Again: "if we find say 5g of salt in the condensate, along with _any_ amount of water, and if the electrolyte concentration is 50g per liter, then we can conclude 0.1 liter of electrolyte has escaped as mist. . . .

Underestimation, however, is not as bad as overestimation; it would mean, for example, that what is calculated as

COP=1.2 might actually be 1.5 or more. But we want accurate determinations of COPs (to get the mean value and the standard deviation). I do not understand Ed's objection that the ionic composition of the electrolyte must be known.

Ed's objection could be valid maybe if a large mass of escaped salt is found, which we hope won't be the case.

A filter can always be calibrated by using the electrolyte taken from the cell. We can place 1 cc of the electrolyte on the filter, allow the water to evaporate and measure the amount of the remaining solid deposit. Then determinations of dm2 can be made, at least approximately, in consecutive tests. It would be important to often add 100 C water to the cell to keep the concentration of the electrolyte constant, more or less.

Yes.

My hope is that dm2 will be found negligible. A submitted paper stating that this was indeed the case would be much more difficult to reject than the paper in which the negligibility of dm2 is assumed.

Yes!

P.S. (next day): Now I realize I missed your point (and Ed's). You referred to the external paper filter solution, and assumed that the wet filter was on the scale along with the cell hence affecting it's weight, which indeed would lead to a large underestimation of Ev due to re-condensation of some of the vapor in the filter.

The answer is still "No! Wrong", and the explanation was in my proposal:

IMPORTANT: the mist collector must exert no force on the cell: we mustn't count its weight with the monitored cell weight because at least some of the vapor will condense in the mist collector, which would make the evaporated mass erroneous. If we use a vacuum cleaner bag it must be independently suspended, with its opening on top, and the cell's rigid vent tube bent twice to connect airtightly but with zero vertical force with the bag's soft rubber gasket.

In case your vacuum cleaner bags don't look the same as mine, mine feature a thin soft rubber membrane with a circular hole in them, which connects airtightly to a slightly conical rigid tube at the end of the cleaner hose by a push-on action when installing the bag in the cleaner. This kind of airtight coupling does not exert any significant force perpendicularly to the membrane plane, so if this plane is horizontal there is no vertical force which could affect the cell's weight. So there would be no underestimation of Ev at all. Same kind of no-vertical-force coupling should be used in the condenser solution IMHO. A bellows type airtight coupling would work too (probably better actually).

The corollary of this absence of vertical force between the cell and the output capture device is that the latter must be suspended independently of the cell of course.

Let me know if the above was still unclear, as this point is important: it's ok to underestimate the COP, but not grossly!

54) L. Kowalski again:
J.M. wrote “Mist is one of those doubts, and chemical heat is another.” According to private messages, Paris-2 results will allow us to rule out the anticipated "chemical heat" objection. I hope that new information will soon be published here. Then we can discuss significance of numerical data.

We want to know much water is lost as vapor (dm1), and how much water is lost as tiny droplets, (dm2). A separate experiment, not the one whose purpose is to measure the COP, can be conducted. Michel's idea is to capture a sample of what is escaping. This can be done by using glass wool (also used as thermal insulation) or by using a paper-like filter (also used in common vacuum cleaners). In each case the sample is weighted twice, first as wet and second as dried (after water is allowed to evaporate. The difference was tell us how much water was in the sample, the second mass will tell us how much deposits. A lot of deposit would indicated that dm2 was large, very little deposits would indicate that dm2 was very little.

Knowing the volume of water in the sample, and the amount of salt in it, we are able to calculate the salt concentration

(for example, in mg/cc = grams/liter). Suppose the sample had 0.27 mg of solid deposit per one cubic centimeter of evaporated water. We then take some electrolyte from the cell and repeat the procedure. Suppose we find 27 mg of solid per cubic centimeter. Would it be appropriate to say that dm2, mass lost in the form of tiny drops, is 1% of the total mass lost? I think so. Note that the vapor escaping from the cell consist of pure water while the droplets escaping from the cell contain the salt. Consider the extremes. If 100% of the liquid was lost in the form of tiny drops then our sample would have the same concentration of salt as in the electrolyte taken the cell. And if 100% of the electrolyte was lost in the form of vapor then the concentration in the sample would be zero.

Did I describe your suggestions correctly, Michel (paper filter) and Ed (glass wool)?

Here is another way to accomplish the same thing. Suppose a large funnel is placed above an open plasma electrolysis cell. The leads to the electrodes, in this control experiment, can be on the side of an open plastic cell. The narrow exit tube of the funnel is plugged in and sticks up, like a blocked chimney. The wide end of the funnel is only several centimeters above the cell. A bag with ice can be used to keep the funnel cold. The diameter of the funnel should be larger than the diameter of the cell. Under such conditions what comes out of the cell will condense on the inner surface of the funnel and drip down into a large plate (on which the cell is standing). We can collect the liquid, measure its volume, and remove the water by evaporation. This again will give us concentration of the salt in the sample, for example, 0.27 mg/cc, as above.

I suggest that an estimation of dm2 should be made in Paris-2. Any one of these three approaches is likely to tell us the fraction of the liquid lost in the form of tiny droplets. If dm2<<dm1, for example, 1%, then the reported COP are fine. Otherwise, the COPs should be recalculated by taking the dm2 into account. Note that in Colorado2 COPs were calculated under the assumption that the dm2 is negligible. Will the Paris-2 data justify that assumption? That remains to be seen.
P.S.
Actually three mass measurements must be performed with each sample, not two. First before catching the liquid, second when the catcher is wet, and third after the catcher and remaining solid in it, are dry.

P.P.S.
On Apr 2, 2006, at 3:34 PM, Michel Jullian wrote:

You see I too think that mist is not really a problem, I don't even think chemical heat is a problem, but again what we think doesn't matter in the least, we need hard facts to put to rest any doubt a honest reviewer could have. **Mist is one of those doubts, and chemical heat is another.**

1) According to private messages, Paris-2 results will allow us to rule out the anticipated "chemical heat" objection. I hope that new information will soon be published here. Then we can discuss significance of numerical data.

2) Go to Google type "distillation," in the search box, and click SEARCH. Then type on "images," above the search box. You will see standard glassware used in laboratories. Before deciding what is the best we must know what we want. How much water is lost as vapor dm1, and how much water is lost as tiny droplets, dm2. A separate experiment, not the one whose purpose is to measure the COP, can be conducted. Michel's idea is to capture a sample of what is escaping. This can be done by using glass wool (also used as thermal insulation) or by using a paper-like filter (also used in common vacuum cleaners). In each case the sample is weighted twice, first as wet and second as dried (after water is allowed to evaporate. The difference was tell us how much water was in the sample, the second mass will tell us how much deposits. A lot of deposit would indicated that dm2 was large, very little deposits would indicate that dm2 was very little.

55) J. Rothwel:
The ice would probably cool down the cell and interfere with calorimetry. Keep ice away from these experiments. I have seen some really dreadful experiments involving ice and calorimeters (and one impressive one). Mizuno used a much better condenser. The flow calorimetry cooling water condensed the steam and the water returned to the cell. See: <http://lenr-canr.org/acrobat/MizunoTconfirmatib.pdf>.

I do not understand why you want to use such complex and unusual methods, when simple, direct, foolproof methods are available. "Wet steam" is only a problem when you do take steps to prevent droplets from leaving the cell or to measure the total enthalpy of the effluent gas. Such steps are trivial, as Storms pointed out, and once you take them you need not worry about the problem again. You do not need to invent six different ways to address this. Simply select the most convenient method from the ones already suggested by Mizuno, Storms and me. (I suggest sparging and measuring the temperature rise of the water column.) It does not matter which method you use.

56) L. Kowalski:
Yes, ice should be kept away while measuring the COP. My suggestion is to conduct a control experiment whose only purpose is to determine the fraction of mass escaping in the form of tiny drops. Once know, for a given electric power, and given geometry, it can be used in experiments whose purpose is to measure the COPs.

Any method is OK for me. The funnel method was just an improvisation. The glass wool catcher seems to be the easier. But to avoid possible consequences associated with glass-wool needles I would use paper towels, napkins, or toilet paper instead. Keeping such collectors above the running plasma cell would accumulate escaping liquids at the rates of grams per minute (?). What can be more simple than this? Measuring the percentage of the liquid lost as droplets, in a separate control experiment, should be done by anybody performing Mizuno type experiments. Perhaps Richard will do this. Then we will know by how much our Colorado-2 COPs must be corrected.

57) M. Jullian:
I submit on the contrary that mist measurement, or any control measurement, must be done on the very runs for which we measure the COP. Otherwise we prove nothing at all, because the ODD overunity runs could be thought by an honest reviewer to be due to the PECULIAR, hard to get, right kind of discharge generating equally PECULIAR ARTEFACTS, like for example: - nebulizing a particularly large amount of mist (e.g. by this discharge generating ultrasounds of the same frequency as used in an ultrasound nebulizer). (a)- plasma reacting exothermically with the salt. (b) - generating an insulating foam layer (c) - or anything which may give the false appearance of excess heat and hasn't been thought about yet.

58) Jed Rothwell:
Michel Jullian wrote: “Hi Ludwik! I submit on the contrary that mist measurement, or any control measurement, must be done on the very runs for which we measure the COP.” Yes. This is essential. Also you should use the simplest and most conventional method possible. Never invent a new or unusual technique if there is already a standard, widely accepted one.

59) L. Kowalski:
Then the ohmic heater should not be used; the nonevaporative losses of thermal energy should be made without it. How can this be accomplished? Let the perfect not be the enemy of the good. I would also prefer a setup in which all measurements are made during one operation. What I am suggesting is a simple costs-nothing test that should make the overunity COPs more credible.

60) Chemistry facts (not posted):
a) What is formed when the K_2CO_3 is dissolved in H_2O? It is K^+, CO^{--}, HCO_3^- and some H_2CO_3. How much of each depends on the temperature. The CO_2 is lost to air because the H_2CO_3 breaks into H_2O+CO_2. The amount of CO_2 going into air increases with temperature. After the electrolyte cool the CO_2 may be taken back from the air but this is a slow process (hours or days) of recreating the H_2CO_3.

b) Can the K_2CO_3 be restored, for example, when the electrolyte was not used for many days and then allowed to evaporate, forming a solid residual deposit? That would create an illusion that no potasium carbonate was spent during the plasma electrolysis. The fact is that all K_2CO_3 disappears quickly when the electrolyte is made.

$HCO_3^- + OH^- \dashrightarrow \dashleftarrow CO_3^{--} + H_2O$

Some KOH might be formed in the electrolyte (from K^+ and OH^-). It is a complex, temperature-dependent kinetics.

61) Peter Gluck, who is a chemist, wrote:
Perhaps it would be useful- for understanding how the Mizuno system works- to perform some qualitative tests, before deciding re. the new experimental setup. It seems we have a system in which a solution of a K salt in water at ~ 100 C is in contact with a plasma flame at (?) 3000 C. A huge temperature gradient. Unnatural combination. It would useful to:

(a) measure the temperature above the plasma zone - a bit above the water level;

(b) to put a baffle - e.g. a sheet of shiny stainless steel in the same zone in order to collect the microdroplets and to see if what's released from the cell is K2CO3 or KOH (pH will tell, or you put a bit of acid and see if gas-CO2 is released or not)

(c) if it is KOH mainly, then to establish if the initial K2CO3 is transformed in KOH- that means CO2 is added to the gases, the material balance has to be made for the ion K+

(d) if this is the case, to see what happens - in the actual system if the carbonate is replaced with KOH from the start- is plasma formed? the COP is O/U? etc.

(e) to see if the W precipitate has the the structure I expect i.e. a core of salt covered by molten and frozen metal (formed by spark erosion) - a simple microscope is needed.

62) L. Kowalski:
Jed wrote: “I submit on the contrary that mist measurement, or any control measurement, must be done on the very runs for which we measure the COP. ”The paper-towel test (PTT), suggested yesterday, can be performed in that way. A typical Colorado-2 experiment lasted 12 minutes; after that we had white flashes indicating that the sticking end of the cathode was essentially consumed. Suppose the time from t~0 to t~2 min is used to perform one PTT and the time from t~8 to t~10 min is used to perform another PTT test. The COP can then be measured in 8 or 9 minutes between these two tests.

63) Michel Jullian:
Your paper towel test proposal consists in covering the top opening of the cell with paper towels to collect escaped salt. I don't see how you could perform a COP measurement while doing this test, as some amount of evaporated water will recondenses in the towels, wronging the evaporated mass measurement . . .

64) L. Kowalski:
I believe that the paper towel test is the same test you suggested (keeping the vacuum cleaner bag above the open cell). The only difference is that your bag is above the cell the entire time interval during which the plasma electrolysis is going on, for example 11 minutes while I would collect two small samples of the escaping liquid, one during the first minute and another near the end. The COP is measured between the sampling. Nothing is turned off or changed. In that way the COP measurement is performed under the same conditions as when the two samples are collected.

Yes, you can question "the same conditions" assumption but, as stated by Jed, that would not be reasonable, especially if the two samples give about the same % of mass lost in the form of droplets. Neither time nor mass lost by the cell have to be monitored when a paper towel (or toilet paper, or a handkerchief) is kept (by hands) about 5 cm above the open cell for about one minutes. That test might not answer all the questions but it is so simple that one must perform it. I hope that Pierre and Richard will perform such tests and share the results with us. We do expect the percentage to be small. That results should be reported in our anticipated paper.

65) J. Rothwell:
I doubt this test would prove anything. First of all, the method is crude. Some water vapor would condense on the towel (false positive). On the other hand if there are droplets in wet steam, many would miss the towel and drift away

in the air (false negative). It is impossible to tell which is more likely. Second, there would only be 5 or 6 g of droplets over 1 minute, and you have no idea what fraction of this you can capture with a towel. In your message of 3/15/2006, you said that over a 20 minute run, the weight of the cell drops from 3052 down to 2801 g. That is 13 g per minute. You said the apparent COP was ~1.4, and a null run was around ~0.8. So about 4/10 of the lost water would be in droplets to explain away this excess heat.

I think you should stop dreaming up crude and unusual ways to address this problem, and simply use industry standard methods. Also, by the way, if you do this experiment again, I strongly recommend you use a computer, multiple thermocouples, digital cameras, and other modern instruments to record the event in detail, and a magnetic stirrer and other gadgets to improve accuracy. There is no point to going to all this trouble to do the experiment if you are going to use primitive 19th-century instruments.

66) Michel Jullian:
Ludwik I hadn't realized you meant to hold the paper towel _by hand_! But in this case as Jed just pointed out the steam and mist won't be forced through the paper as was the case with my airtightly connected vacuum bag suggestion, so you will only capture a tiny fraction of the mist because it will just go round the obstacle, so you'll overestimate the COP. It's a very sloppy solution, and it just won't work.

67) L.Kowalski:
Suspecting that the debate about a reliable estimate of the % of the liquid lost in the form of tiny droplets reached the point at which most people are no longer interested, I suggest that Michel, Jed and myself discuss this topic in private, at least for a while. My reply to the last message from Michel will go to him and to Jed only. But I will be happy to add names of those who might also be willing to participate. Write to at <kowalskil@mail.montclair.edu>.

68) L. Kowalski:
PRIVATE
1) Michel, please write again the latest version of your method. I suspect I am missing something important. Please attached a crude picture. Then I will comment.

2) Jed, you wrote: "First of all, the method is crude. Some water vapor would condense on the towel (false positive). On the other hand if there are droplets in wet steam, many would miss the towel and drift away in the air (false negative). It is impossible to tell which is more likely." I agree, that is a serious issue. I do not want to catch all that comes out, a large enough sample is sufficient. But it is true that the percentage of tiny droplets I catch might decrease when the paper towel is kept higher. Perhaps Michel's method will not suffer from this. I will wait for his picture plus detailed description.

69) Michel Julian:
Hi Ludwik, the vacuum cleaner bag solution was an earlier more complicated proposal, forget about it. As I said no accessory is needed in the current proposal. So I guess it's now time to address the more refined proposal, which allows for CO2 emission.

It is based on potassium mass loss (which can only occur through mist emission), rather than on total salt mass loss (which is affected not only by mist emission but also by gaseous CO2 emission as Peter Gluck pointed out). So basically we just replace "salt" by "potassium" in the method:

1/ weigh the cell's potassium before the run (when preparing the fresh electrolyte ; mass of potassium can be deduced easily from salt mass and salt formula) 2/ weigh the cell's potassium after the run (e.g. measure by precise chemical analysis K+ concentration in g/l, which any medical lab can do for you, and multiply by precisely measured volume)
3/ subtract to get escaped potassium mass
4/ infer escaped droplets mass from escaped potassium mass, based on
salt concentration and formula. Does it still make sense?

70) Jed Rothwell wrote:
We beat that horse to death days ago. Ed & I suggested several ways to solve this problem. If you do not like our

suggestions, do it your own way. I am fed up with repeating these suggestions and I will not say another word about it. In fact, I am fed up with this whole discussion!

71) L. Kowalski wrote:
Too bad that you decided not to share me with what you know about old ways of measuring the percentage of droplets escaping with steam. I do not think our university library has book about this. But I will look for them. Perhaps measuring the temperature gradient in a pipe above the cell, as you hinted, will become clear to me. I know how to measure the gradient of temperature but I do not how to use this information to get what we need. Do not give up on me; I would be glad to endorse your recommendation after understanding it, and after playing with some realistic numbers.

72) J. Rothwell wrote:
Don't be tiresome Ludwik. Review the message archive here and look at the literature. I spelled it out several times and so did Ed Storms. I will reiterate the methods one last time.

Pons: reflux calorimeter. For more, enter the search term "reflux" into the Google search box at LENR-CANR.org.

Mizuno: flow calorimetry, where the cooling fluid goes through a condenser first. http://www.lenr-canr.org/acrobat/MizunoTconfirmatib.pdf

Storms: glass wool in effluent gas tube.

Rothwell: sparge effluent gas tube through a water column. I suggest you insulate the water column and measure the temperature rise. Storms suggests you measure the pH of the water column, or condense the steam by some other method and measure the pH of the condensate. Storms: use a large, closed Seebeck calorimeter. (I agree this method is the best by far.)

73) J. Rothwell wrote:
Here is my suggested method of doing the Ohmori-Mizuno glow discharge experiment while accurately accounting for wet steam. This is based upon many observations I made of both Ohmori and Mizuno at work in their labs, and it is also based upon suggestions made by Storms in this forum recently.

If the run is going to be short, 10 or 15 minutes, I suggest you use a well-insulated cell. (What I have designated "bomb calorimetry.") Seebeck calorimetry is preferred but it is more expensive.

Cover the cell with an airtight lid with various orifices in it to accommodate the anode and cathode lead wires, thermocouples and so on, plus an exhaust tube to capture all effluent gas. You might want to include an emergency valve in the lid. This can be as simple as a drinking straw bent into a "U" shape and stuffed into two holes.

Stuff glass wool into the end of the exhaust tube that goes into the cell. This will stop most of the moisture from leaving the cell.

1) WHAT IS MOISTURE? PROBABLY DROPS OF WATER, NOT VAPOR. THE INTERCEPTED DROPS WILL DRIP DOWN TO THE CELL, AS DESIRED. RIGHT?

Prepare a condensation water column. This is a well-insulated, tall beaker. Weigh it, fill it with ~200 mL of distilled water, and weigh it again to determine the exact mass of water. Measure the pH of the water.

--> Right. Liquid water entrained in the steam will drip back into the cell.

Put the other end of the exhaust tube at the bottom of the water column so that all effluent gas bubbles out through the column and the steam condenses. Also insert a thermocouple into the water and record the temperature.

2) THAT PART IS NOT CLEAR TO ME. HOW IS THE TOP OF THE EXHAUST TUBE (ABOVE THE GLASS

WOOL) CONNECTED TO THE WATER COLUMN ABOVE? ON THE PICTURE I AM MAKING THE WATER IN YOUR CONDENSATION COLUMN WOULD RUN DOWN INTO THE CELL (THROUGH THE GLASS WOOL). WHAT AM I MISSING.

--> It is very simple, [see the drawing below]: That drawing shows that the water column is not above the cell, as i assumed. It is a beaker standing on the same table that the scale supporting the cell. The U-shaped tube containing the glass wool bends twice above the lead of the cell. It provides the only possible escape from the cell. In other words, the lid above the cell must be tight.

The part of the escape tube containing the glass wool is vertical. Thus the intercepted drops can fall down by gravity. Then the tube bends to become horizontal. After that it bends again and becomes nearly vertical. That is how it enters the beaker with water where the vapor is expected to condense. The right-side ending of the tube is situated near the bottom of the that beaker, deep below the surface of water, about as low as the glowing cathode in the cell. The O2 and H2 bubbles come out from the escape tube, rise in the condenser and disappear in the air above the open beaker. Yes, a simple picture is better than many words.

The cell should be equipped with at least three thermocouples separated vertically and horizontally. It should be mounted on a magnetic stirrer. The vibrations from the stirrer make it impossible to measure the weight of the cell during the experiment accurately, but mixing the fluid is more important than measuring the weight precisely. This is not important; you can measure the weight precisely before you turn on the stirrer, and after the experiment when the stirrer is turned off.

Record the appearance of the plasma with a video camera, and a digital still camera. You can probably set the digital still camera to take pictures automatically every 15 seconds or so, by connecting it to a computer. Be sure you turn on the video camera and still-camera date-time-stamp, but keep the date-time-stamp away from the important parts of the picture, such as the cathode. This will help you performed the experiment from a safe distance.

During the run, record all temperatures frequently, such as once per second, rather than every 10 seconds. If the cell explodes you will want a record of this -- assuming you survive. (If you think I am joking about this, you have not been paying attention and you should not be doing this experiment.) The video record may also be helpful in the event of an explosion. You should also record the water column temperature, ambient temperature, input power, weight scale reading, and all other relevant parameters in the same computer, in simultaneous time stamped records. This should be obvious but I suppose some people might record them in separate devices and then try to reconcile them manually.

Immediately after the run, momentarily turn off the magnetic stirrer to record the exact mass of the cell. Then turned back on and let it run until the cell cools to room temperature, which should take about an hour. Continue recording all parameters during this period.

Also immediately after the run, stir the water column (unless you also have a magnetic mixer for it), to be sure you have recorded the correct temperature. If the temperature has risen less than 1°C, you started with too much water in the column. If it rises more than 10°C you need to increase the starting volume of water. Measure the pH of the water column to estimate how much of the unboiled water has passed through the glass wool trap.

3) MY UNDERSTANDING WAS THAT ONLY THE VAPOR PASSES THE TRAP? THAT IS NOT THE UNBOILED WATER.

--> Mostly vapor, but a small amount of liquid water is bound to get through. Probably a tiny amount that does not matter.

4) I WOULD NOT KNOW HOW TO CALCULATE THE AMOUNT OF UNBOILED WATER PASSING THROUGH THE TRAP ON THE BASIS OF PH . . . A LITTLE NUMERICAL ILLUSTRATION WOULD HELP.

--> Neither would I but I am sure it would be easy to find out.

When the cell cools down, turn off the magnetic stirrer and record the exact mass one more time to account for any additional losses to evaporation, and measure the mass of the water column beaker.

That should do it. This should give you enough data to accurately account for heat lost to evaporation and radiation from the cell.

5) I SEE THAT THE OHMIC HEATER IS NOT MENTIONED. HOW IS IT USED?

--> It should be included to calibrate. I neglected to mention it, because it is obvious.

It should greatly reduce wet steam, and even if there is any wet steam, all of the enthalpy from it will be captured in the water column so it will not matter.

6) BY WET STEAM YOU PROBABLY MEAN VAPOR MIXED WITH TINY DROPLETS. IS THIS CORRECT?

--> Yes

Note that mass lost from the cell and not accounted for in the water column is mainly free hydrogen and oxygen. You can estimate whether this exceeds normal Faradaic levels, which would indicate that some hydrogen and oxygen was generated by pyrolysis.

7) THE ABOVE TWO SENTENCES ARE NOT CLEAR TO ME. MY UNDERSTANDING WAS THAT THE WATER COLUMN IS OPEN FROM ABOVE. UP TO THIS POINT I THOUGHT THAT THE COLUMN WAS OPEN, ALLOWING THE H2 AND O2 TO ESCAPE INTO THE AIR. WHAT DID I MISS?

--> You missed nothing. The mass lost from the cell is in three phases:

Liquid -- collected in water column
Vapor -- condensed in water column
Gas H2 and O2 -- escapes from column

You can estimate very roughly how much gas escapes by change in mass in the cell minus change in mass of column.

I leave the details of the heat balance computation up to you.

General advice: Do not stint on instruments or materials. Do not use haphazard methods, unusual methods, half-measures or unreliable equipment. If you are going to devote several weeks or months of your life to this work -- and perhaps even risk your life and health to accident -- you should either *do this right* or *do not do it at all*. I cannot over emphasize that. As Storms pointed out, this field has seen too many half-baked experiments already.

If the results show a positive heat balance I recommend you publish a paper along with every scrap of data and every single still photo on a web page, such as LENR-CANR.org. Do not hold back or summarize data; let those who would critique your results see every detail.

General advice to all researchers, everywhere: Read the last message written by Wilbur Wright, a few weeks before his untimely death. Wright was one of the most gifted experimental researchers who ever lived, and he survived hundreds of tests far more dangerous than a glow discharge experiment. His message is on page 11 of this document:

http://lenr-canr.org/acrobat/RothwellJthewrightb.pdf

Every word of Wright's message applies to cold fusion, in spades. Most research has failed because people ignored the precepts he outlines. I would post his text here, but I encourage you to read the whole document.

P.S.
I wrote:

You might want to include an emergency valve in the lid. This can be as simple as a drinking straw bent into a "U" shape and stuffed into two holes.

Cancel that. The exhaust tube is good enough. If there a buildup of steam too quick for that tube to vent, the drinking straw valve will also fail. The top will fly off and the cell will shatter. This happened to X. Zhang *et al.* three times, as I noted in a revised version of chapter 12 in my book, uploaded yesterday. See:

Zhang, X., et al. *On the Explosion in a Deuterium/Palladium Electrolytic System*. in *Third International Conference on Cold Fusion, "Frontiers of Cold Fusion"*. 1992. Nagoya Japan: Universal Academy Press, Inc., Tokyo, Japan.

Message: Do not stand too close to these cells. Do not sit there with your eye a few centimeters away from the cathode the whole time, watching the glow discharge. Use safety goggles. For Goodness Sake. Have some common sense.

74) L. Kowalski wrote:
In trying something new Richard wants to use a closed cell (with a recombiner). Those who did this used a flow calorimeter. One must learn how to avoid problems with the recombiner, and how to build a reliable flow calorimeter. A bomb calorimeter, mentioned by Jed, probably offers a way to avoid the flow calorimeter.

(a) Considering the high rate at which excess energy is generated in a plasma electrolysis cell, for example 50 W or 100W, the calorimeter can be very simple. In fact, it can be a carton box surrounded by a layer of white packing styrofoam. A small air circulation fan is likely to be neceassary to keep the inner temperature uniform. No scale would be used, as in the water-evaporation calorimeter.

(b) One should be able to open the "door" of the calorimeter and put the working cell inside rapidly. Then it becomes a matter of recording the rising temperature for the duration of a test. Suppose the door is closed at t=0 and the glow discharge current is turned off at t=9 min. Suppose the change of temperature, in the last 5 minutes, is 24 C. How much thermal energy was generated during the test?

(c) The heat capacity of the calorimeter (K in joules per degree) must be known to answer this question. It can be determined by using an ohmic heater, for example, a 60 W light bulb. Suppose the temperature changed by 9 C when the light bulb was on for 10 minutes = 0.1667 hour. The electric energy received would be close to 60*0.1667=10 w*h. I would not trust the 60 W written on the ball; I would measure the w*h with an instrument. The measured 10 w*h translates into 36000 J. Thus K=36000/9=4000 J/C. That is the calibration of my bomb calorimeter. The value of K must be determined when the cell with the electrolyte is inside the calorimeter. This is important because by removing the cell, or by reducing the amount of the electrolyte, one would decrease the value of K.

(d) Now the question asked in (c) can be answered. The formula is Ethermal=K*dT=4000*24=96000 J. That is it. (It terms of power Pth = 96000J / 300s = 320 W).

(e) There is no need to wary about splashing or about tiny droplets being mixed with the vapor. The electric energy, on the other hand, delivered to the cell during the last 5 min, would be measured in the same way as for an open cell. Suppose the Pel turns out to be 240 W (leading to Eel=240*300=72000 J. Then one would say that the COP=96000/72000=1.33. But one thing was neglected. The recombiner has to be heated and this must be taken under consideration before calculating the COP. Suppose the power needed for this is 40 W (?). This leads to 40*300/3600=3.333 w*h = 12000 J. Thus Tth =(96000-12000) = 84000 J and Pel=240-40=200 W. Thus Eel=200*300=60000 J and the COP=84000/60000=1.40. The energy delivered by the electric fan, for example 20 W (?) could also be accounted for.

(e) The nonevaporative losses are likely to be negligible, as I assumed, if the styrofoam is thick and and fits the box tightly. A sticky tape along the rims would help to reduce convectional losses. If losses are not negligible then they can

be measured (see item i below).

(f) Unfortunately, the second big issue -- chemical origin of excess heat -- is not being addressed by switching from one type of calorimeter to another. That is where help from a good chemist would be essential.

g) Would this approach also work for an open cell? Probably yes, if one was willing the dry the calorimeter after each test. I would not try this.

h) I never used a bomb calorimeter. That is why the above description is probably too naive. What else should be taken under consideration?

i) Oh yes, the box should have a small mass, to make the calorimeter more sensitive (small K). Thermal inertia will probably be a factor. I suppose one would have to wait several minutes, after the current is turned off, till the temperature stops rising. Waiting much longer, perhaps several hours, one would see the temperature going down. The rate at which the temperature goes down could be used to calculate thermal losses due to conduction and convection. What else should be anticipated?

75) R. Slaughter wrote:
I'm confused...nothing new... I have two designs in mind. the first is a closed cell with a vent tube as describe by Michel and others in earlier post. We can measure the non-evaporated heat lost of this cell the same way we did with the open breaker cell. Instead of sparging through a condenser column we sparge the vent tube steam through water in a second beaker. The steam condense and adds water to the second beaker. We can even put the second beaker in an ice bath to make sure we condense all the steam. The weight gain of the second beaker is P-evaporated. We calculate COP the same as before. I'm counting on the small size of the vent tube to greatly decrease the possibility of mist and prevent expulsion of K2C03? We can also put glass wool as suggested by others in the vent tube to filter the steam. The liquid in the second beaker should just be distilled water if everything is correct. We can now check the condensed liquid for other chemicals and residues.

The second design is a truly closed cell with a condenser loop. The steam that escapes the vent tube sent to an open air condenser a small pump returns the liquid back to the cell. The trick to this design will be getting a steady liquid flow through the condenser. We measure the input and output temperatures of the condenser and with flow rate we have P-evaporated. We run the same setup using the ohmic heater and we can measure the non-evaporated loses in the cell. We again calculate COP as before.
P.S.
I should have added that both of these approaches would work best if the reaction can be keep going long enough for the system to reach equilibrium. I see a need to be able to feed the cathode to keep the system running for perhaps two hours. In the first design we would need a water reserve to keep the electrolyte level constant so the system could run for hours.

76) L. Kowalski (not posted):
I find

77) M. Julian:
Ok the proposals so far are (BOMs only for now please),

Michel J-1.0
- insulated cell (e.g. transparent Dewar)
- insulating lid, not airtight, vented, sitting on top of the cell ready to
pop off (holds the electrodes and heater)

Richard S-1.1
- Lid should be tight enough so that steam and gas can not escape around the edge. All steam and gas must exit the vent tube.
- Vent tube should be stainless steel. In the shape of a "U".

- Suggestion on the best way to recombine the H2 O2, for safety
concerns!
perhaps a spark plug or glow plug.
- Suggestion on best way to capture and condense the steam? My thoughts
are to run an insulated plastic tube from the vent to a second beaker of water on the balance and sparge the steam through water in the second beaker.

Michel J-1.0 being the simplest so far, let's make it the PUF (proposal under fire)

Michel J-1.0 method to determine mass loss due to mist (inspired by Peter Gluck's comment on potassium not being consumed):

Escaped mist mass = (Potassium mass loss)*K, where K is an easy to determine constant factor (65.71 for 0.2M K2CO3), and potassium mass loss is determined by chemical analysis of final electrolyte, initial potassium mass being known.

I initially thought this required a fresh electrolyte for each run, on second thought that's not necessary, initial potassium of one run being final potassium of the previous one. Small samples of the cell's water could be taken after each run for batch-analysing at the end of the series of runs. If no potassium has disappeared the COPs don't need correcting for mist, otherwise we have an accurate correction. Any potassium loss due to transmutations would be negligible of course.

Sample taking and analyzing is required for chemical heat assessment anyway, so added cost of the method is null.

Questions/comments/critics on this proposal please, let's quickly find it's flaws so we can amend it and move on to another proposal.

78) Peter Gluck:
Strong polycarbonate shield, . . . fire extinguisher, . . .goggles. [Also] an analytical chemist friend who will tell you what on the earth happens with the potassium carbonate and all the other chemical, physical and nuclear (?) players involved in the game. [Also] an open mind - to decide to go along the line: "first understand, then measure again"

79) M. Julian:
Remembering unpublished information I have received from Pierre on preliminary Paris-2 results with their standard (silvered) Dewar, without changing the method for tracking mist and chemical heat I proposed, I would like to further simplify my proposal's BOM as follows:

MJ-1.1
- open insulated cell (e.g. narrow transparent cylindrical Dewar cf attached picture)

(insulating lid is not necessary if the insulated cell has a small enough open area for non-evap losses to be sizeably smaller than observed excess heat, which is the case with the Paris-2 narrow Dewar, where non-evap losses around 30W are only one third of excess heat!)

Electrodes and heater (the latter would probably only be needed for calibration) could be held as in Paris-1 and 2 experiments by a tripod weighed along with the cell, cf photo in http://www.lenr-canr.org/acrobat/FauvarqueJabnormalex.pdf

Periodical replenishing to approximately constant volume (level mark on the container) could be done by adding external boiling water from e.g. a saucepan on slow boil in counted constant doses of e.g. 20ml, or as in Fauvarque setup from an ambient temperature reservoir weighed along with the
cell.

What would then distinguish the MJ-1.1 proposal from Paris-2 Dewar experiment, apart from optional transparency of

the Dewar (which allows for comfortable visual adjustment of the discharge and should not increase non-evap losses by more than a few Watts I would think), would mainly consist in the added method for tracking mist and chemical heat.

80) Ludwik Kowalski wrote:
Even two or three determinations of K1/K2 (see below), under typical conditions, would be sufficient for us at this stage. Do not forget what triggered this discussion. We want to convince potential referees, of our anticipated paper, that the reported values of the COP are not due to the unaccounted for droplets. I am expecting K1/K2 to be close to 1.00. We will not claim a confirmation of the Mizuno-type excess heat if K1/K2 turns out to be significantly smaller. Do you agree, Richard and Pierre? I am assuming that Texas-2, Paris-2 and Colorado-3 results will be similar to those from Paris-1 and Colorado-2 experiments. Otherwise the paper would have to wait till we understand the situation.

Appendix ?

a) Basic assumption:
The K2CO3 dissolved in water turns into ions of different kind. One kind is K+. In this analysis we will assume that what is escaping from the open cell, during a test, is only a mixture of water vapor and tiny droplets of electrolyte. That is an approximation; the mass of the H2 and O2, due to electrolysis, and possibly of other gasses, such as CO2 and CH4, is assumed to be a negligible fraction of the water mass loss. Note potassium ions are removed from the cell by the escaping droplets of the electrolyte; the vapor escaping from the cell does not cary the ions.

b) Conducting the test:
A small amount of the electrolyte is removed from the cell with a pipette, before a test in which the COP is to be measured. That is sample #1. A comparable amount of the electrolyte is removed from the cell after that test. That is sample #2. Both samples are sent to an analytical lab to determine concentrations of K+. Note that professional laboratories perform such measurements routinely on blood and urine. Units of ion concentration can be mg/cc or ions/cc.

c) Discussion:
Suppose that sample #1 shows 100 units of K+ while sample #2 shows only 99.0 such units. That would mean that 1% of the initial K+ was lost. Any arbitrary units can be used, for example, number of ions, micrograms, milligrams, etc.

If only the water vapor is escaping then the concentration of K+ in sample #2 should be practically the same as in sample 1. In that case K2/K1 would be close to 1.00. In a mixture of vapor and drops, however, the concentration of K+ in the sample #2 would be smaller than in the sample #1. The lowest possible value of K2/K1 corresponds to a situation when nothing but liquid droplets contribute to the mass escaping from the cell. In that extreme case (only drops are escaping) the value of the K2/K1 would be (M-dM)/M. That measurable ratio will be represented by the symbol W. If M=1000 g and dM=40 g then W=0.96. It is the lowest possible values of K2/K1 for a given experiment.
.

In a mixture of vapor and droplets the K2/K1 ratio is a number between 1 and W, depending of the composition of the escaping mixture. We want to determine the composition of the escaping mixture on the basis of the actually measured W and K2/K1. Let R be a measure of the mixture composition; it is defined as the mass of tiny droplets divided by the total mass of the escaping mixture. To calculate the COP correctly we need to know R. The COP=1.2, from our Colorado-2 experiment, would be an illusion if R were close to 0.2. Assuming that the relation between R and K2/K1, for any given W, is linear one has:

R= (1 - K2/K1) / (1 - W)

As expected, this formula gives R=0 when K2=K1 (only vapor escapes) and R=1 when K2/K1=W (only droplets escape). If K2/K1 happens to be 0.988 (for W=0.96) then R is 0.3. That would indicate that 30% of the mixture consists of tiny droplets. The table below gives the expected values K2/K1 for different values of R, and vice versa. Note that the assumed W=0.96 is realistic; a typical test lasts about ten minutes and the mass lost during that interval is close to 40 grams.

R 0.010 0.040 0.070 0.100 0.200 0.300 0.500

K2/K1 0.9996 0.9984 0.9972 0.9960 0.9920 0.9880 0.9800

One thing becomes clear, to distinguish small differences in R, for example, between
4% and 7%, the K2/K1 determinations must be very accurate.

Note that the accuracy requirement becomes less severe (only 3 significant digits instead of four, as above) when W is reduced. Suppose that ten short tests, performed one after another. After each test about 40 grams of boiling water is added to the cell to keep the initial M constant. Then all ten tests can be treated as one test during which dM turns out to be close to 400. For that long test W=(1000-400)/1000 = 0.6. The relation between the R and K2/K1, for W=0.6, is tabulated below.

R 0.010 0.040 0.070 0.100 0.200 0.300 0.500

K2/K1 0.996 0.984 0.972 0.960 0.920 0.880 0.800

suppose the K2/K1 happens to be 0.984. That would indicate that R=0.04 -- 4% of the escaping mixture consists of tiny drops of the electrolyte.

d) Plan of action:
The mass of of escaping droplets must be taken under consideration when COPs are calculated. The mean COP=1.24, was calculated (after Colorado-2 experiments) under the assumption that R is very close to zero. That assumption must now be tested experimentally. Will the K2/K1 be very close to 1? I hope so. But that remains to be seen. Honest referees will probably take our claim seriously if the K2/K1 data are used to validate it.

81) Michel Jullian (posting even more simple description, publicly):
Ok so here is a revised version of the maths for the zero-hardware mist
evaluation method I propose.

NOTATIONS
- - - - - -
V Volume of solution in cell
CK concentration of potassium in cell solution
mK mass of potassium in cell
dmK mass of potassium lost (presumably via droplets)
Vd Volume of lost droplets
md mass of lost droplets (correction for droplets
to be subtracted from total mass loss)
StoK Ratio of salt mass to potassium mass
(1.77 for K2CO3 and 1.44 for KOH,
we will use 1.77 for conservatism)

RUN DATA
- - - - - -
1- BEFORE RUN
V1 (L) (will be roughly maintained during the run by adding pure water)
CK1 (g/L) (the same concentration will be assumed in the droplets)

2- AFTER RUN, RINSING OF CELL WALLS ETC WITH PURE WATER, AND MIXING
V2 (L) (a bit larger than V1)
CK2 (g/L)

CALCULATIONS
- - - - - - - -
mK1 = CK1*V1 (g)
mK2 = CK2*V2 (g)
dmK = mK1-mK2 (g)
Vd = dmK/CK1 (L)
md = Vd*1000+dmK*StoK (g)

Comments/corrections/critics welcome.

82) Ludwik Kowalski (after waiting the whole day for a reply to Michel**):**
1) Michel's method of studying wet steam -- a mixture of water vapor and droplets of the electrolyte escaping from an open cell -- seems to be good, at least in principle. It should be an essential part of our COP determinations. Fortunately, it is not too late to make the Paris-2 and the Colorado-2 claims more trustworthy. I hope that Richard and Pierre will confirm that the mass of lost droplets is only a negligible fraction of the liquid lost in a typical COP test. The percentage of the electrolyte droplets in wet steam must be reported in our anticipated paper.

2) My first comment is that the last formula (dm= ...) is not needed. Knowing Vd (volume of escaping droplets) is sufficient. I am assuming that one gram corresponds to nearly one cc for the liquid water or the electrolyte. I am assuming that what is lost during a COP test is essentially pure water vapor and droplets of the electrolyte containing K+ ions. The combined mass of H2, O2, CO2, etc. is presumably negligible. For example, if 40 grams of the liquid was lost during a test (or 40 grams of water added to the cell to keep the same amount of liquid in the cell) then the Vd= 10 grams would indicate that droplets contribute 25% to the escaping mixture. In such case the mean COP=1.24, claimed in Colorado-2, would be an illusion. I hope that the Vd for such tests will be less than 1 gram; that would validate the tentative claim that excess heat was indeed close to 24% of the inputted energy.

3) Suppose we want to distinguish the ideal 0% "mixture" from a mixture containing 2% droplets. How accurate should the determinations of the CK1 and CK2 be? Suppose CK1=4.0000*10^19 ions of K+ per cubic centimeter. This corresponds to 27 grams of the K2CO3 salt dissolved in one liter of water, as in fresh 0.2 M electrolyte. Consider a typical test, lasting 10 minutes, in which 40 grams of mass escaped from the open cell. If the escaping mixture has 2% of tiny drops then the mass of droplets is 0.8 grams (the volume is close to 0.8 cc). This allows us to calculate the CK2. Unfortunately, the CK2 is only slightly smaller than the CK1. Replacing 0.8 grams, of the escaping electrolyte, by fresh water, changes the concentration of K+ ions by (0.8/1000)*100% = 0.08%. In other words the CK2 becomes 3.9968*10^19 ions per cubic centimeter. Measuring the CK1 and CK2 at the accuracy level of five digits is probably possible in a specialized lab. Very accurate determinations of CK2/CK1 (relative concentrations from two samples, one extracted at t=0 and another at t=10 minutes) are likely to be easier than very accurate determinations of individual CK1 and CK2.

4) Michel suggested much longer tests. Suppose that ten consecutive tests are treated as one 100 min test. In such test 400 of the mixture would escape. This corresponds to 8 grams of the electrolyte (instead of 0.8 g). The final CK2 would be 0.8% smaller than the CK1 (3.967*10^19 ions per cc). Under such condition the accuracy of only four significant digits would be sufficient to distinguish the wet steam containing 2% of droplets from the pure vapor.

5) The earlier suggestion of Michel -- capturing the escaping mixture and condensing the two samples (to determine the CK2/CK1 ratio) -- seems to be more attractive. In that case the accuracy at the level of two or three significant digits would be sufficient to distinguish the 2% mixture from the ideal 0% mixture. Measuring the CK2/CK1, in an open Mizuno-type cell, can be decoupled from the COP measurements. I already wrote how I would do this by using a large funnel. Jed's objection to the use of ice (to keep the funnel cold) would be valid if the COP were to be measured. But it is not valid for an experiment whose purpose is evaluate a typical steam wetness. The wetness of a sample, R, is defined as the ratio of the mass of electrolyte droplets and the total mass of wet steam. That is what we need in order to calculate the COP correctly. The description of my cold-funnel condenser can be found in the message #54 above.

Michel wrote that medical laboratories routinely measure concentrations of potassium ions. How accurate are these

measurements? In what units are medical concentrations expressed? Should a typical chemist be expected to know how to measure the CK2/CK1 ratios (at the level of ~3% accuracy) in an improvised lab?

82) Michel Jullian:
. . . The last formula md = Vd*1000+dmK*StoK (g) is needed actually, to convert Vd (in liters) to the mass of escaped droplets (grams) which is what we are after. My conversion accounts not only for the mass of the water but also for the mass of the salt, as it should (even if it doesn't make much difference we might as well use the exact formula since it is quite simple)

Since Vd=dmK/CK1 the final formula can also be written md=dmK*(1000/CK1+StoK), which e.g. for 0.2M K2CO3, i.e. 0.2 mole of K2CO3 per liter, i.e. 0.4 mole of K per liter, i.e. CK1=0.4*39.1 g/L = 15.64g/L, would boil down to: md=dmK*(1000/CK1+StoK)=dmK*(1000/15.64+1.77). As expected we find: md=65.71*dmK (66*dmK will be used to compute accuracy below)

[You asked] "How accurate are these measurements?" We can see by ourselves in our own blood tests. Potassium is an important blood electrolyte, and it's concentration in routine blood tests is given to the hundredth of a millimole per liter (e.g. potassium (mmol/L) 4.47). Since we have about one liter of solution in the cell, we would know potassium mass to the hundredth of a millimole, i.e. to 0.39 mg (molar mass of K being 39g or so). Even if the error on dmK was five times that, it would only be about 2mg, so the error on the total escaped droplets mass would be only about 66*0.002 ~ 0.1g which you will admit is quite accurate!

83) L. Kowalski:
My recent blood report shows 4.2 mmol/L of potassium. The normal range is 3.5 to 4.5. I do not know how accurate the 4.2 result is. Several results (for the blood) would be needed to estimate the standard deviation. My guess is that it could be as large as 0.3, for a routine medical test. That would be 7%. By the way, the unit of concentration should be mmol, not the mmol/L. Do you agree? It could also be grams per liter or number of molecules per liter, or mg/L, or mg/cc, etc.

Suppose that the first sample has no droplets at all, its potassium concentration, CK1, is 0.000 mmol. Suppose the second sample has 100% of droplets. What is the potassium ion concentration, CK2, in the second sample? It is the same as in the original electrolyte. That amounts to 0.4 mol = 400 mmol (27.6 grams of K2CO3 are dissolved in one liter of water to produce a 0.200 M solution of the salt). A wet steam containing 1% of droplets would have 100 times smaller molarity -- 4 mmpl. That is about the same as for my blood. For the third sample, containing 2% of droplets molarity would be 8 mmol, etc. A difference in the potassium concentration resulting from only 1% difference in the steam wetness seems to measurable with automated medical analyzers. That is good news. But such analyzers could not be used when samples of the electrolyte are collected from the cell (before and after an experiment), as illustrated yesterday.

Condensing several grams of wet steam, with a cold funnel test, should not take longer than two or three minutes. That would be sufficient for several samples needed to establish the accuracy at which potassium concentrations are measured. My advice to Peter, Richard and Scott is to perform the cold funnel test, and to interpret it in the context in which the test was originally suggested by Michel. He is now preoccupied by other nuances but this should not delay a simple test described above. Let the perfect not be the enemy of the good.

Appended on 4/13/06:
On Apr 12, 2006, Michel asked:

Do you agree now that 0.025 mmol/L accuracy on CK1 and CK2 yields
about 0.1g accuracy on escaped mist mass if volumes are exactly
known and of the order of 1L?

1) What are we discussing? It is a method of measuring wetness of the steam escaping from an open cell during a Mizuno-type experiment. I agree with Jed (and others) that this problem can be eliminated by using the closed cell method. But that was the method we used. Paris-1 and Colorado-2 experiments were performed with open cells and

we want to know the composition of the lost liquid (a mixture of water vapor and droplets of the electrolyte). The mass of the H2, O2, CO2, etc. was considered to be negligible. That is how the problem was originally formulated by Michel. If droplets of the electrolyte represent ~20% of the escaping mixture then the reported COP~1.20 can be explained as an artifact. On the other hand, if the percentage of droplets is close to zero then the reported COP~1.20 should be taken more seriously.

Two methods of measuring the percentage of droplets have been proposed by Michel. Which of these two methods is more desirable, from the point of view of accuracy? That is the issue.

a) Method 1: compare the concentration of K+ in two samples of the electrolyte, one extracted from the cell before the plasma electrolysis experiment and another extracted after the experiment. If two concentrations are identical then only a negligible fraction of the escaping mixture consists of droplets.

b) Method 2: compare the concentration of K+ in two samples of condensed mixture. One when the ohmic heater is used and another when plasma electrolysis is in progress. If the two two concentrations are identical then only a negligible fraction of the escaping mixture consists of droplets.

2) The numerical example below will show why I am in favor of the method (b). To prepare a 0.2000 M solution of K2CO3 we dissolve 27.000 grams of the salt in 1000.0 grams of water. Five digits of accuracy will be used in my arithmetic. The original concentration of K+, based on known atomic weights of K, C and O, is:

27.000 * (2*39.098 / 138.19) = 15.278 grams of K+ in 1015,3 grams of the electrolyte.

In other words, the original concentration, CK1 is 15.048 units (the units are grams of K+ per 1000 grams of mixture). That is the value CK1 for the method (a). Suppose a test, lasting 10 minutes, was conducted and t50.000 grams of the mixture escaped. To compensate for this we add 50.000 grams of water to the cell and measure the CK2. What is the expected value of CK2 when 20% of the mixture consists of droplets of the electrolyte? The fraction of the K+ removed during the test is (50 / 1015.3) * 100% = 4.9246 %. That amounts to 0.75238 grams of K+. The new mass of K+ is (15.278-0.75238) = 14.526 grams of K+. It is dissolved per 1015.3 grams of the mixture. Thus the CK2= 14.307 units.

4) Now let me compare the CK1 and CK2 for the second method. The CK1=0.0000 because no K+ is expected to be found in condensed "mixture" when droplets represent 0%. And what is the value of CK2 when 20% of the mixture consists of droplets of the electrolyte? We now have 0.75238 grams of K+ in 50 grams of condensed mixture. That gives us CK2=0.015048. Distinguishing 0.0000 from 0.015048 is likely to be much easier than distinguishing 15.048 from 14.307.

5) To summarize let me show a table of expected values CK for different percentages of droplets in the mixture, first for the method (a) and then for the method (b). Units of concentration are g/1000g, as explained above.

drops --> 0% 1% 2% 3% 4% 20%
==========
CK (a) --> 15.048 15.011 14.974 14.937 14.900 14.307
CK (b) --> 0.0000 0.0015 0.0030 0.0045 0.0060 0.0300

The table shows that five digits of accuracy would be needed to measure concentrations from the method (a) and only three digits from the method (b). That is why I wrote that the method (b) is more desirable. It is easier to deal with situations in which what we want to know (% of droplets) is directly proportional to what we actually measure. Note that numbers in the last line double and triple with doubling and tripling of the corresponding numbers in the first line.

6) When Richard and Pierre send me samples (one from condensing the mixture during the ohmic heater boiling and one from condensing the mixture during the plasma electrolysis) then I will try to measure their concentrations on the basis of conductivities. To do this I will build a small cell with two Pt electrodes. The current flowing through the cell is likely to be directly proportional to CK, provided the voltage (such 0.5 V) and temperature (such as 20 C) remain

constant. I will calibrate that cell by using several electrolytes of known concentrations. Should I anticipate problems with this simple approach?

Yes, I know that other ions, if present, would also contribute to the current. The conductivity test is not selective; the current will be proportional to the total concentration of all cations. But suppose the two currents are practically identical for my two samples. In that case it would be possible to say that the contribution of droplets was negligible during our experiments.

84) Michel Jullian wrote:
Another possible chemical fuel which could be supplied indefinitely by the atmosphere: nitrogen (80% of ambient air). As Bill Collis rightly mentioned in the Jules Vernes thread: "Ah but one of the benefits of burning in pure oxygen rather than
air is that you cannot form any polluting nitrogen oxides (NOx)."

My scenario for this other possible Mizuno artefact: **(1)** atmospheric N2 dissolves spontaneously in the electrolyte solution (SCUBA divers know this phenomenon quite well) **(2)** N2 in solution is convected towards the plasma area **(3)** N2 burns in the high temperature plasma where explosive O2+H2 recombination occurs, forming NOx (a good soul with one of those rubber bibles may tell us how much heat this produces per gram of burnt N2 **(4)** GOTO (1) I wonder how this artefact could manage not to happen if the cell is exposed to ambiant air BTW. Another good reason not to let atmosphere interfere with the experiment isn't it?

85) Ludwik Kowalski:
As shown in unit #290, several people think that Colorado 2 experiment was a waste of time. I have no idea how to estimate the amount of excess heat generated by N2 reactions. Why should these reactions produce 24% of excess heat and not, for example, 50% or 500 %?

Not being a chemist I will not be able to address every possible objection. Perhaps it was a mistake to go ahead without a good chemist. The goal was to perform an experiment as in Paris 1. We did this and we got similar results. But this will not be enough for referees. A paper without convincing evidence, against chemical origin of excess heat, presented by a reputable chemist, is likely to be rejected. I would not criticize the editor for this. It is too bad that original results of Mizuno, or results of numerous reports from Naudin, were not discussed, as far as i know, in terms of exothermic chemical reactions. It is too bad that nobody waned me about the issue of safety after units #252and #253 was posted.

86) Michel Jullian:
Group, I was quite glad to find out, thanks to a Google search, that one can do thermochemistry without one of those rubber covered chemistry handbooks and associated expertise and tedious calculations :)

I was able to check the hypothesis in my previous post, and more, with a trial version of CHEMIX ("lab" edition, available at http://home.c2i.net/astandne/), a very helpful tool whose thermochemistry page could be dubbed "thermochemistry for dummies", so simple that even it being in Norwegian language was not an obstacle to using it! (I must have missed the language option at download or install time).

Example of use, you type in the following unbalanced reaction (electrolysis of water):

H2O(l) > H2(g) + O2(g)

it balances it for you (works out the coefficients), works out the enthalpy change, and outputs:

2H2O(l) = 2H2(g) + O2(g) - 571.6kJ

meaning that the reaction is endoenergetic, 1 mole of the elementary reaction requiring 571.6kJ of energy (electrical or thermal or whatever) You can even get it to display the energy for a given mass of either a reactant or a product, e.g. you type: 1gH2O(l) > H2O(g) and it outputs:

H2O(l) = H2O(g) - 2.44237kJ

meaning vaporizing 1 g of liquid water requires 2442J (and not 2260J as we have been using, can anybody explain this discrepancy BTW?)

The various Nox (NO or NO2) forming reactions that I tried turned out to be globally endothermic (absorb heat) so we probably don't have to worry about those.

A more preoccupying reaction is the spontaneous and exothermic (produces heat) reaction between N2 and H2 forming ammonia gas:

N2(g) + 3H2(g) -> 2NH3(g) (+ 15.1795kJ per g of H2)

but fortunately this produces much less energy per g of H2 than was used to produce the same gram of H2 by dissociation of water (e.g. thermal dissociation):

2H2O(g) -> 2H2(g) + O2(g) (- 119.948kJ per g of H2)

so we don't have to worry about this reaction either. However the possibilities of reactions between the various substances in play being quasi infinite it would be very presomptuous to claim none of the reaction paths is globally exothermic.

Whereas preventing the atmosphere and it's unlimited supply of various substances from interfering with the experiment, and still getting over hours/days/months much more excess heat than could possibly be produced chemically by the precisely known limited amount of substances we will have deliberately put into the cell, would make the claim of non-chemical origin of the excess heat much stronger obviously.

About the method 3 I suggested below to achieve this (keeping the solution boiling at all times to maintain an outgoing flow of steam), of course the cell opening would have to sufficiently narrow, and the liquid level sufficiently low, to make sure the outgoing steam constitutes an efficent barrier against atmospheric gases contacting the liquid surface.

87) Michel Jullian:
Hi John. Yes, liquid and gas H2O are at same temperature (100°C). I have found that the discrepancy comes from formation enthalpies variations with temperature being significantly different for liquid and gas between standard temp 25°C and 100°C.

According to
<http://webbook.nist.gov/cgi/cbook.cgi?Formula=H2O&MatchIso=on&NoIon=on&Units=SI&cTG=on&cTC=on>
the standard (25°C) formation enthalpies are:

DH0f H2O(g) -241.83 kJ/mol
DH0f H2O(l) -285.83 kJ/mol

So the standard enthalpy change is +44kJ/mol, which for water's 18.02g/mol is 44/18.02=2442J/g as given by CHEMIX.

But if you now take into account the temperature dependencies, also given in the NIST page, you find the actual enthalpy change at 100°C is only about +41kJ/mol (based on rough interpolations from their tables: gas DH0f increases by about 2.5kJ, whereas liquid DH0f increases by about 5.5kJ, so gas minus liquid decreases by about 3kJ), which is 41/18.02=2275J/g very close to the 2260J/g we have been using. So 2260J/g must be the correct value for 100°C evaporation heat of water indeed.

This shows that the usual approximation that enthalpies do not depend on T can lead to significant inaccuracies in

some cases. What would be needed is a super-CHEMIX which wouldn't make such approximations.

P.S.
Powerful consequences of the proposed "permanent one way steaming" scheme with very long duration runs:

1/ After a while, the only gases that can possibly escape the cell in meaningful amounts are water vapor, hydrogen and oxygen. 2/ The long duration of the run renders the potassium mass loss significant if there is any significant salted mist escaping, which makes it easily measurable.

1/ and 2/ make the mist-corrected evaporated mass calorimetry method simpler, easier and more convincing than ever. For example it is not needed anymore to explain why mass or energy balance of other gases such as CO2 cannot overestimate the COP, since there are no other gases. All there is to explain is that our counting of H2 and O2 escaped mass as evaporated H2O mass only underestimates the COP since it takes much more energy to dissociate 1g of water than to evaporate it.

Of course for the runs to last hours/days the necessary cathode replacements, whether continuous or not, have to be part of the run. If we keep using the easily procured 2.4mm welding sticks we could consider forming them beforehand by operating them in short preliminary dummy runs (e.g. the day before) so they are already suitably pointed and cracked when they are inserted into the experiment, ready to produce the right kind of discharge right away.

Regarding my proposal of an immersed smoothing filter to make input power and energy measurement easier and more convincing by suppressing spiky i and v waveforms and associated EMI, it has occurred to me that only the most dissipative part of it (the resistor) really has to be immersed, as neglecting the comparatively small losses in the capacitor and inductor only underestimates the COP, and only slightly. This simplifies things by allowing the use of the already included ohmic heater as the filter resistor at electrolysis time, and suppressing the need for any additional submersed component: the cap and inductor just have to be located in an EMI-shielded area, e.g. a grounded Faraday cage made of screen which would also enclose the cell.

The pieces of the puzzle seem to all come together nicely, making my dream of a both simplistic and indisputable Mizuno experiment less unrealistic than it was.

Michel Jullian (private message):
I know our common objective is to prove to anyone interested -not just the converted few but also honest scientists and politics- that the Mizuno excess heat is (a) indisputable and (b) indisputably abnormal. This should have been done long ago by Mizuno himself or others but hasn't. To fully realize this objective, we both know that some changes are required wrt existing experiments.

I have proposed yesterday what is probably a minimal (let me know if you agree on this qualifier(*)) set of such changes wrt the nice and simple Fauvarque-ICCF12 version of the experiment run in Paris and Colorado, in my "making the puzzle fit together" post. Note the cell remains _physically open_ in this proposal so it can't turn into a bomb a la Mizuno ICCF12 (no open cell has ever exploded I don't think, correct me if I am wrong)

What this proposal needs now (if it works, and even more if it doesn't so we can quickly find it's flaws and move to a better one without regrets) is commenting and criticizing by the group so an unbiased and argumented consensus can be reached wrt it's fitness to achieve the objective.

88) Richard Slaughter (private):
Ludwik, Good morning hope all is going well. I'm still waiting on getting my boards back so I can't measure the input power correctly. I've designed a new cell that is looking very promising. The new cell is closed except for a 0.5 inch hole in the top to let out the H2 02 gas. I've put a (condenser) coil of tubing just below the lid of the cell. Between the lid and the coil are several layers of stainless steel screen. The screen is in contact with the tubing so it stays the temperature of the tubing.

I've a large ice chest with ice water and I'm running the ice water though the condenser. Using my 0.25 inch tungsten rod I'm able to run the cell for an hour before the tungsten burn away. The water in the ice chest is staying at 1C for duration of the test so I only need to measure the outlet temperature of the condenser and the flow rate to get my output power. The outlet water from the condenser circulates back into the ice chest. I spray it as a fine mist back into the ice chest so it looses a lot of heat to the air.

The temperature of the electrolyte is about the same as before 80C. When I run the test there is very little loss in mass. Looks like input power is going mostly into making steam and the steam is immediately condensing on coil of tubing. Haven't got any real data yet but I'm encouraged. If I can make this type of cell run for several hours at equilibrium with excess heat it should put to rest most of the objections were hearing on CMNS.

89) Michel Jullian:
The changes wrt Fauvarque-ICCF12 in the new proposal (let's call it BM_MJ-2.0, BM for boiloff Mizuno) are really few and simple, I'll summarize them here:

- insulated cell with a not-too-wide and not-too-low opening (e.g. same double walled reactor as in JLN's enhanced CFR, only with a closed airspace in the double wall) - a couple protocol changes to prevent ingress of known (CO2, N2) and unknown reactive atmospheric substances, make the runs long-duration (hours/days so any initial chemical fuel is exhausted), and correct evaporated mass for escaped mist (note long runs make the cell potassium concentration method more accurate)

- plus an _optional_ hardware change which boils down to adding an inductor and an EMI shielding screen and doing some rewiring to make electrical measurements totally convincing, even if done with a dumb AC mains watt-hour meter which anyone can use and read.

Note the cell remains _physically open_ in this proposal so it can't turn into a bomb a la Mizuno ICCF12.

What this "minimal change" proposal needs now (if it works, and even more if it doesn't so we can quickly find it's flaws and move to a better one without regrets) is commenting and criticizing by the group so an unbiased consensus can be reached wrt it's fitness to realize it's objective.

It's objective being what I assume to be a common objective of this group: proving to anyone interested -not just the converted few but also honest scientists and deciders- that the Mizuno excess heat, and hence cold fusion excess heat in general, is (a) indisputable and (b) indisputably abnormal. As I suggested before, simpler solutions should be discussed/eliminated first, because they are more likely to realize the above objective than more complicated solutions. Or am I missing something?

90) Richard Slaughter:
MIchel,
I'm working on a design for a new cell. I'm trying to incorporate as much as I can about what has been said here. I think to have an indisputable experiment we need to do the following. The cell has to be completely closed. A high speed analog measurement of input v(dt)*i(dt) something like the AD835 multipllier chip. The H2-02 needs to be recombine inside the cell. The cells need to run at equlibrium for at least 1 hour producing excess heat.

I think this can all be accomplished and still have as simple a system as used in the Fauvarque experiment. The key is to condense the steam inside the cell at the same rate it is being generated. I propose we put a liquid to air condencer inside the cell. The condencer only has to disapate about 300Watts to bring the system into equlibrium. A constant temperatue ice bath provide cooling water to the condenser. We only need to measure the output temperatue and flow through the condenser to calculate output power. I'm not sure of the best way to recombine the H2-02. I'm thinking of using a spark plug or glow plug. The whole cell can be well insulated so the only heat output is via the condenser.

If flow though the condenser is 0.5 liters/min and we have a temperature rise of 15C that's over 500 watts. This seems reasonable approach.

91) Michel Jullian:
Richard, I will be glad to discuss your new proposal (RS-2.0?). Shall we discuss mine next then?

92) Richard Slaughter:
Michel, Lets dicuss yours first. I concerned that as long as you are trying to do a boil off it will be difficult to keep the system running for hours or days? Replinishing of the lost water greatly adds to the complexity of calculated the heat loss. Can the critics say that fresh water is adding "fuel"? I tried to replinish the water for a while and found, it difficult to maintain the fresh water temperature because of the output steam. I'm sure this problem can be solved. The boil off approach is always going to be suseptable to changing conditions inside the lab such as Temperature, humidty and pressure. I don't see the system ever reaching an equlibrium where COP is constant. If we can devise an experiment where the COP is greater than 1 and constant for hours or days that is going to be very convincing.

You comments on the EMI sheild and computing input power are correct. I think this is just a case of selecting and building the correct hardware. The other problem with all system is how to keep the W rod active for hour or days? I see two approaches to this problem. One is to build a system that auto-feeds the electrode. The other is devise an experiment that can reaches equilibrium quickly and thus doesn"t need as much electrode. My Musing, Richard

93) Michel Jullan:
Richard wrote: Lets dicuss yours first. I concerned that as long as you are trying to do a boil off it will be difficult to keep the system running for hours or days? In any non-automated scheme, boiloff or otherwise, hours will be easy, days will require taking shifts like at the helm of a boat... or more appropriately like at the controls of a power plant :)

Replinishing of the lost water greatly adds to the complexity of calculated the heat loss. Easy, we add water from pre-weighed bottles and weigh the last unfinished
bottle to know how much pure water we have added.

Can the critics say that fresh water is adding "fuel"? No :) Unless we add gasoline or whisky to it to improve our COP ;) Using water as a fuel to produce heat would be quite an achievement anyway.

I tried to replinish the water for a while and found it difficult to maintain the fresh water temperature because of the output steam. I'm sure this problem can be solved. Let's keep the bottles away from the steam.

The boil off approach is always going to be suseptable to changing conditions inside the lab such as Temperature, humidty and pressure. No, lab conditions have no influence whatsoever on this scheme since non-evap losses are small by design (<30W) and therefore can be neglected altogether if we can sustain a COP>1.1 at 300W. This prevents any objection based on bubbles or foam reducing those losses since we already assume they are null!

I don't see the system ever reaching an equlibrium where COP is constant.If we can devise an experiment where the COP is greater than 1 and constant for hours or days that is going to be very convincing. Indeed, it will be very much like a car going on for ever without ever refueling.

You comments on the EMI sheild and computing input power are correct.I think this is just a case of selecting and building the correct hardware. Actually an off-the-shelf residential electricity meter (joule or watt-hour meter) working from 100-400VAC installed between the variac (or insulation transformer) and the rectifier will be perfect with a proper smoothing filter, which only requires adding an inductor to existing setups and doing some rewiring to use the ohmic heater as the filter's resistor at electrolysis time ; flipping a multipole switch to go from electrolysis mode to heating mode will put the heater in the right circuit.

The other problem with all system is how to keep the W rod active for hour or days? I see two approaches to this problem. One is to build a system that auto-feeds the electrode. The other is devise an experiment that can reaches equilibrium quickly and thus doesn"t needas much electrode. As I said in an earlier post we can also keep using the easily procured 2.4mm welding sticks. I suggested to pre-form them (by operating them in a "dummy" glow discharge before the actual run) so they can produce a high COP immediately when inserted into the system. While changing the

cathode, which won't take long with a proper holder/connector, we switch to ohmic heating mode to keep the vapor barrier active, and then flip back to electrolysis mode.

More constructive comments/criticisms please, it seems we haven't succeeded
in ruling out this scheme yet and there are others awaiting discussion so we must validate it or debunk it fast.

94) Ludwik Kowalski:
Colorado2 experiments replicated the COP>1 claim of the Paris-2 team. They showed that by doing the same things we obtain similar results. That was an important step forward. To make Colorado-2 results publishable we must be able to convince honest referees that excess heat cannot be explained in terms of known chemical reactions. That is the main issue. Unfortunately, it has not been addressed by recognized authorities in chemical aspects of high voltage electrolysis. Did Mizuno, or some of his collaborators, publish something along these lines in Japanese?

My understanding was, and still is, that a complete analysis of potentially possible exothermic chemical reactions is now being conducted in Paris. Im I correct? Planning for new setups, ahead of such analysis, does not seem to be productive, at this stage. Writing a paper based on Colorado-2 results also seems to be premature, for the same reason.

But we should compare the conductivity of water (from which the electrolyte was made) with conductivity of condensed liquid escaping from the cells (when the COP was measured). In that way we can estimate the percentage of tiny droplets of the electrolyte in the escaping steam. Several people suggested that the COP>1 might be due to our inability to distinguish between water vapor and droplets of the electrolyte. Yes, this problem would be eliminated in a closed cell, as illustrated by Jed. But Paris-1 and Colorado-2 experiments were performed by using open cells. Our anticipated paper will describe these experiments, and results should be made trustworthy.

95) Richard Slaughter:
Michel, If the goal you've set for your experiment is to make the existence of excess heat "indisputable, and indisputable abnormal". I think that it is essential that your data colection system run continuous for hours or days. You must collect the input and output power measurments at least once a minute and ideally once a second. You must be able to add water to your system and adjust the electrode without disrupting the data stream and keeping the reaction going. In the French 1 and Colorado 2 experiments this was one of the shortcomming. We could only collect continuous data for 5 to 20 minutes so we could only say we had excess heat for a 5 to 10 minutes

I think from an engineering standpoint it will be very difficult in the boil off experiment to maintain the reaction and the continuous data stream. Any disruption in the system seems capable of stopping the production of excess heat. In Colorado we would add hot water to replinish the lost water so our down time was a minimum. Still adding the hot water could stop the reaction. Adding cold water always stops the reaction. Adjusting the electrode required turinng off the input power and killing the reation.

The continuous data stream is essential to the primary goal of being indisputable. If you can show that excess heat continues for hours or days it's hard to argure that it is chemical in nature. It is this practical problem of keeping the reacting going for hours or days that is the biggest downside I see to the boil off experiment. If you have a engineering solution to add water and adjust the electrode so the reaction continues that's great. I don't see a way and keep the experiment simple. That why I'm leaning toward the closed cell approach. Richard

96) Michel Jullian:
Hi Richard, my comments in your text below, feel free to do the same.

If the goal you've set for your experiment is to make the existence of excess heat "indisputable, and indisputable abnormal". I think that it is essential that your data colection system run continuous for hours or days.

Long runs would be nice, highly desirable even, but are not essential in my proposal. Short runs as you have been doing would be still ok in this scheme: provided the vapor barrier is not interrupted in-between, there can be no ingress of potential chemical fuels.

You must collect the input and output power measurments at least once a minute and ideally once a second.

I was thinking of collecting them permanently by integration: measuring cumulated input energy (watt-hours from a simple electricity meter working on the digital multiplier principle, that's how recent meters work it seems) and cumulated output energy (evaporated mass), and reading them as often as we can so we get a smooth curve, but without this being an obligation: if there is no reading for 2mn it doesn't affect the run's global COP.

You must be able to add water to your system and adjust the electrode without disrupting the data stream and keeping the reaction going. In the French 1 and Colorado 2 experiments this was one of the shortcomming. We could only collect continuous data for 5 to 20 minutes so we could only say we had excess heat for a 5 to 10 minutes

I think from an engineering standpoint it will be very difficult in the boil off experiment to maintain the reaction and the continuous data stream. Any disruption in the system seems capable of stopping the production of excess heat. In Colorado we would add hot water to replinish the lost water so our down time was a minimum. Still adding the hot water could stop the reaction. Adding cold water always stops the reaction. Adjusting the electrode required turinng off the input power and killing the reation.

All this is useful information Richard. Why did you have to turn off the input power BTW? The cathode was grounded wasn't it?

The continuous data stream is essential to the primary goal of being indisputable. If you can show that excess heat continues for hours or days it's hard to argure that it is chemical in nature. It is this practical problem of keeping the reacting going for hours or days that is the biggest downside I see to the boil off experiment. If you have a engineering solution to add water and adjust the electrode so the reaction continues that's great.

Replenishing with boiling water is one thing. Weighing it would be trickier but we could use the magnitude of the steps on the scale readings to know precisely how much we have added couldn't we? To adjust the electrode, how about holding/connecting it in a sliding arrangement so it could be tapped gently with some tool to push it down a few mm at a time?

I don't see a way and keep the experiment simple. That why I'm leaning toward the closed cell approach.

I don't follow your reasoning. A closed cell would make things even more difficult wrt the above engineering questions than an open one it seems to me. Also what would be the advantages of the closed cell that the one-way scheme of the vapor barrier doesn't offer? They would have to be worth the risk of explosion...Michel

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289) A different claim for excess heat

Ludwik Kowalski; 4/2/2006
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This item is based on what has been reported in 2005 by Sterling D. Allan and Adrian Akau, in Pure Energy Systems News. Here is the URL for this article:

http://pesn.com/2005/06/26/9600116_Naudin_MAHG/

1) The MAHG stands for "Moller's Atomic Hydrogen Generator" that Jean Louis Naudin is experimenting with. Alexander Frolov, from St. Petersburg, is another inventor of this energy producing device. His email message, quoted in the article, states: "I am happy to see independent positive confirmation of my invention. Moller knew nothing about this way but he proposed to work on this topic and he paid 50% of cost. Also we have contract about 50-50 on the topic (including authorship)." Frolov also tells us that testing of the device is planned in Australia.

2) The excess energy, according to Moller, is somehow released when molecular hydrogen is turned into the atomic hydrogen and then back into the molecular hydrogen. Any chemist would say that this is not possible, unless the excess energy comes from another source? What is the source? In the case of CMNS that source is believed to nuclear reactions. Moller seems to thing that empty space supplies "free" energy.

3) As I wrote in another context, it is pointless to speculate about sources of excess heat before reality of that excess is validated. Naudin claims to generate excess heat much more efficiently then those who performed Mizuno type experiments. The article refers to the 526% efficiency. That implies that 5.26 joules of energy appears at the output for every joule that enters the system to sustain the process. Referring to this result the authors write: "This is the first known replication and collaboration of the work done by Nicholas Moller in conjunction with Alexander V. Frolov of Faraday Lab Ltd. of St. Petersburg, Russia in January 2003." Here is how that collaboration is described by N. Moller (February 2004) on Naudin's website <http://jlnlabs.imars.com/mahg/mahg1.htm>

"After having studied in detail, the work on atomic hydrogen that Irving Langmuir performed 80 years ago (see article Irving Langmuir & Atomic Hydrogen), Mr. Nicholas Moller of Spectrum Investments Ltd decided to experiment with Langmuirs findings in greater depth. To this effect Spectrum Investments Ltd. contracted with Faraday Lab Ltd. of St. Petersburg, Russia in January 2003 in order to verify Langmuirs proof of concept. After a constructive co-operation, Mr. Alexander Frolov, Director of Faraday Lab Ltd. confirmed Mr. Moller's assumption that recombination of atomic hydrogen can be used as physical mechanism for the operation of a practical high efficiency heat source/generator.

The main point was to determine what method of the hydrogen dissociation could be efficient. Several different methods (plasma welding, Pd proton-exchange membranes, high-frequency glow discharge and direct heating) were discussed. Agreement was reached on the design of a test bench with a special gas-filled tube, based on a standard design of a powerful vacuum tube. Hydrogen filling of 0.1 atm and dew point -60 C degrees was agreed with sub-contractors and design for the tube was completed (see photo and diagram below)." The diagram of the Moller/Frolov device is shown but my background is not sufficient to understand it. The text below the diagram uses familiar technical terms but it is not clear to me at all. A lot of words and numbers but not enough of the "logical glue" to make the text comprehensive. I wander what more knowledgeable people think about clarity of this description. The link to

“full construction diagrams” produced very impressive drawings but that is not what one needs to understand the device.

4) Fortunately, the article provides a layman-oriented section whose title is “Mechanism.” The device is essentially a flow calorimeter; water removes heat generated in the inner cell. That cell is filled with hydrogen at the pressure of 0.1 atm. Presumably, the inner setup is a glow discharge cell whose cathode is tungsten. The hydrogen is not consumed; it undergoes cyclic conversions from molecular form to atomic form and then back from the atomic form to the molecular form. In the first path the energy is consumed while in the second path the energy is released. The excess heat is the difference between the energy needed to decompose the H2 into two H atoms, and the energy released when two atoms of H form the H2 molecules.

The voltage between the anode and the cathode, about 250 V, must be pulsating at the frequency of 10 MHz and shape of each pulse is said to be important. This reminds me the super waves used by Israeli researchers. “The shape and length of time for the pulse might be the most important part of the experiment”. Why should frequency and shape be important? I suppose it has something to do with natural (resonance) frequency of H2 molecules. But these frequencies, as far as I know, are much higher than 10 MHz. High harmonics are likely to play a significant role in breaking the H2 molecules. The technical term for this process is “dissociation.” Its probability increases rapidly with the temperature, as demonstrated, in 1926 by Langmuir. Practically no molecules dissociate below 2000 K and nearly all molecules dissociate above 8000 K, without super waves.

5) The section entitled “Open source project” is also interesting. It describes what motivates researchers and provides additional information. Here is one quote: “This technology is nearly 70 years old, and is not likely to be patentable. (A patent was obtained by Frolov in Russia, but a source that request to remain anonymous states that it is probably not valid). It appears to be a feasible candidate for the ‘open source’ model of development by interested scientists worldwide.” And here is another quote: “Nicholas Moller, one of two individuals after whom this variant (2.0) device is named, said, ‘The time has come for removing the responsibility of defining the energy policies and structures of the future from the hands of the established energy industries, and place it in the hands of men of science who will work for the preservation of the environment that sustains all life on Earth.’ "

Impressive data from recent Naudin’s experiments are shown in the section entitled MAHG. In one experiment (May 2005) the difference between the output power and the input power incased nearly linearly when the input power. It changed from 50 W at 100 W of input, to 1000 W at 500 W at the input. In other words, the efficiency at the input power of 500 W was 300%. Generating excess heat at the rate of several hundred watts is remarkable. On June 23, 2005 the efficiency was 1153%. The device is referred to as an energy pump. I suppose that this term reflect the believe that energy is being pumped from the world of nothingness to the world of atoms and molecules. The effect of the duty cycle, for square pulses, on efficiency was demonstrated by Naudin. The efficiency increases significantly when 10 MHz pulses become shorter. Note that for shorter pulses (5% of each cycle) the frequency spectrum is wider and more input energy is delivered in the form of high harmonics than for the longer pulses (40% of each cycle).

A fast electronic switch was used by Naudin to control the the duration of pulses from a simple d.c. source. I wander what the efficiency would be if a very sophisticated power supply, such as Pinnacle (see Appendix in the unit #270) were used instead. As far as I know, Pinnacle can be programmed to deliver all kind of superwaves. It is worth mentioning that plasma, in Mizuno type experiments, also acts as an electric switch rapidly modulating the electric power. The modulation is random and the frequency spectrum is very broad. Can this also be significant in terms of what happened near the very hot tip of our tungsten cathode? Karabut, whose experiments are described in the unit #13, also used a pulsating power supply. This is another independent (?) indicator that high frequencies might play a significant role in production of excess energy. I am thinking in terms of resonance frequencies of the oscillating H2 molecules (or groups of molecules) and about moments at which distances between their atoms are somehow reduced.

6) And here is a quote from the last section. “Once others have been able to repeat these results, and the effect fully characterized, explained, and optimized; the big question will be how practical this technology will be as a free energy tapping modality. Solar energy is free energy, but the hardware required to convert photons to electricity is anything but free. Likewise, even though this MAHG technology apparently taps free energy, does not automatically mean that it will solve the world's energy problems. The machine cost and efficiency will be a primary consideration in the

practicality of the technology for commercial application. Preliminary results and extrapolations appear promising for the MAHG. Further studies and development will answer that question more definitively.

In an email granting permission to use the images that appear in this article, Naudin said, ‘I hope that my modest contribution of my researches about this fascinating device will soon give us a clean energy source for the future and will contribute to saving our planet.’ " I hope his expectations will be fulfilled, sooner than later. Those who are interested in Naudin’s investigations of MAHG devices should refer to this website:

http://jlnlabs.imars.com/mahg/index.htm

Also worth reading is Moller’s article entitled “Irving Langmuire and atomic hydrogen.” It is essentially a biography of Irving Langmuire, a Nobel laureate who died in 1957. The article can be found at:

http://jlnlabs.imars.com/mahg/article.htm

The biography ends with speculations about how Langmuire would interpret heat released by atomic hydrogen (when it turns into molecular hydrogen) if he were familiar with the idea of the infinite amount of free energy from the so-called “emptiness.” It is ironic that Langmuir’s name is also associated with criticizing pseudoscience. Would he be declared a pseudoscientist if he decided to confirm Naudin’s findings? I do not think that Langmuir would speculate about free energy before convincing himself, experimentally, that claimed effect is not an illusion (due to experimental errors or well known processes). After convincing himself he would probably focus on convincing others.

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290) Attitudes toward our ongoing project

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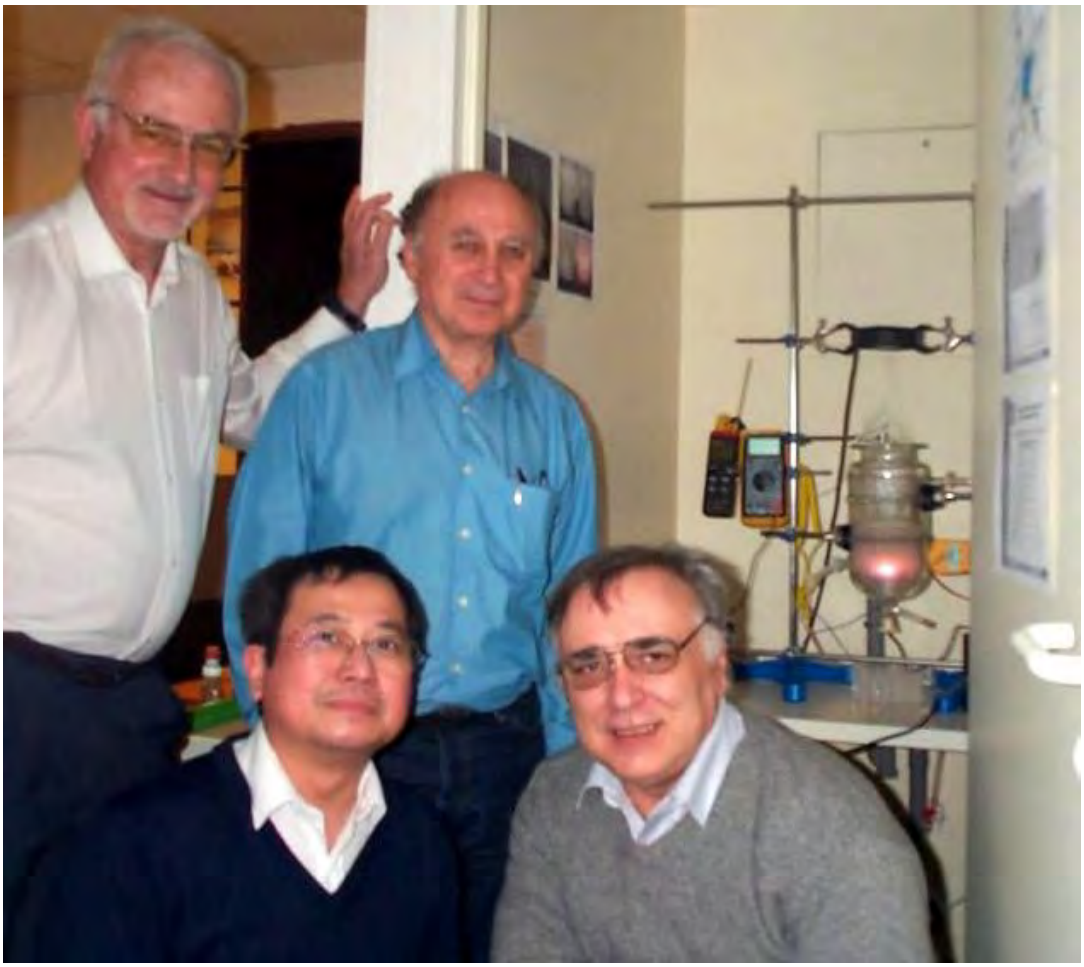
1) This morning Pierre sent me a picture which I see think is worth posting. Pictures of other researchers, with whom I had privilege of working on the ongoing CMNS project (Scott Little and Richard Slaughter) have already been posted in unit 270. The picture below shows Pierre Clauzon (upper left) and Gerard Lalleve (lower right). Also shown are Jean Paul Biberian (upper right) and Jean Louis Naudin (lower left). Jean Paul might start a new Mizuno-type experiment in Marseilles soon while Jean Louis is a veteran in the field of plasma cells, as indicated in the unit #289.



Do not miss a Mizuno-type cell on that photo. It is next to Gerard's head. The red-orange spot is the glowing plasma column at the tip of the cathode. The next photo shows Ed Storms (left side) and Fangil Gareev (right side). I took that picture December 2006, at our last conference in Japan (ICCF12). Gareev is from the Nuclear Research Laboratory in Dubna, Russia.

And how could I not show the photo of me with Russian friends? On the right side is Alexander Karabut, then the interpreter Natalia Famina, then myself, and then Irina Savvatimova. Discoveries of Karabut and Savvatimova, described in the unit #13, had profound effect on me. The picture below was taken December 2006, at our last conference in Japan.

A) *

So much about pictures. Now I would like to show the unexpected message that Ed Storm posted on the restricted discussion list for CMNS researchers.

" I want to add emphasis to what Jed said and reiterate what I have been saying in previous e-mails. Trying to do CF experiments on the cheap and with crude equipment is a complete waste of time. Like most people in the field, I started this way and have regretted it ever since. After many years of trial and error, and on the job training, many of us know what works and what does not. It is essential that this experience be used unless you want to spend years getting this experience yourself. The advice is to use standard, well understood methods; use equipment that is reliable; and design the experiment so that it is as simple as possible.

On the other hand, I see that it is hard for some people to take such advice. People seem to be designed to insist on doing an experiment their own way so that they can express their own creativity. Perhaps, making mistakes and learning the lessons are required for everyone. That is why actually running an experiment teaches more than any amount of advice. Only failure gives interest in making a change in the approach."

B) *
And here is my immediate reply to this message. "Ed, instead of arguing in general let me review our specific situation.

1) A claim was made (Paris-1) that a very simple method can be used to demonstrate excess heat. What should be the most natural way to check this? It is to perform the identical experiment and look for results.

2) We did this in Texas-1 and in Colorado-1. Results did not confirm excess heat. Our report at ICCF12 was criticized for not performing the experiment in the same way as in Paris-1. A researcher from the Paris-1 team, Pierre Clauzon, came to help us to replicate their experiment properly. Under his guidance we were able to observe excess heat. We confirmed that identical, more or less, results are obtained under identical conditions.

3) This was a big step forward. In any other field of science we would write a paper and this would probably be

viewed as a small step forward. Other researchers, or ourselves, would then start performing better experiments to explore various aspects of the natural phenomenon, including practical applications.

4) But, as you know very well, it is not easy to publish results of CMNS investigations in a refereed journal. Facing this situation, we do not have the luxury of being satisfied by publishing the results. Our work is unfinished. Starting another project, for example an experiment in a closed cell with a flow calorimeter, would mean that we simply wasted time. We want our results to be known, as they are, not as they might be in another experiment. If we are right then we want to convince others that we are right. If we are wrong then we want to know where we are wrong.

5) I think the methodology chosen for the Paris-1 experiment was appropriate to investigate the COP>1.2; it would not be appropriate for the COP<1.05. The claimed signal is said to be large, and systematic errors of 2% or 3% should not prevent a researcher from identifying it. After all, the energies to measure are in the range between 30 KJ and 100 KJ.

6) We performed about 50 evaluations of the COP and found the results to be reproducible. The mean value was 1.24 and the standard deviation was 0.13. Preliminary Paris-2 and Colorado-3 results (at least ten measurements, as I know from private messages) are consistent with these data.

7) Why should we abandon the methodology which produces credible results? Why should we not continue searching for possible systematic errors? Perhaps there are no large errors; perhaps the true value is somewhere between 1.1 and 1.3. We want to know this for sure.

8) You wrote: 'Trying to do CF experiments on the cheap and with crude equipment is a complete waste of time.' I do not think that we were wasting time. Our instruments are not too crude. You also wrote "The advice is to use standard, well understood methods; use equipment that is reliable; and design the experiment so that it is as simple as possible." That is what we were doing. We measured temperatures with a standard student laboratory thermometer, we measured the mass lost with a standard digital laboratory scale, we measured electric energy by several standard methods and verified that differences between these methods were very small.

9) I suspect that our weak points are in the interpretation, not in measurements. Fortunately, voices on this list helped us to identify two weak points: (a) absence of determination of the amount of liquid lost in the form of tiny droplets and (b) the possibility that the measured excess heat is due to well known chemical reactions.

10) It is always better when such issues are recognized before a paper is submitted. In our situation this is essential. We must address the weak points in the paper before the referees identify them. Unfortunately, we are not chemists. That is why help from chemists and electrochemists is expected. With help we might be able to succeed; without help and encouragement we might fail. Just imagine what might possibly happen after excess heat, in a Mizuno-type experiment, is recognized as real by mainstream scientists, by the DOE, the NSF, and the editors of leading journals. Yes, I know that our paper will not produce spectacular results. But it might be a successful first step toward recognition. Then more sophisticated approaches will be justified. Giving up at this stage would be a big mistake."

C) *
I waited for the reply from Ed but what came instead was a message posted by Jed Rothwel. I wander what Ed Storms thinks about Jed's message. It is shown below. Note that what is attributed to me is in black while what is from Jed is in blue. Rothwell wrote:

Ludwik Kowalski wrote:

1) A claim was made (Paris-1) that a very simple method can be used
to demonstrate excess heat. What should be the most natural way to
check this? It is to perform the identical experiment and look for results.

That is a good first step.

A researcher from the Paris-1 team, Pierre Clauzon, came to help us

to replicate their experiment properly. Under his guidance we were able to observe excess heat. We confirmed that identical, more or less, results are obtained under identical conditions.

Good on you, as they say in Sydney.

3) This was a big step forward.

Well, it is replication #5 or so. Not bad, and it calls for round of applause, but it has not pushed the state-of-the-art or added any knowledge yet.

4) But, as you know very well, it is not easy to publish results of CMNS investigations in a refereed journal.

"Not easy" is incorrect. It is utterly impossible, and if your goal is to publish in a refereed journal you are wasting your time. I think your goal should be to convince qualified people that your results are real, not artifactual.

Facing this situation, we do not have the luxury of being satisfied by publishing the results. Our work is unfinished. Starting another project, for example an experiment in a closed cell with a flow calorimeter, would mean that we simply wasted time.

No, it would mean that you learn the technique and you are ready to begin a more serious experiment.

We want our results to be known, as they are, not as they might be in another experiment.

Why? Ohmori's results are already known. You have not improved upon him or proved anything he did not prove long ago. This is kind of like building an exact replication of the original 1903 Wright Flyer, to prove that airplanes can fly. Why not build a better model? why repeat crude experiments that were performed years ago? Ohmori had no choice -- he had no money and he faced enormous opposition from the University -- but you can use a proper computer, a digital camera to record the appearance of the plasma, multiple thermocouples or a Seebeck calorimeter. Why should you limit yourself to instruments that were available in 1890? I cannot see any logic or any reasons for this.

5) I think the methodology chosen for the Paris-1 experiment was appropriate to investigate the COP larger than 1.2

Okay it is "appropriate to investigate." But if you want to convince large numbers of people, or if you want to learn much more about the reaction, you will use better instruments.

We performed about 50 evaluations of the COP and found the results to be reproducible.

Good. You are about the fifth group to confirm Ohmori, using a method similar to his, albeit with less skill and care than he used. If that is all you want to accomplish than you are finished and you can stop doing the experiments.

6) Why should we abandon the methodology which produces credible results?

Because we already have credible results! Why continue with a methodology that has been tapped out and has taught us all that it can teach? Why repeat old experiments that were already as convincing? There was never any doubt that Ohmori and Mizuno were reporting honestly, and that they did observe apparent excess heat. I saw it myself in their labs on many occasions. Your confirmation does little to buttress their credibility, and it adds nothing to our knowledge of this phenomenon.

Ed wrote that “trying to do CF experiments on the cheap and with crude equipment is a complete waste of time. ”I do not think that we were wasting time. Our instruments are not too crude.”

They are way too crude. Take it from Ed -- and Mizuno, and me for that matter.

Ed wrote “The advice is to use standard, well understood methods; use equipment that is reliable; and design the experiment so that it is as simple as possible.” That is what we were doing.

You are certainly not using well understood methods or reliable equipment. You roached your computer! How reliable can that be? (I mean you burned it up -- in 1960s jargon.) As for simplicity, perhaps Storms should have said, as Einstein did, "as simple as possible, but not too simple."

We measured temperatures with a standard student laboratory thermometer, we measured the mass lost with a standard digital laboratory scale, we measured electric energy by several standard methods and verified that differences between these methods were very small.

Yes, so did Ohmori and Mizuno, many years ago. Why do you think Mizuno stopped doing this long ago? Because the method has many inherent disadvantages and inaccuracies, as I have explained time after time.

D) *
Johnny Coviello wrote:
. . . No reason to continue raising the same objections over and over again, once they are acknowledged by the researchers. Certainly, the researchers reporting this recent excess heat result in Colorado were open to criticism of their experimental methods. Ludwik even said that due to the rather crude nature of calculating excess heat, the experiment results under COPs of 1.20 should be considered suspect. Of course, many of their results were above COP 1.20, so once again the chemical explanation or other error explanations, such as misting due to the violent nature of the reaction, might not be adequate.

Look at it this way. The Colorado experiments are another indication that something unusual is occurring in these cells. If they did not record unusual COPs, then there would be nothing to talk about. All they can do is take the criticisms into account and design another cell that tries to address the pontial error concerns, and see if unusual COPs are once again recorded. Process of elimination. It does appear to me that a reproducible cold fusion experiment might be evolving from the original Muzino work.

E) *
Jed Rothwell wrote (in black):
He was referring to a message from Michel Jullian (see quotations in blue).

Ed, instead of patronizing and stigmatizing . . .

Storms has been doing this for 16 years. Take his advice or leave it, but stop acting childish.

. . . please note that the simple and inexpensive (for wide replication) . . .

Define inexpensive. $50? $500? $2,500? What advantage would there be in saving $2000? I cannot think of any. How widely do you want this to be replicated, anyway? I do not think we should encourage large numbers of people at home to try an experiment that calls for boiling poisonous electrolyte, 3000°C metal, and which has been known to explode violently without warning for no apparent reason. Someone could be seriously hurt or killed doing this. You should think about liability before you go around promoting replications by inexperienced people outside of well-equipped laboratories.

but rigorous (for indisputability)

If you want something indisputable, you will have to use more sophisticated techniques and better instruments that Ohmori did. Frankly, his experiment was better than the one described here. He has skill and decades of experience. As long as you stick with inexpensive 19th-century techniques, you will not improve on what he did, and you will not prove anything he did not prove years ago. That is why Mizuno went on to use more sophisticated techniques.

. . . boiloff Mizuno experiment we are trying to design in this collaborative way has never been done before, so if it can be done at all it will require creativity obviously . .

You have that backward. You are being too creative. To do this experiment properly you need to be less creative and more willing to take advice from experts and do things according to the textbooks.

. . . i.e. lots of many silly complicated proposals which don't work before reaching the clever simple one which will work.

Cleverness is not called for. People do not believe clever results. We have already described techniques that will work. Your best choice would be a large Seebeck and a closed cell (perhaps a reflux cell).

F) *
L. Kowalski:
We can not address all issues . . . Our experiment produced some results and I want to know if the COP=1.24 (st. dev.=0.13) is not an illusion due to an error. I am certainly not going to discourage others from perform better experiments. And I will be extremely happy when they come up with results that are more trustworthy, for example, COP=1.75 (st. dev. 0.03). General problems are worth discussing . . . I hope that your comment on my reply to Ed will generate an interesting discussion. The issue -- why bother with what has already been done? -- is well framed in your message.

G) *
Michel Julian (responding to a message from ED that have not seen):
Ed is quoted in blue, Michel's comments are in blue.

Dear Ed, my comments in your text. (To Jed and JCJ, less verbal violence please)

Dear Michel, I do not mean to be insulting. My comment was a simple observation

No insult taken then. Sorry I misinterpreted your comments. [What follows is from from another message of Michel]:

Group, assuming we all agree on the objective of the simplest possible indisputable boiloff Mizuno experiment design, we could discuss honestly and peacefully the merits of all proposals, giving priority at any given time to the simplest that is currently proposed (which should take the least time to debunk), and working our way progressively up through the more sophisticated ones as and if we find flaws in the simpler ones. The metric for simpler could be that more people can replicate the experiment, based on the bill of materials.

We should try to refrain from any preference based on whether the solution is 19th century or 21st century, nor whether it was invented here or there, nor whether the proposer has been 17 years or 1 month in the field, nor whether he has scientific/technical education or not, nor whether he can shout louder than another. Merits only.

If we don't understand a proposal we should say so rather than ignoring it, and the proponent should explain. Critical sense would be welcome. Flames or patronizing wouldn't. . . .

H) *
Jed Rothwell:
Michel Jullian wrote: "The metric for simpler could be that more people can replicate the experiment, based on the bill of materials."

Why would the bill of materials be the limiting factor? I should think that an easier, safer, more certain method would increase the number of people who can replicate. Whether an experiment cost $1,000 or $5,000, and whether it takes 5 components or 20, seems immaterial.

[He also wrote:] “We should try to refrain from any preference based on whether the solution is 19th century or 21st century . . .”

Indeed, why stop at the 19th century? Let us consider using 18th century techniques to refine our own tungsten from wolframite. I assume the goal of this technological time travel is to make life difficult, create extra labor, and reduce precision, so that when we return to the 21st century with a sigh of relief, we will appreciate computers, thermocouples and the other 'tools of advanced civilization' (what the Japanese call 'bunmei no riki'). It is sort like camping, in other words. I myself spent enough time in the pre-computer world of typewriters, slide-rules, film-cameras and mercury thermometers. Nostalgia aside, I have no desire to reenact it.

[He also wrote:] “ nor whether it was invented here or there, nor whether the proposer has been 17 years or 1 month in the field, nor whether he has scientific/technical education or not . . .” Another interesting suggestion! Ah, but consider -- (a) "Those who cannot learn from history are doomed to repeat it." - George Santayana [and] "Experience is a dear teacher, but a fool will learn at no other." - Benjamin Franklin

G) *

Ed Storms wrote:

Of course a machine shop is required along with a glass blower and a computer programer. Without either the money to hire such people or the personal ability to do the job, you should not be in the business of trying to prove that an unbelievable effect is real. In the good old days when I learned science, everything had to be designed and constructed by the experimenter. Now, a great deal can be bought, but this takes money. Without money, you can still make the required items, but this takes skill. As a result, most people cut corners and jerry rig a Rube Goldberg device that does nothing but entertain the experimenter. This is worthwhile, but it is not science and will not be believed by the world of science no matter how well the arguments are constructed or debated.

I) *

L.Kowalski wrote (but not posted):

Yes, in comparison with Ed Storms and Michael McKubre we are amateurs. By "we" I mean people involved in Colorado-2, Paris-2, and Colorado-3 experimens. Scott Little, in my oinion, belongs to a much higher category of researchers. This jugemen is based on his exceptional technical skills, and on experience he gained on working with calorimeters. He cooperated with Mizuno but was not able to replicate excess heat. I am dissappointed that the Texas-2 experiment must be delayed because they are investigating something else. I would feel much more comforteble about the Colorado-2 results if Scott also obesrved the COPs significantel larger that unity.

For the time being I rely on the data collected in Colorado-2, plus private confirmations from about ten more recent tests (preliminary data from Paris-2 and Colorado-3 experiments). I already responded to the hint Ed made about our “complete wasting of time.” I am not going to repeat this again on the CMNS discussion list. Nothing could be gained from this. We were not “cutting corners” in the Colorado-2 experiment. We did what science teachers and students could do in a school lab (after reading units #252 and #253) . Now we are discussing ways to determine how much liquid is lost in the form of tiny droplets. One of these methods, or more, will be used in subsequent experiments. What is wrong with this? Why should we stop now? We found potential sources of errors and we want to be sure that excess heat is not an illusion. We need help and encouragement. I think that our simple device did a little more than “nothing.” What I am affraid of is that people planning for the Paris-2, Colorado-3, Texas-2 and Marseilles-1 experiment are going to give up because the most senior CMNS researchers are not providing as much help and uncouragement that I expected. I hope this will not happen.

O) *

Jed Rothwell wrote:

You do NOT -- repeat NOT -- want anyone to do this as a science fair project! This experiment should not be done by high school kids or by people who do not have safety equipment at hand, including goggles, a shower and so on. Let me reiterate what I said earlier, with emphasis: This experiment is DANGEROUS. The electrolyte is toxic and it is boiling. The light from the cathode is so intense it will damage your eyes if you look at it too closely for too long. On at least one occasion, a glow discharge cell has exploded violently without warning, releasing over 400 times more

energy than was input into the cell. This is far beyond the limits of chemistry -- there is no doubt the energy release was anomalous. It drove a glass shard about 1 cm into Mizuno's neck, next to the carotid artery, and the noise deafened him and his colleague for several hours.

I spent several days watching both Ohmori and Mizuno performed their versions of this experiment. Both of them are careful, and both have decades of experience doing electrochemistry, but Ohmori's technique was so frightening to Mizuno that Mizuno refused enter the lab. "I will watch from out here, " he said, standing out in the hall. "The way Ohmori does this scares the hell out of me." This was five years before his own cell exploded. . . .

K) *

Ludwik Kowalski wrote:

(1) An explosion due to accumulation of hydrogen is much less likely to occur in an open Mizuno-type cell. (2) Nearly all chemistry experiments performed by students must be supervised by teachers. If I were to supervise an open-cell-Mizuno-type student project I would used the open cell. And I would insist of using a standard laboratory hood, plus protection glasses, etc. . Yes, safety aspects are very important. (3) Do you remember a report from two high school girls who confirmed the COPs >1 in Louisiana two years ago? I do not recall any safety concerns being raised (at our conferences) about open cell experiments by high school students. Why now? I would like to see several student reports about over-unity COPs each year. The fact that these reports do not prove anything new would not bother me. At this stage we need confirmations, the more the better. And think about educational and motivational effects of such projects on students.

L) *

Jed Rothwell:

This explosion [in Mizuno cell] was anomalous. It could not possibly have been caused by hydrogen, or any chemical reaction. I presume it was a runaway cold fusion reaction, like the one that melted or vaporized Fleischmann and Pons' cathode in Feb. 1985. We should not kid ourselves about this issue anymore. I have now seen five credible reports of anomalous explosions, four them clearly beyond the limits of chemistry. I think it has been established that cold fusion can produce runaway reactions and explosions. It is my gut feeling that the glow discharge version is particularly prone to this, because it sometimes produces large energy spikes very rapidly. I only hope cold fusion cannot be used to make a large-scale nuclear bomb, but no one can be sure of that at this stage.

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291) Yes, these experiments are dangerous, but . . .

Ludwik Kowalski; 4/9/2006
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Several days ago Jed Rothwell, the author of the book "Cold Fusion and the Future," posted the following message on the restricted list of CMNS researchers. Note that CMNS stands for Condensed Matter Nuclear Science -- a new name for what used to be called Cold Fusion (CF). The CF is a subfield of CMNS in which unexpected nuclear particles are studied. Jed wrote:

I recently updated my book, and I included a list of 4 anomalous cold fusion explosions in chapter 12. I also uploaded some photos of accidents here:

http://lenr-canr.org/Experiments.htm#PhotosAccidents

I just realized there was another anomalous explosion reported at ICCF-3:

Zhang, X., et al. *On the Explosion in a Deuterium/Palladium Electrolytic System*. in *Third International Conference on Cold Fusion, "Frontiers of Cold Fusion"*. 1992. Nagoya Japan: Universal Academy Press, Inc., Tokyo, Japan.

This event occurred in April 1991. I just asked Zhang if he still thinks it was anomalous and if I should include in the list. Glow discharge appears to be the most dangerous and unstable version of cold fusion, but the other types are also hazardous.

This was followed by several messages (shown in the appendix below") in which Jed argued that plasma electrolysis experiments should not be conducted by students and amateurs. This goes against what I wrote in the unit #252. And I still disagree with Rothwell that Mizuno type experiments in an open cell, as described in the unit #286, are much more dangerous than experiments students perform in high schools and universities. Safety issues are important one protection rules, such as using a ventilated hood, protective glasses, etc., must be respected. Furthermore, student experiments must be supervised by science teachers. In replying to one of Jed's messages I wrote:

An explosion due to accumulation of hydrogen is much less likely to occur in an open Mizuno-type cell. Nearly all chemistry experiments performed by students must be supervised by science teachers. If I were to supervise a Mizuno-type student project I would used the open cell. And I would insist of using a standard laboratory hood, plus protection glasses, etc. Yes, safety aspects are very important.

Do you remember a report from two high school girls who confirmed the COPs >1 in Louisiana two years ago? I do not recall any safety concerns being raised (at our conferences) about open cell experiments by high school students. Why now? I would like to see several student reports about over-unity COPs each year. The fact that these reports do not prove anything new would not bother me. At this stage we need confirmations, the more the better. And think about educational and motivational effects of such projects on students.

The topic of risks in research is very broad and interesting. It has recently been discussed in the context of some ongoing medical and agricultural projects. Frederic Joliot Curie addressed that topic in his (1934 ?) Nobel Prize

address. He speculated about a possibility that a nuclear chain reaction can develop in our environment and destroy it at once. That is what Jed is speculating about. I think he is exaggerating. But his comments, like his book, are worth reading.

APPENDIX: Rothwell's messages:

Message #1

You do NOT -- repeat NOT -- want anyone to do this as a science fair project! This experiment should not be done by high school kids or by people who do not have safety equipment at hand, including goggles, a shower and so on. Let me reiterate what I said earlier, with emphasis: This experiment is DANGEROUS. The electrolyte is toxic and it is boiling. The light from the cathode is so intense it will damage your eyes if you look at it too closely for too long.

On at least one occasion, a glow discharge cell has exploded violently without warning, releasing over 400 times more energy than was input into the cell. This is far beyond the limits of chemistry -- there is no doubt the energy release was anomalous. It drove a glass shard about 1 cm into Mizuno's neck, next to the carotid artery, and the noise deafened him and his colleague for several hours. I spent several days watching both Ohmori and Mizuno performed their versions of this experiment. Both of them are careful, and both have decades of experience doing electrochemistry, but Ohmori's technique was so frightening to Mizuno that Mizuno refused enter the lab. "I will watch from out here," he said, standing out in the hall. "The way Ohmori does this scares the hell out of me." This was five years before his own cell exploded.

Let me put this very bluntly. If you go around encouraging unqualified, unprepared, inexperienced people to do this experiment, and one of them is hurt or killed, you will regret it for the rest of your life. You might end up in civil court owing huge sums of money. This is like encouraging people to repair gasoline motors and gas tanks in their garage. I have know two people who burned their house to the ground doing that. One of them also incinerated his 6-year-old son. For you to even suggest this should be done as a "science fair project" is utterly irresponsible and stupid. Furthermore, it would serve no useful purpose. Hundreds of inexperienced people doing this as a science-fair-level project would not convince any scientist, and it would not teach us anything new about the reaction.

Message #2

Ludwik wrote: "An explosion due to accumulation of hydrogen is much less likely to occur in an open Mizuno-type cell." This explosion was anomalous. It could not possibly have been caused by hydrogen, or any chemical reaction. I presume it was a runaway cold fusion reaction, like the one that melted or vaporized

Fleischmann and Pons' cathode in Feb. 1985. We should not kid ourselves about this issue anymore. I have now seen five credible reports of anomalous explosions, four them clearly beyond the limits of chemistry. I think it has been established that cold fusion can produce runaway reactions and explosions. It is my gut feeling that the glow discharge version is particularly prone to this, because it sometimes produces large energy spikes very rapidly. I only hope cold fusion cannot be used to make a large-scale nuclear bomb, but no one can be sure of that at this stage.

Ludwik asked: "Do you remember a report from two high school girls who confirmed the COPs >1 in Louisiana two years ago?" Was that with glow discharge, or ordinary electrochemistry? Glow discharge is much more dangerous at a practical, hands-on level. The electrolyte boils, the cell shakes and rattles, the light is blinding, power levels and temperatures are much higher than other types of cold fusion, and the cells sometimes produce large volumes of free H2 and O2 gas, from pyrolysis.

Message #3

Ludwik Kowalski wrote: "Would you support a moratorium on experiments with Mizuno-type cells anywhere, including national labs, old deep mines etc.?" No, that would be ridiculous. Let me put it this way. Internal combustion gasoline engine technology is 120 years old. It has been developed in national labs and corporations, and people work on gasoline engines in millions of shops and garages across the land. But, even after 120 years these things are still dangerous, and they always will be. As I said, I knew two people who destroyed their houses -- one who killed his son -- because they were monkeying with gasoline engines in their garage, and they did not have the proper tools, training, ventilation and so on.

There are many other dangerous jobs that untrained amateurs should not engage in, such as working with AC electricity house wiring or using heavy duty chain saws. Nobody suggests we should ban the use of AC electricity, even though every baby and toddler in the country is a few meters away from sticking a finger into a socket, and babies do that every day. We must take risks, but we should also take precautions. (You should cover up those sockets with plastic gadgets until the kid turns 3! And don't do it when the kid is watching.)

As I have said and said again, people who do the Mizuno experiment should use proper tools and safety procedures. They should know about these procedures because they should be trained chemists -- or at least they should consult with chemists. They should use a television camera to observe the glow discharge, rather than squinting at it from 50 cm away. They should use other common sense methods of avoiding danger. They should be willing to spend a few thousand dollars extra to ensure safety, if that is what it costs.

Of course if the experiment produces an explosion on the scale of a small nuclear bomb nothing can be done. I would not rule out that possibility completely, but I personally would be willing to observe or participate in these experiments despite that remote possibility. Present-day fossil fuel based systems cause enormous damage, and they kill vast numbers of people. Coal alone kills roughly 20,000 people per year in the US. They are probably causing worldwide global warming which may kill millions of people, wipe out hundreds of thousands of species, and destroy thousands of cities and towns. So although there is a remote possibility that cold fusion might be capable of causing nuclear bomb scale explosions, it is worth taking that risk because the energy systems we have now are causing catastrophic damage.

Message 4:
[X asked for details.], they are here: <http://www.lenr-canr.org/acrobat/MizunoTanomalouse.pdf> [X also asked: "Would this not provide sceptic-zapping proof that LNER are real?" It proves LENR is real, but it will not zap any skeptics because Mizuno cannot make it happen on demand, and no skeptic will believe that it happened in the first place. Mostly it has zapped Mizuno and his colleague. He is no longer doing these experiments because he is frightened, which is understandable. There have been at least 4 anomalous explosions, and probably 3 others count (one at BARC and two reported by W.-S. Zhang). See: <http://www.lenr-canr.org/Experiments.htm#PhotosAccidents>

That book, by the way, is available (free) over the Internet. Jed wrote: "If you would like to print a copy, we suggest you download the high-resolution version:

http://lenr-canr.org/ColdFusionAndTheFuturehires.pdf (15 MB)

Please feel free to print as many copies as you like. Note that it is cheaper to print in black and white. The first two chapters of the book have been translated into Italian and are available on line here: http://www.progettomeg.it/fflibrofreeit.htm "

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292) Are we fighting an uphill battle ?

Ludwik Kowalski; 4/12/2006
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The messages below were posted recently on the restricted Internet list for the CMNS researchers. They show what these people think and feel about being discriminated by the establishment of mainstream science.

Message 1;
Jean Paul Biberian, a French electrochemist from Marseilles, wrote:

When I perform an experiment, I have three goals in mind:

1- Most important: convince myself, that the measurements are valid, and make sure that there is no artifact. People who have done calorimetry can tell how easy it is to make mistakes, I have done many myself. Calorimetry needs a lot of practice, and there is nothing more difficult than a simple experiment.

2- Then and only then I can think of a demonstration to convince others, i.e. colleagues of mine interested enough to have a look at the experiment.

3- Try to publish the results. Here things become very difficult, because production of heat alone is not sufficient. The referees will want to have an explanation of where the heat comes from. Even though the total amount of heat might be beyond any chemically possible reaction, doubts may persist. Think of the heat pump which produces several times more heat than the electrical power input. You can never prove that somewhere somehow there is not a hidden energy source. It becomes necessary to come up with an explanation, i.e. for instance the production of energetic particles or helium, tritium, transmutation. ...The point is that no referee will want to spend the time to come to the lab and stay a few days with me to check everything in details. Even if he happens to do it, the editor or another referee will kill the OK of the first referee, because the credibility of the journal is at stakes.

Why are we in such a situation? :
We want to sell Cold Fusion as a scientific reality, and get acceptance from mainstream science. After 17 years, twelve international conferences, thousands of papers and experiments, we are not yet accepted. Why? I think because nobody wants Cold Fusion :

1- Scientists don't like paradigm changes, most are very conservative and do not like to have to change their habits. Lot has already been said about that, no need to develop here.

2- Energy suppliers: oil-gas-coal-nuclear don't need a new competitor, like with the mafia each one has a part of the territory, and they will join to expel a newcomer, even though they fight all the time between themselves. A new energy source will endanger all of them.

3- Environmentalists do not want Cold Fusion, for them this is the worst thing that could happen, if Cold Fusion becomes a major source of unlimited energy, then mankind will destroy the planet.

4- The military don't want us to look at that. There might be military applications, and this might give everyone access

to powerful weapons. Nobody understand some of the explosions that occurred during electrolysis.

We are fighting an uphill battle, we are in a situation where everybody and everything is against us. What is our best hope:

1- An experiment that produces lot of heat, with a large COP, for a long period of time. The calorimetry must be very simple, with no calibration. My opinion is that mass flow calorimetry is the best. Input power should be simple, i.e. DC current so that no correction is needed, The output power is easy to calculate if you can measure the input and output temperatures with a mercury thermometer, and the water flow with a stopwatch.

2- An experiment that produces anomalous isotopes, the Iwamura work is in that direction, but there is not enough material produced to convince skeptics.

3- The best experiment: a stand alone system,with no electric wire attached producing heat or electricity. However, if anyone can do that he doesn't care about publishing, he can probably sell the device!

Conclusion:
I do not have a solution yet, but I think that a convincing Mizuno type experiment should be done with mass flow calorimetry, I am thinking about doing one in Marseilles. Duplicating what has been done several times before will not help.

Massage #2
Ludwik Kowalski
I do not think that an explanation is essential at the very early stage. An experimental claim like "we demonstrated an excess energy that can not be due to a mistake or to an artifact" should be publishable. Pierre Curie had no theory explaining why his radium source was generating heat. That was in 1903. Good luck with Marseilles experiments.

Message 3
Jean Paul Biberian
Pierre Curie, he had found anomalies in the uranium salts, but nobody wanted to believe him. When he and his wife managed to isolate radium, and that at this point it was obvious to everyone that something unusual was happening then and only then radioactivity was accepted. A little bit like having a stand alone system producing heat without any external source...

Message 4:
From Jed Rothwell
Jean-Paul Biberian wrote: “Calorimetry needs a lot of practice, and there is nothing more difficult than a simple experiment. ” Exactly right. Try replicating J. P. Joule's experiments some time and you will see how true this is. [He also wrote] “Then and only then I can think of a demonstration to convince others, i.e. colleagues of mine interested enough to have a look at the experiment. ” Good start!. He also wrote] ”Try to publish the results. Here things become very difficult, because production of heat alone is not sufficient. The referees will want to have an explanation of where the heat comes from. .....” When editors and referees say things like that, you are wasting your time. Forget about trying to convince such people.

We agree that the best way to begin to convince people is with colleagues who are interested in the experiment. After you shall the experiment to your colleagues, who is next? What group of people do you show it to after that? People who attend the ICCF conferences, naturally, but they only meet once a year. However, 5000 people per week gather at LENR-CANR.org and they download 4000 papers. These are mostly researchers and they appear to be serious, open-minded, and willing to read difficult papers. So obviously you should publish at LENR-CANR.org, and also at ISCMNS, and your own web page, and at as many other web pages as you can find. You write papers in order to inform other researchers of your work, and convince them that your results are important. You can accomplish those goals by publishing online.

[Jean Paul also wrote] “Even though the total amount of heat might be beyond any chemically possible reaction,

doubts may persist. Think of the heat pump which produces several times more heat than the electrical power input. You can never prove that somewhere somehow there is not a hidden energy source." That is incorrect. Only a few heat pump types exist and they are readily identifiable. Furthermore, if the excess power is greater than a few watts it is easy to identify where in the heat pump cycle it is coming from. Look for the component that is covered with condensation and cold to the touch. If the Mizuno cell is a heat pump, some part of it would be covered with ice. It would also be one of the most efficient refrigerators ever invented, and a world-class discovery. [He also wrote] "Energy suppliers: oil-gas-coal-nuclear don't need a new competitor . . . " I doubt they are aware of the situation.

[He also wrote] "I do not have a solution yet, but I think that a convincing Mizuno type experiment should be done with mass flow calorimetry, I am thinking about doing one in Marseilles." It will be much better to use Seebeck calorimetry.
[He also wrote] "Duplicating what has been done several times before will not help." Exactly right. Ohmori and Mizuno would agree, I think.

Message 5:
Jed Rothwell wrote:
[X wrote, sarcastically] "Nature does not employ an editorial board of senior scientists, nor is it affiliated to a scientific society or institution, thus its decisions are independent, unbiased by scientific or national prejudices of particular individuals."

<http://www.nature.com/nature/authors/get_published/index.html> "]

That's pretty funny. It reminds me of Sci. Am. editor John Rennie's statements to me, "we aren't scientists, so don't blame us for our opinions. " Sort of the opposite tack after you get past the denial part, but equally loony. Quote:

"The second misconception concerns Scientific American's function. We're journalists here at the magazine, even those of us with scientific credentials. We don't claim to be authorities on physics or any other discipline (for all that there is quite a lot of real expertise built into our staff). For that reason, the scientific points of view we choose to publish are ones that have already been vetted in the technical, peer-reviewed literature and that generally seem to represent a consensus within the scientific community. To do otherwise would mean that we were pretending to be more expert than the actual scientific authorities. That is not what Scientific American's readers want or expect.

(As for whether we're entitled to mock cold fusion...well, sorry if you disagree, but that opinion reflects the consensus of most scientists, too.)

So it really doesn't make a difference to me if LENR-CANR advocates petition me for articles on the subject; I'll put them on the stack of similar requests from the scientific creationists, the global warming deniers the face-on-Mars people, the crypto-archaeologists, and all the others who want publicity and scientific respectability but can't make their case convincingly to the community of scientists. But I'll say this again, too: if LENR-CANR's physicists can convince the mainstream physics community that they've got a credible case and articles to that effect start appearing in major peer-reviewed journals, Scientific American would be glad to write about it." http://lenr-canr.org/AppealandSciAm.pdf

These are strange people. You wonder how they ended up in charge of major scientific journals. (The previous editor, Piel, inherited the job.) ... .

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293) Jed Rothwell comments on some accusations

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Wikipedia is an online encyclopedia where anyone can delete, add or replace anything. The following message, from Jed Rothwell, just appeared on the restricted discussion list for CMNS researchers:

The cold fusion article at Wikipedia has grown too large, so it must be split up. Someone asked me to assist with the sub-article "cold fusion controversy." I should not waste my time on this sort of thing, but I did. The skeptics will soon trash this and erase it, but I had a lot of fun writing it. Have a look before it is gone:

en.wikipedia.org/wiki/Cold_fusion_controversy (The article was deleted in 2006)

I have a copy on my desk, preserved for posterity. I did not set out to make this humorous, although I can never resist. I doubt the skeptics will see it as funny. But I trust the readers here will see the humor in parts such as my deadpan rebuttal of the claim that cold fusion researchers are insane; my description of Hoffman's masterpiece; and the juicy quote from Happer and its source (Taubes).

- Jed

I think that what Jed wrote is worth preserving. Knowing that others will modify his text I pasted it below. It will be interesting to compare the current version with future versions. Be aware that what appears as a link in Wikipedia is nothing but a blue word below.

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Cold Fusion Controversy

Originally published in Wikipedia, April 2006, updated 4/20/2007

Ever since the Fleischmann-Pons experiment in March of 1989, the existence of chemically catalyzed cold fusion has remained a controversial issue. This article discusses the major skeptical arguments in the controversy. Experimental evidence for cold fusion can be found in the main article, cold fusion.

==Cold fusion as a violation of theory==

Most leading skeptics dismissed cold fusion because it appeared to violate the laws that govern high energy plasma physics. In a plasma reaction, copious neutrons "commensurate" with helium are produced, whereas with cold fusion the number of neutrons per helium atom is roughly 11 million times smaller. Skeptics say this means cold fusion is impossible. As Prof. H. Feshbach (MIT) put it in 1991: "I have had 50 years of experience in nuclear physics and I know what's possible and what's not........I don't want to see any more evidence! I think it's a bunch of *junk* and I don't want to have anything further to do with it." [1]

The books by F. Close [2] and J. Huizenga [3] are mainly devoted to proving that cold fusion violates theory and is therefore impossible. Huizenga, who was the head of the DoE ERAB panel that dismissed cold fusion in 1989, concluded his book with a 6-point summation. Point number six states that we know *a priori* that all positive cold

fusion excess heat results must be wrong:

> "Furthermore, if the claimed excess heat exceeds that possible by other conventional processes (chemical, mechanical, etc.), one must conclude that an error has been made in measuring the excess heat."

Cold Fusion researchers feel that they subscribe to the traditional view, that experiments are the standard by which all claims must be judged. [4] [25] They believe this is fundamental to the scientific method. When a phenomenon has been replicated many times at a high signal to noise ratio, that proves it does exist, and if theory predicts it cannot exist, the theory must be wrong. Cold fusion theorists believe that cold fusion does not violate conventional theory. The number of neutrons produced by cold fusion is much smaller than plasma fusion because a metal lattice at room temperature is very different from the center of the sun. As cold fusion theorist Julian Schwinger put it, "The defense [of cold fusion] is simply stated: The circumstances of cold fusion are not those of hot fusion". [5]

==Explicit rejection of the experimental method==

Several leading skeptics have stated that cold fusion proves the experimental method itself does not work. In other words, when dozens or hundreds of laboratories report they have replicated a phenomenon, they might all be wrong, and the only way to be sure a finding is correct is to show that it conforms to established theory. [Close, Huizenga ibid.] [6]

A variation of this idea was expressed by R. Ballinger (MIT) and B. Kevles (Yale). They say that Fleischmann and Pons were definitely wrong, but those who later replicated them may be right. Ballinger wrote:

> "It would not matter to me if a thousand other investigations were to subsequently perform experiments that see excess heat. These results may all be correct, but it would be an insult to these investigators to connect them with Pons and Fleischmann. .....Putting the 'Cold Fusion' issue on the same page with Wien, Rayleigh-Jeans, Davison Germer, Einstein, and Planck is analogous to comparing a Dick Tracy comic book story with the Bible." [7]

Kevles [8] accused Fleischmann and Pons of misconduct (fraud) and "scientific misdeeds,", and she said that the later replications of their work prove nothing to the contrary: [9]

> "Eventually, [Fleischmann and Pons] particular claims were refuted as theoretically unfounded and without experimental support. This is the incident I referred to in my article and it has altogether nothing to do with research since in this field." [10]

In all of the "research since" researchers used similar materials and reported similar results, and they cited the original paper by Fleischmann and Pons, so it is difficult to judge what Kevles has in mind.

Cold fusion researchers also assert that incomplete understanding of the process does not invalidate it, often citing the 1898 report by Marie and Pierre Curie that radium was permanently warmer than its surroundings. This report was accepted even though the source of the warmth was not known to science [Beaudette, ibid. p. 3]. Indeed, Nobel laureate Schwinger pointed out that discoveries of processes such as high temperature superconducting are "a prime example of embracing the concept without having to understand the mechanism." [ibid] The comparison is debatable, however. J. Piel, late editor of Scientific American, likened this "incomplete knowledge" claim to Langmuir's criteria for pathological science. [11]

==Cold fusion as pathological science==

Many have dismissed cold fusion as an example of pathological science: in such a science, a scientist, originally conforming to the scientific method, unconsciously veers from that method, and begins a pathological process of wishful data interpretation.

While there are no rigorous criteria for defining a pathological science, the following characteristics were listed by

Irving Langmuir when he invented the term:

* The maximum effect that is observed is produced by a causative agent of barely detectable intensity, and the magnitude of the effect is substantially independent of the intensity of the cause.
* The effect is of a magnitude that remains close to the limit of detectability, or many measurements are necessary because of the very low statistical significance of the results.
* There are claims of great accuracy.
* Fantastic theories contrary to experience are suggested.
* Criticisms are met by ad hoc excuses.
* The ratio of supporters to critics rises and then falls gradually to oblivion.

===The causative agent is almost undetectable===

The causative agent of excess heat in cold fusion experiment, if any, is not certain, but most cold fusion researchers think it is nuclear fusion. Nuclear reactions can only be detected by their effects. Some nuclear reactions have effects that can be easily measured (such as the production of tritium), while others are more difficult to detect (such as the production of helium-4).

Showing a correlation between excess heat and the amount of helium produced is challenging, but cold fusion researchers say it has been done at China Lake, the Italian national laboratories and elsewhere. [12] [13]

Other nuclear evidence, such as tritium, neutrons and gamma rays are far easier to measure. Cold fusion researchers say that these products have been confirmed in thousands of experiments in dozens of different laboratories, at levels far above the limits of detection, in some cases thousands of times above those limits. [14] For example, at the Bhabha Atomic Research Centre (BARC) the reactor safety experts have been measuring tritium for decades with accuracy and confidence. When several experiments at BARC experiment produced tritium, at levels up to 20,000 times above the initial concentration, they had no difficulty whatever detecting or confirming this, and they used several different methods. [15] [16] [17] [18]

===The effect is almost undetectable===

The excess heat has been measured by several different types of calorimeters often at a level that is easily detected, and sometimes impossible to miss. Tritium has been measured at levels ranging from 100 to several million times background. Gamma rays have also been measured far above background. Helium from cold fusion reactions is often close to the limits of detection but in at least one case it exceeded atmospheric concentration. Transmutations have in some cases converted milligram levels of materials, making them easy to detect. [19] [20] [21]

===The causative agent is not commensurate with the effect===

The causative agent of excess heat in cold fusion experiment, if any, is not certain, but every indication is that if they exist it is nuclear fusion. Nuclear reactions can only be detected by their effects. Some nuclear reactions have effects that can be easily measured (eg. neutron emission), while others are more difficult to detect (eg. Helium-4).

Showing a correlation between excess heat and the amount of helium is challenging, but as noted above, researchers at China Lake, the Italian National Nuclear Laboratories, Los Alamos, SRI and other laboratories report success. Tritium and neutrons, which are much easier to detect, have been measured at levels far above the limits of detection in hundreds of laboratories.

===There are claims of great accuracy in the measurement===

Cold fusion researchers do not claim great accuracy. They often use standard, off the shelf instruments within the manufacturer's certified levels of accuracy. Great accuracy is often not called for in any case, since, as noted above, the signal is often quite strong (far above background).

===Proposed explanations===

Critics of cold fusion say there are currently no satisfactory theories of cold fusion. Many cold fusion researchers agree with them. They consider cold fusion an experimental observation that is not derived from or supported by theory at this stage.

Many theories have been proposed to explain cold fusion, and some of these would generally be considered fantastic or fringe theories. However, other theories have been proposed by mainstream physicists such as Julian Schwinger and Peter Hagelstein. Schwinger and Hagelstein believe their theories are valid, and that that they do not conflict with the canon of established physics.

===Initial interest in the topic does not last===

Skeptics say that interest in cold fusion faded rapidly, and that the "vast majority" of scientists do not believe it exists. They do not cite public opinion polls or other hard data to back up this assertion. A poll published in Japan in 1994 indicated that most scientists there believe cold fusion is real, and nearly all support continued research. [22]

The majority of newspapers and news magazine reports on cold fusion published in the U.S. are negative. They usually say that the research was "debunked" and that it was fradulent and/or incompetent. (See below.)

Skeptics [Piel, ibid.] also say that no peer-reviewed journal papers on cold fusion have been published, but this is incorrect. Approximately 1000 have been published in mainstream peer-reviewed journals, [http://lenr-canr.org/DetailOnly.htm] and they continue to be published despite the hostility and opposition to the research expressed by APS officials, the *Washington Post*, some MIT professors and others. (See below)

==Doubts on the quality of the cold fusion scientists==

Cold fusion scientists have been often criticized for lacking credentials. However, many of them were previously considered to be world class experts in their fields. Distinguished Prof. John O'M Bockris, for example, wrote an authoritative and widely used textbook, *Modern Electochemistry*. He is a Fellow of the International Society of Electrochemistry [http://www.ise-online.org/geninfo/fellows_details.php]

Heinz Gerischer was considered a leading electrochemist. He was the Director of the Max Planck Institute for Physical Chemistry in Berlin, and a prize was established in his memory. He concluded that "there is now undoubtedly overwhelming indications that nuclear processes take place in the metal alloys." [24] Nobel laureate Julian Schwinger was considered a leading theoretical physicist and was respected by most scientists, but he reported being denigrated and attacked after he began writing theoretical papers about cold fusion. [ibid.] Dr. P. K. Iyengar conducted and directed cold fusion research while he was director of BARC. [http://lenr-canr.org/acrobat/IyengarPKprefaceand.pdf] He later became the chairman of the Indian Atomic Energy Commission

Profs. Miles, Oriani and Huggins have published textbooks and hundreds of articles, are designated Distinguished Professors and Fellows by universities and the U.S. Navy, and have been honored by the Electrochemical Society, NATO and other prestigious organizations. Other cold fusion researchers include three editors of major plasma fusion and physics journals, a retired member of the French Atomic Energy Commission, and many top researchers from U.S. national laboratories.

===Martin Fleischmann===

Martin Fleischmann is widely considered one of the top electrochemistry|electrochemists in the world. He is a Fellow of the Royal Society and past president of the International Society of Electrochemistry, and he was awarded a medal by the Society. He has published numerous papers in leading journals. On the other hand, he has no credentials in nuclear physics, and his first paper on cold fusion was shown to contain errors in this subject, in neutron detection. However, subsequent research was performed by leading experts in neutrons at Los Alamos, BARC and many other institutions, and these studies confirmed that cold fusion experiments produce neutrons. [Srinivasan, ibid.]

==Claims that experiments have been debunked or that they are fraudulent==

Most newspaper and magazine articles attacking cold fusion say it was debunked, or that it is fraud.

Debunked. The term "debunked" is not defined exactly, but it is usually taken to mean that replications were attempted but they failed. Cold fusion researchers believe these replications failed because the researchers performing them were not skilled in the art and because the experiment is inherently difficult to perform. The most famous three negative experiments performed in 1989 were at MIT, Caltech and Harwell. All three were subsequently shown to be positive. [25] [26] [27] [28]

Some articles have said that cold fusion was debunked because it was shown to be theoretically impossible. As noted above, cold fusion researchers believe this is a violation of the scientific method.

Many skeptics feel that cold fusion should be attacked, ridiculed and actively suppressed. In March 1990, D. Lindley, editor at Nature, wrote: "All cold fusion theories can be demolished one way or another, but it takes some effort.... Would a measure of unrestrained mockery, even a little unqualified vituperation have speeded cold fusion's demise?" [28] Cold fusion researchers feel this is a violation of academic freedom. Schwinger felt that such intemperate views will lead to the "death of science" [Schwinger, ibid.]

Fraud. Dozens of articles have said this. Writers in the *Washington Post* have been notably vehement. In 2006 Prof. B. Kevles (Yale) described cold fusion a "scientific misdeed." [Kevles, ibid.] In 1991 Robert Park, [30] wrote the following in a *Washington Post* review of the book by Frank Close:

> "..... Close asks in the first chapter, 'Was this a delusion, an error, or a fraud?' By the end of the book, it is clear that cold fusion progressed through all three. What began as wishful interpretations of sloppy and incomplete experiments ended with altered data, suppression of contradictory evidence and deliberate obfuscations.
>
> Fleischmann and Pons were no longer alone. Inept scientists whose reputations would be tarnished, greedy administrators who had involved their institutions, gullible politicians who had squandered the taxpayers' dollars, lazy journalists who had accepted every press release at face value -- all now had an interest in making it appear that the issue had not been settled. Their easy corruption was one of the most chilling aspects of this sad comedy.
>
> To be sure, there are true believers among the cold-fusion acolytes, just as there are sincere scientists who believe in psychokinesis, flying saucers, creationism and the Chicago Cubs. The lesson from *Too Hot to Handle* is that a PhD in science is not an inoculation against foolishness -- or mendacity."

The on-line database at LENR-CANR.org lists 3,400 papers on cold fusion written by 4,688 authors and co-authors, as of April 2006. Most of these papers have been positive, and none of the authors has benefited financially or otherwise. On the contrary, many of them have seen their careers stalled or ruined because they pursue this research. [Beaudette, ibid.] So there does not appear to be any motive to commit fraud, and it would be difficult for roughly 4,000 people to commit fraud and keep this fact secret.

==Claims that all cold fusion researchers are deluded or incompetent==

Articles in the press and in major scientific magazines have often said that all cold fusion scientists are practicing pathological science and all are deluded or incompetent. Robert Park made that assertion in the *Washington Post* article quoted above, in his weekly column, and in e-mail letters to cold fusion researchers. One of the members of the 1989 DoE ERAB panel, Prof. W. Happer, said that: "Just by looking at Fleischmann and Pons on television you could tell they were incompetent boobs." [Taubes, ibid.] F. Slakey, the Science Policy Administrator of the American Physical Society, said that cold fusion scientists are "a cult of fervent half-wits" "While every result and conclusion they publish meets with overwhelming scientific evidence to the contrary, they resolutely pursue their illusion of fusing hydrogen in a mason jar. . . . And a few scientists, captivated by [Fleischmann and Pons'] fantasy .....pursue cold fusion with Branch Davidian intensity." [31] S. Koonin said in May 1989 at an APS meeting, "My conclusion is that the experiments are just wrong and that we are suffering from the incompetence and delusion of Doctors Pons and

Fleischmann." [32] Skeptics sometimes say they have not made such intemperate comments, but there are hundreds of well-documented examples from major establishment scientists, publishers and officials. [Mallove, Beaudette, ibid.]

The people making these comments repeated them often, and they have never retracted or apologized. They say they are not exaggerating and they honestly believe that Fleischmann, Pons and all other cold fusion researchers are severely deluded and grossly incompetent. The people listed above were assigned a direct role in establishing U.S. policy toward cold fusion, in the ERAB panel and elsewhere, and their views have had great influence.

As noted above, leading cold fusion researchers and theoreticians include many prominent experts in electrochemistry and physics. It is unlikely that such people are deluded or incapable of performing experiments in their own fields.

==Claims that experimental errors have been made==

Since cold fusion is an experimental claim, cold fusion researchers feel that the only way to prove it is wrong is to demonstrate there is an error in the experimental technique or instruments. However, only a few skeptical authors have searched for such errors. Skeptics have written one book, at least five papers, and one magazine article citing errors, but cold fusion researchers feel the skeptics have failed to make their case. Some well-known examples include:

* D. Morrison versus M. Fleischmann debate [33] Morrison accused Fleischmann of employing "a complicated non-linear regression analysis" . . . "to allow a claim of excess enthalpy to be made." Fleischmann pointed out that they did not use that analysis. Morrison estimated that recombination might have produced 1.1 MJ at 145 W maximum power; Fleischmann pointed out that chemistry textbooks prove it could only produce 600 J at 5 mW maximum power. Several other points are disputed.

* N. Hoffman, *A Dialogue on Chemically Induced Nuclear Effects* [23] Much of this book is devoted to the hypothesis that all tritium results in cold fusion were caused by contaminated heavy water. This heavy water was contaminated, the author says, because the Ontario Hydro Company sold used moderator water from Candu fission reactors through chemical supply houses, to members of the public. There are two problems with this hypothesis. First, tritium levels are always measured before the experiment begins, so if the heavy water was already contaminated this would be noted. Second, upon learning about the book, Ontario Hydro vehemently denied selling used moderator water. They pointed out this would be illegal because this water contains roughly 100 million times more radioactive contamination than allowed by law, and it would cost tremendous amounts of money to remove this contamination and make the water safe. (Other sections of the book dealing with the Joule-Thompson effect and helium studies do not dispute the published results.) Cold fusion researchers at SRI pointed out that this book does not mention any experiment showing excess heat even though most experiments in the field have been done to look for heat.

* W. B. Clarke and Mass Spectrometry. Clarke published several articles with coworkers examining various aspects of helium detection in cold fusion-related samples. One study [42] has led to suggestions that some of the cold fusion researchers have not adequately handled the analytical chemistry technique of mass spectrometry.

* K. L. Shanahan and Calibration Constant Shifts. K. L. Shanahan has published 3 articles proposing a non-nuclear means to obtain apparent excess heat signals in cold fusion cells. In the first[34], Shanahan proposed that the system heat distribution and/or heat flow pathways might change and that this would induce a change in the calibration constants for the cell. He reanalyzed some actual cold fusion data published by E. Storms[37], under an assumption of zero excess heat, and found a variation in calibration constants of +/- 3% would explain the results, which is within typical error bounds of a fairly high quality scientific study. Subsequently, Szpak, Mossier-Boss, Miles, and Fleischmann questioned the proposal[39], and Shanahan replied [35] with an expanded explanation and applied it to explain the authors results. E. Storms rebutted Shanahan [38] by pointing out that the data does not fit his model.

* A *Scientific American* article in 2006 with a sidebar that said there are four weaknesses in the cold fusion experiments. [40] (The article is here [http://www.sciam.com/article.cfm?articleID=00059015-99C5-1213-987F83414B7F011C&ref=sciam&chanID=sa006], but the sidebar is missing. It is quoted here: [http://lenr-canr.org/News.htm#SciAmSlam].) These statements are not in evidence in the experimental literature. In a letter to J.

Rothwell, the editors said they have not read any of the experimental literature on cold fusion, because, they say, this literature does not exist: no papers have been published "in major peer-reviewed journals." [Piel, ibid.] Presumably this explains why the editors made these four errors.

==Burden of proof argument==

Many skeptics have said that the burden of proof is on cold fusion researchers to prove their point. As the editor of the Scientific American put it: "But it is not up to mainstream physicists to disprove LENR-CANR [cold fusion]; it is up to LENR-CANR's physicists to come up with convincing proofs. The burden of evidence is on those who wish to establish a new proposition." [Piel, ibid.]

Cold fusion researchers feel they have met this burden. Cold fusion experiments are based upon traditional instruments and techniques, such as calorimeters (most of them developed between the 1780 and 1840), autoradiographs (circa 1890), and conventional tritium detection and mass spectroscopy. Calorimetry is based upon the laws of thermodynamics. Since most skeptics agree that autoradiographs, the laws of thermodynamics and so on are valid, cold fusion advocates argue that the skeptics should agree that cold fusion experiments are valid, and that the burden of proof is on those who say these techniques and laws are inoperative.

==Other skeptical arguments==

Some skeptics say they do not believe the results because there may be an error in the experiments which has not yet been discovered. This argument is invalid because it cannot be falsified, and because the same can be said for any experiment.

Skeptics quote Carl Sagan's axiom that "extraordinary claims require extraordinary evidence." Cold fusion researchers disagree. They feel that extraordinary claims are best supported with ordinary evidence from off-the-shelf instruments and standard techniques, and this is the kind of evidence they have published. They also feel that all claims, and all arguments (including skeptical assertions that attempt to disprove cold fusion) must be held to the same standards of rigor.

Hagelstein has encountered skeptics who say "a commercial product is the next hurdle to be jumped through before any significant funding can be justified." He responded, "This is simply not right." He explained:

> "Scientists in the field have gone to extremes in attempts to satisfy skeptics. Cells were stirred, blanks were done, extremely elaborate closed cell calorimeters have been developed (in which the effect has been demonstrated), the signal to noise ratio has been improved so that positive results can now be claimed at the 50 sigma level, the reproducibility issue has been laid to rest; but still it is not enough." [41]

Hagelstein and others point out that plasma fusion has failed to produce a practical power reactor despite 60 years of research and over $100 billion in funding, yet no one questions the existence of plasma fusion for that reason. They also point out that countless other natural phenomena have no practical application, but are not disputed.

Lindley [ibid.] and many other skeptics have said that before they believe the experimental results, cold fusion researchers must first provide a complete theory to explain the phenomenon. This also violates a fundamental tenet of the scientific method, since there are and always have been countless unexplained phenomena which are unquestionably real (such as high temperature superconductivity and radium fission, as noted above). Cold fusion researchers feel that it is the job of science to explain anomalies rather than to dismiss them.

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Additional input from Jed. (4/23/06):

Writing this Wikipeida article has been an education for me. It has revealed aspects of the skeptical imagination and thought processes I was unaware of. One thing that shocked me is how many skeptics are livid with anger because I am quoting verbatim Huizenga and other leading skeptics. Several times they have erased these quotes. They do not want to hear their own side!

Here is an example of what I have learned. This is kind of trivial yet thought provoking. The skeptical argument that all researchers are incompetent or frauds is weak. It has to apply to 100% of researchers. If a skeptic admits that 1% or 2% are competent and honest, that makes cold fusion real. There are very roughly 3,000 to 4,000 authors of the positive papers at LENR-CANR. So 1% would be ~40 researchers in ~10 institutions. Most researchers worked ~10 years. (Most are now retired or dead.) So that would be 10 or more independent replications repeated dozens of times. I have never heard of anyone publishing a claim that only one cell in run produced heat, or tritium, or transmutations. Bockris used to run 100 cells at at time.

Anyway, in normal science, that many replications would be considered conclusive proof that an effect is real. Some people want to see 3 or 4 replications, others hold out for 10. But dozens of repeated experiments at 10 labs would surely meet anyone's standard of proof. I guess this is why some skeptics are so adamant. Their claim must be airtight or it fails. Cold fusion has been replicated hundreds if not thousands of times. Of course most skeptics are unaware of this. They think there were only a dozen experiments. But take a skeptic who is vaguely aware of the facts -- one who has glanced at the list of papers at LENR-CANR, and shivered inwardly. He has to force himself to believe that every single one of these tests was a mistake, or fraud. It must a terrible mental burden, forcing yourself to believe such crazy ideas, in violation of all your professional training and common sense.

That is why so many skeptics suffer from cognitive dissonance. They shout to drown out their own inner doubts. They insist that I have distorted their views in this article, when in fact I have reproduced their views faithfully and completely, with verbatim quotes from the best sources available. If a skeptic were to write what Huizenga wrote, 'we know a priori all excess heat results are wrong,' his fellow skeptics would clap him on the back and say 'right on bro, Amen to that.' When I quote Huizenga, and source it, and compare that to Schwinger's rebuttal, they become hysterical and they accuse me of "POV violations" (expressing a point of view). One of them came in and immediately erased all of the rebuttals. He wanted to see skeptical claims in isolation, and he had no sense that they represent a "point of view" (POV). But when they were paired with the CF researchers point of view, he felt it was an abomination.

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294) A 1995 overview of the field by Richard Oriani

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Looking for something else I found an interesting reference to a 1995 overview of the cold fusion controversy. It is a document worth preserving. The overview was presented orally in an address by Richard Oriani to alumni of the Institute of Technology at the University of Minnesota. I worked with Dr. Oriani about one year ago and know how strongly he is motivated to solve the mystery of cold fusion. Richard's background in physical chemistry, metallurgy and material science made him .well prepared to study cold fusion as soon as it was announced. What I am posting below was taken from an e-mail message of <apatula@ub.d.umn.edu> posted on 3/2/1995. The author tells us that Oriani's address was printed in the University of Minnesota's Office of Technology "Research Review," Vol. 24, No. *. February 1995, pages 1, 4-5.

Cold fusion, Richard Oriani told an audience of Institute of Technology alumni, "is certainly worthy of study and funding."

There are good reasons for skepticism, he acknowledged, but there are also good reasons for genuine interest. "Here is some new kind of nuclear physics, and it is too late to heap ridicule on it," he said.

Oriani, professor emeritus in the University's Department of Chemical Engineering and Materials Science, spoke at a December 7 seminar sponsored by the IT Alumni Society. He wished to give his audience "an appreciation of where cold fusion research is after these five years," he said. "People have made a lot of headway."

Oriani framed his presentation with comments on the reputation and abuse of cold fusion, but he devoted most of his time to reviewing two sets of data from the scientific literature: first, "credible experiments" by twelve groups of researchers, including Oriani's own group, who have measured energy production from palladium and deuterium at relatively low temperatures; second, reports from ten groups who have measured tritium, helium, neutrons and charged particles released from combinations of deuterium with palladium or titanium. Throughout his review, Oriani emphasized the lengths the experimenters went to avoid contamination of samples and error in instruments.

When nuclear reactions release energy, Oriani's explanation of the data began, it is because some part of the mass involved is converted to energy. For example, in one of the reactions theoretically associated with cold fusion, an atom of deuterium combines with an atom of tritium to yield helium, a free neutron, a decrease in mass of 0.0188 atomic mass units (amu), and energy at the rate of 1.49 x 10 raised to the -10 joules per amu (equivalent to 8.97 x 10 raised to the 13 joules per mole).

In the five most accurate energy- measurement experiments, the energy output ranged from 106 percent to 170 percent of the energy put into a palladium- deuterium system. For two groups of experimenters, there was a net gain of energy every time they tried the experiment. Oriani's group produced a net gain in two attempts, but thirty subsequent attempts produced no energy. The inconsistent results, said Oriani, seem to depend on the sample of palladium. His third success came after the thirty
failures when he obtained a new sample of palladium from a palladium was obtained from Texas A&M University. Other groups have measured, in three less accurate experiments, energy production ranging from 5 to 15 times the

energy input.

Oriani's second set of reports dealt with observed effects that could only result from nuclear reactions. For example: Fritz Will et. al., electrolyzed heavy water with cathodes made of palladium from two different suppliers. (Heavy water is D (subscript) 2 O, i.e. water containing deuterium rather than common hydrogen. Will was director of the Utah Cold Fusion Institute). One type of palladium yielded no tritium. The other type yielded tritium at 50 times the background level, in four trials out of four. From that second type of palladium, 140 samples not subjected to electrolysis were found to contain no tritium.

Melvin Miles and Benjamin Bush, using palladium and heavy water, produced helium in concentrations ranging from 5.4 parts per million to 9.7 ppm. The background concentration of helium in air is 5.2 ppm. George and Stringham, using sound to cavitate heavy water on palladium foil, produced helium at 10 times background levels in ten trials out of ten. Y. R. Kucherov, et. al., by means of "glow discharge" with a palladium electrode in low- pressure deuterium gas, produced helium at 4 to 100 times background levels and counted 10 raised to the 7 neutrons per second.

Skepticism and ridicule of cold fusion began in 1989, Oriani remembered, when Stanley Pons and Martin Fleischmann announced their discovery through publicity rather than peer review. "They described their work so poorly it seems they wanted to keep it obscure," said Oriani. His own interest in cold fusion was sparked shortly after that, by the work of Steven Jones at Brigham Young University.

Since the Pons and Fleischmann debacle, cold fusion experiments have not been adequately published, Oriani argued, because the journals Science and Nature have been "caustic and abusive" toward the work. When Oriani tried to publish his own experiments, he said, the two journals' replies were to the effect of "We already know cold fusion doesn't work, and you don't understand your results. We're not going to publish them." Oriani's reply: "Many things are published without full understanding, and that's the way it should be."

Oriani then published in the December 1990 issue of Fusion Technology (Oriani, John C. Nelson, Sung-Kyu Lee, and J. H. Broadhurst, "Calorimetric Measurements of Excess Power Output During the Cathodic Charging of Deuterium into Palladium," volume 18, pp. 652- 658). Fusion Technology and the Journal of Electroanalytical Chemistry and Interfacial Electrochemistry, where Pons and Fleischmann first published, are the only two journals still publishing such work, said Oriani.

"I want you to understand my attitude," said Oriani. "A new idea should expect to fight its way to recognition. But in this particular case the fight has been particularly hindered by ridicule. Cold fusioneers have been accused to incompetence, self-delusion, and pathological science. Bockris at Texas A&M was accused to fraud by Gary Taubes in Science [vol. 248 (1990), pp. 1299-1304], of doping his experiment with tritium. That certainly was not the case."

Among sound reasons for skepticism regarding cold fusion, Oriani acknowledged several:

One, "the results are not yet [consistently] reproducible, and we don't know why," he said.

Two, no one has satisfactorily explained what is taking place. "There are as many theories as theorists," said Oriani.

Three, classical physics says the nuclear reaction supposedly taking place can only take place under tremendous heat and pressure, like inside the Sun.

Four, "Cold fusion has attracted a lot of crackpots and mystics," said Oriani. "You have no idea the letters I receive from people who know cold fusion works because the spirit has told them."

It may not be odd, then, that cold fusion research has been difficult to publish or fund. Small federal funding has come from only one agency, the Office of Naval Research, said Oriani. A half-dozen other U. S. groups are working "on a shoestring." The University of Minnesota originally funded Oriani's experiments. They are now "self-funded," he says. In addition, the U. S. Patent Office rejects all applications that mention cold fusion.

Nonetheless, said Oriani, there are pockets of rich funding for cold fusion: SRI International (formerly the Stanford Research Institute) has $2 million a year from the Electric Power Research Institute (EPRI), Japanese interests have equipped a lab in Southern France for Pons and Fleischmann, and significant work is being done in several labs in Japan. "The Japanese are really going after this," said Oriani. "The U. S. is getting behind the 8-ball." A U. S. corporation is, however, buying every cold-fusion-related patent application it can get its hands on, he added.

And the reason EPRI and the Japanese are investing in cold fusion? "If cold fusion is real," said Oriani, "it's an inexpensive source of energy."

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295) Chiropractors had to fight for recognition

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The situation in the CMNS area made me think about chiropractors. These are people trained to deal with musculoskeletal problems. Not long time ago they were denounced by our medical establishment as incompetent intruders. Browsing the Internet I found an interesting short web site

http://www.chiropractic.ie/history.html

devoted to history of that field. At the web site of Dr. Furr I found that the "profession has long been and continues to be ridiculed for advocating the broader clinical utility of manipulative procedures, for example, for patients with cancer, diseases of the viscera, cardiovascular disorders and psychiatric conditions." In other words, chiropractors were accused -- usually by medical boards in different states -- for something the practitioners did not do, or at least were not supposed to do. The field was criticized not on the basis of practical successes but on the basis of ad hoc theories. The founder of chiropractic, D. D. Palmer, believed that his hands had magnetic effect on patients. Claims of "magnetic healing" are still being promoted by business-oriented organizations but this is not considered, as far as I know, to be a theory explaining successful attempts to remediate certain disorders.

Instead of addressing the issues of validity of procedures the establishment fought the emerging discipline by administrative procedures, most often in the form of legal accusations of practicing medicine without a license. And here is another quote, extracted from:

http://www.worldchiropracticalliance.org/consumer/history.htm

"At first, even though it proved to be a successful way of healing the body, chiropractic adjustments were not readily accepted. Years after Harvey Lillard's hearing was restored, the news media delighted in vilifying the pioneer chiropractor, whom they labeled a "charlatan" and a 'crank on magnetism.'

The medical community, afraid of his success and discouraged by its own failure to heal diseases, joined the crusade and wrote letters to the editors of local papers, openly criticizing his methods and accusing him of practicing medicine without a license." That was about 110 years ago. Today the field is recognized as legitimate, with degrees granted by accredited institutions, but some physicians are still antagonistic. How much of this is based on evidence against the effectiveness of chiropractic procedures and how much on economic competition for patients?

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296) About the origin of Mizuno-type excess heat

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My personal participation in high voltage electrolysis experiments (Mizuno type) has been described in several units, most recently in the unit #271. The goal of the Colorado-2 experiment was to check the excess heat claim made by Fauvarque et al. Reality of excess heat, as defined in Paris-1 experiments (COP>1), has been confirmed in our Colorado2 experiments. But what is the origin of that heat? Is it true that it cannot possibly be attributed to well known chemical reactions? Most people believe that chemical origin of excess heat, in Mizuno-type experiments has been ruled out by chemists. But I have not seen this issue being discussed. That is why I want to review earlier publications dealing with plasma electrolysis.

A brief description of the Mizuno type experiment was made by Eugene Mallove (1). I suppose that Jean Louis Naudin, who replicated many Mizuno-type experiments (2), was inspired by (1). Mallove would certainly refer to Naudin's experiments if they were performed earlier. I am not a chemist but I would not be surprised to learn that a large number of chemical reactions takes place during plasma electrolysis in a Mizuno-type cell. Some of these reactions are likely to be exothermic. What kind of evidence do we have that the unaccounted-for heat is not due to these reactions?

That possibility is not even mentioned in (1) and in (2). By naming his cells "cold fusion reactors (CFR)," Naudin implies that the unaccounted heat had nuclear origin. Mallove also made that assumption. Referring to (3) he writes: "Ohmori and Mizuno found major evidence for transmutation of elements and volcanic ejection of metals from the tungsten surface -- these SEM photos were reproduced in their article. They find Hg, Os, Kr, Zn, Cu, Ni, Fe, Cr, Si, and Mg -- with anomalous isotopic content." Yes, that was a good indication that some nuclear processes take place during plasma electrolysis. A detailed description can also be found in Mizuno's book (4).

But, as far as I know, neither Mizuno, nor anyone else, demonstrated a quantitative relation between the excess heat generated and number of nuclear reactions taking place in the cell. Suppose the excess heat is generated at the rate of 50 W. This translates into $3.12*10^{14}$ MeV per second. Assuming that, on the average, a nuclear reaction generates 10 MeV of energy one would expect $3.12*10^{13}$ reactions per second. A typical Mizuno-type experiment lasts about 600 seconds. During that time the number of reactions would be close to $2*10^{16}$. (or more if less than 10 MeV per reaction is released). I suppose that detecting such amounts of reaction products, even if they were stable, would be an easy task for a good analytical chemist. No comparable amounts of reaction products were reported by those who experimented with Mizuno-type cells.

The list of references in (3) points to a paper of R.M. Shaubach and N.J. Gernert (5). The authors of that paper reported very high coefficient of performance (COP=8). Their excess heat was generated at the rate of 41 W. But the number of nuclear reaction products was found to be negligible. On that basis the authors rejected the idea of nuclear origin of excess heat. They believed that new kinds of chemical reactions -- involving hydrinos -- are responsible for excess heat. The experiment described in (5) did not involve electrolysis. I am mentioning it because it was mentioning in (2). The paper attempted to explain excess heat without nuclear reactions. But reality of hydrinos is even more controversial than reality of cold nuclear transmutation.

Another reference in (3) is the paper of P.M. Kanarev and T. Mizuno (6). The plasmaelectrolysis experiment of Kanarev was performed at 220 V and the current was between 0.5 and 2 A. The experimentally measured COPs, up to 1.5, were said to be "less than the results of the calculations originating from the existing cold fusion theories [6]." The author of the theory, by the way, is Kanarev himself. That theory is far from being clear to me, probably due to my insufficient background. The experimental setup in the Figure 1 is also very confusing; I would prefer a simple drawing focusing on the principle of operation and not a patent illustration with unexplained labels.

The authors claim that excess heat, generated at the rates of tens of watts, was due to cold fusion. My first impression was that Tables 3 and 4 will show how excess heat is correlated with the number of reactions products. But that turned out to be an illusion. The percentages shown in the tables (for example, 1.1% for Al) refer to surfaces; what is needed is the total number of atoms produced in the cell. The total number of atoms of the surface of the cathode is not specified and this prevents one from turning percentages into numbers of atoms.

Added on 4/28/06:
Unable to find what I wanted (justification of nuclear origin of excess heat in Mizuno-type experiments) I turned to (3); Mallove and Naudin were presumably inspired by that paper. In the introduction Ohmori and Mizuno wrote: "To our surprise, we observed the strong excess energy evolution to such an extent that the electrode became incandescent . . . " The COP, in this seminal experiment, was reported as 2.6 while excess energy was generated at the rate of 183 W. Tungsten was chosen as a cathode because "in order to withdraw greater nuclear energy" one needs a material whose binding energy per nucleon is low. Seven existing cold-nuclear-transmutation reports were listed as motivation for the first Mizuno-type experiment. Two electrolytes were used -- Na_2SO_4 (0.5M) and K_2CO_3 (also 0.5M).

Referring to high amount of excess energy, and to products of nuclear transmutation -- such as Pb, Fe, N, Cr, and C -- the authors conclude that nuclear reactions taking place in tungsten are likely to be responsible for its incandescent temperatures. But this is not very convincing. A claim "nuclear reactions occurred during plasma electrolysis" should not be confused with a stronger claim "excess heat in plasma electrolysis is due to nuclear reactions." The tentative conclusion of Ohmori and Mizuno would be much more convincing if the amount of nuclear reaction products were shown to be consistent, at least within one or two orders of magnitude, with the amount of excess heat.

Added on 4/29/06:
Trying to find more convincing evidence I turned to another paper of Mizuno et al. (7). In that paper the authors confirm production of new elements and generation of excess heat. The issue of non-nuclear origin of excess heat is addressed but a conclusion is reached that chemical reactions cannot possibly be responsible for the measured amount of excess heat. Here is the main line of reasoning:

1) It is well known that 380 kJ of heat is released when one mole (183.85 grams) of W is consumed to produce H_2WO_4.

2) In reality only 0.1 grams of tungsten was lost during a test in which excess heat was measured. That, could produce 0.207 kJ of heat. The amount of excess heat measured, 54.4 kJ, was found to be considerably larger. Thus no more that 0.4% of excess heat measured could be due to chemical consumption of tungsten.

3) This 0.4% fraction is actually an exaggeration because the 0.1 grams of tungsten lost from the cathode was found at the bottom of the cell in the form of pure metallic powder. In other words, tungsten did not react with oxygen, it was simply removed "by hydrogen corrosion as well as heat damage."

4) Another chemical reaction taking place in the cell, during plasma electrolysis, is "the decomposition of the carbonate in a water solution." That reaction is said to be endothermic; it removes 274 kJ of heat per mole of K_2CO_3.

I do not understand the last comment. The 0.2M of K_2CO_3 is dissolved in water when the electrolyte is prepared, usually hours or days before the experiment. At the time of an experiment the potassium exists in the form of ions, not in the form of K_2CO_3. How can decomposition of K_2CO_3 take place during the experiment? But the argument against the "tungsten is a chemical fuel" idea makes sense to me. What is missing, however, is a statement that no other

exothermic reactions can take place during an experiment. Should I assume that formation of $H_2 WO_4$ from W is this the only possible exothermic reaction?

It is important to keep in mind excess heat per si was not the main part of the CMNS controversy when Fleicshmann and Pont published their findings. What was violently opposed was the hypothesis that excess heat has nuclear origin. I recall reading that, according to F&P, and others who investigated the phenomenon, the chemical origin of excess heat was ruled out. That was accomplished by considering all exothermic reactions that were possible in the cell. The combined heat released by these reactions was shown to be negligible in comparison with the excess heat measures. That is how the idea of nuclear origin was justified. A similar statement is needed about plasma electrolysis. What is true for the F&P effect is not necessarily true for the M&O effect.

Personally, I do not exclude a possibility that excess heat we measured might be due to chemical processes. But that is a matter of attitude, not the matter of conviction based on knowledge of chemistry. Hopefully, recognized authorities in the field of electrochemistry will reassured people like me that the Mizuno type excess heat cannot be attributed to known chemical reactions. That will be a very significant step forward, with respect to what was stated in (7). But the issue will not be resolved until excess heat is shown to be compatible with the number of reaction products, at least to within two orders of magnitude. Such compatibility has been demonstrated for the F&P effect (accumulation of excess heat at the rate close to 23 MeV per ^{4}He atom). Will it also be demonstrated for the F&O effect? I hope so.

Appended on 5/1/2006
X wrote:
The link to Shauback and Gernett in references should be
<http://www.hydrino.org/Labs/Anomalous-Heat-from-Atomic-Hydrogen.pdf>
taken from <http://jlnlabs.imars.com/cfr/index.htm>

The home page is <http://www.hydrino.org/> which is the hydrino study group (Mills). The reports page contains some reproductions of Mills done by others.

[You wrote: "But reality of hydrinos is even more controversial than reality of cold nuclear transmutation." If by 'hydrino' you mean "hydrogen with tighter orbitals", the statement is correct. However, the empirical 'odd / new hydrogen' is available for viewing in many of Mills RoD ionic vapor experiments anytime you care to run one for yourself.

References:
1) Eugene Mallove at <http://www.amasci.com/weird/anode.txt>
2) Jean Lois Naudin, at <http://jlnlabs.imars.com/cfr/index.htm>
3) Ohmori and Mizuno "Strong Excess Energy Evolution, New Element Production, and Electromagnetic Wave And/Or Neutron Emission in the Light Water Electrolysis with a Tungsten Cathode." presented at ICCF7,1997.
4) T. Mizuno; “Nuclear Transmutations: The Reality of Cold Fusion;” Infinite Energy Press, 1998.
5) R. M. Shaubach and N.J. Gernert “Anomalous Heat from Atomic Hydrogen in contact with potassium carbonate.” <http://Anomalous-Heat-from-Atomic-Hydrogen-1.pdf>. At the end of the paper the authors announce that they the next set of experiments will be performed under a contract with the US government. “Results will be available in the spring of 1994.” It is interesting that a link to this paper appears, at the website of R. Mills: <http://www.blacklightpower.com/techarchive.shtml>. The same website indicates that the experiment was performed by NASA. But the link to the NASA paper is broken. That is puzzling.
6) P.M. Kanarev, “Cold fusion by plasma electrolysis of water.” The date of this downloadable paper can only be inferred from references; the most recent are from 2002. <http://guns.connect.fi/innoplaza/energy/story/Kanarev/coldfusion/>. The paragraph next to Table 1 implies that Mizuno name was added because chemical analysis of cathodes was performed by him.
7) T. Mizuno, T. Ohmori, K. Azumi, T. Akimoto and A. Takahashi; “Confirmation of heat generation and anomalous element caused by plasma electrolysis in the liquid;” Conference Proceedings Vol. 70, ìICCF8î Società Italiana Di Fisica, Bologna, 2000, p. 75

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297) From recent messages on the CMNS list

Ludwik Kowalski; 5/7/2006
Department of Mathematical Sciences
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1) What is the current situation with Colorado2 results?
1) Experimental results (COP=1.24, st.dev. = 0.13), described in the unit #271, at my CMNS web site, should be summarized and published. As I wrote before, I will start writing the draft as soon as Pierre Clauzon sends me conclusions from Gerard's analysis, and the appendix written by their reputable chemist. I expect that Gerard's investigation will determine the percentage of tiny droplets in the escaping mixture of vapor and liquid. Presumably that percentage will be much smaller that 24% needed to create an illusion of COP=1.24. Richard also promised to investigate the possibility of that kind of illusion. The appendix will presumably rule out a possibility that excess heat is due to well known chemical reactions.

While waiting for these two things I started worrying that the COP>1 might be an illusion due to the temperature dependence of L, latent heat of evaporation, as shown in:

http://www.spiraxsarco.com/learn/modules/2_15_01.asp

But this turned out to be a false alarm. The latent heat of evaporation is nothing else but the difference between the enthalpy of one kilogram of water in our highest state, escaping vapor, and our lower state, liquid water at 100 C. In other words,

L=H(v)-H(l).

Enthalpy, H, is a state function. It means that L does not depend on what might happen to a unit mass of water between the initial and final states. Inspired by a contribution of Michel Jullian (spreadsheet for thermochemical calculations) I wrote:

"Mizuno, Naudin and Iorio did not comment on what I wrote about the possible illusion of excess heat resulting from the temperature dependence of L. Why are they not responding? Does it mean they agree about the possibility of illusion or that they agree with what Michel J. and John N. wrote about the issue? For some reason messages from Michel and John were not clear to me. But this morning someone explained their position to me. This convinced me that the temperature dependence of the enthalpy of evaporation, L, cannot possibly be the cause of an illusion, at least not in the way that I was thinking.

The argument goes like this. Enthalpy is a state parameter. Suppose that one gram of water goes from state A to state Z in a sequence of steps,

A --> B --> C --> D --> E --> . . . --> Z

A is when water is at 100 C
B is when water is at 300 C (for example, very close to the cathode)
C is when that water becomes vapor
D is when that vapor changes into XXX (for example, by a sequence of nuclear reactions)

E is when XXX turns into YYY, etc. etc.
. . . .
Z is when the LSE (last something else) becomes one gram of steam escaping from the Mizuno-type cell.

The amount of electric energy, Q, needed to transform 1 gram of water from A to Z does not depend on what happened between A and Z, it must be equal to (Hz-Ha), where Hz and Ha are enthalpies in the states Z and A. ..... In other words, poorly understood intermediate processes (what happens during plasma electrolysis) are totally irrelevant in the contest of excess heat, as in Paris1 and Colorado2 experiments. The COP>1 can be an illusion associated with tiny droplets (mistakenly assumed to be vapor), but that has nothing to do with the temperature dependence of L."

Richard Slaughter is experimenting with a new cell; it is the beginning of his Colorador3 experiment. Instead of using an open cell, as in Paris1 and Colorado2 setups, his new cell is nearly totally closed. A small hole in the lid prevents accumulation of potentially explosive hydrogen and oxygen. The pressure in the cell is very close to one atmosphere. Only a small amount of vapor escapes into the air, most vapor condenses inside the cell. The mass of the escaping vapor is measured, as in Colorado2 experiment. With this approach the COP is calculated as:

COP = (Q1+Q2+Q3) / E

where E is the electric energy received by the cell during an experiment while (Q1+Q2+Q3) is the thermal energy removed from the cell. As before, Q1 stands for the energy needed to produce the escaping steam while Q2 stands for the energy lost by conduction, convection and radiation. The ways of measuring E, Q1 and Q2 were described in unit #271. And what is Q3? It is thermal energy removed from the cell by a flow calorimeter. The cell is designed to minimize Q1 and Q2. But these two quantities are measured, as in Colorado2 experiments.

The flow calorimeter, also acting as a condenser, is the major addition. It is a flat spiral copper tube below the lid (inside the cell). Icy water enters the tube at one end and comes out at the other end, at a slightly elevated temperature. The Q3, for a known rate of flow, is calculated from the measured change in temperature. Most of the vapor created during the experiment condenses on the spiral tube. Suppose that 100 grams of liquid water is turned into vapor but only 10 grams of that vapor escapes from the cell. The remaining 90% condenses on the cold tube and drops down into the electrolyte. Condensation is the heat-releasing process; the latent heat of condensation is numerically equal to latent heat of evaporation. That is why Q3 becomes the dominant component of the thermal energy output.

According to private messages from Richard, preliminary results confirm previous findings -- the COPs are always between 1.1 and 1.3. The new approach is much less dependent on tiny droplets because the mass lost during an experiment is very small. The nearly-closed-cell approach, as in preliminary Colorado3 experiments of Richard, seems to be a very good way of validating reality of COP>1. The totally-closed-cell approach seems to be more complicated (I am thinking about the heated recombiner) and quite dangerous."

The message shown below (blue font) was posted on the restricted CMNS list the day before yesterday. But nobody responded, so far. For some reason most of the recent messages posted on the list do not discuss science. The authors prefer to share historical information, criticize pathological skeptics and discuss social aspects related to negative attitude of scientific establishment toward our field. My expectation was that the list will be used, mostly, to share information about ongoing experiments and to discuss scientific aspects of ongoing investigations. But that is not what I observe. Why is it so?

I did not plan to ask this question. It evolved from my private correspondence with Richard. He shared my initial concern about L and I decided to publicize it on the list. My expectation was that some scientifically-oriented people will comment on what I wrote below. The absence of replies forced me to ask the "why is it so" question. If somebody asked me to comment of a scientific topic, and if I had something to contribute, for example, to criticize or to agree with an idea or proposal, I would do so at once. That is why I expected to hear from Mizuno and Naudin. Why didn't they answer, one way or another?

[I am removing that message because it was wrong. Was this obvious to Mizuno and Naudin? If it was then why didn't they correct me?]

2) What is pure research for?
An interesting message appeared on the list, several hours after the above was posted. Referring to an article in The New York Times (4/27/06), one subscriber asked about benefits society received from research in the area of high energy physics. My reply was as follows:

"Suppose a politician asked Michael Faraday about usefulness of electromagnetic induction that he was discovering. The answer would probably be "Someday you can tax it." I know that many are familiar with this phrase; it is often attributed to Faraday. The episode presumably took place around 1830s. Faraday was probably motivated by desire to discover laws of nature, not by vision of transformers and generators supporting the way of life of future generations. I do not think it was possible to anticipate usefulness of electromagnetic induction when it was discovered, nearly simultaneously, by Faraday and by Henry. That is what I used to tell students each year. How can I resist to say it again?

I do not know what fraction of our GNP (gross natural product) should be allocated to pure research. The National Academy of Sciences should answer this question. Yes, I know, that this is a political and economic issue, not a scientific issue. The primary purpose of this list was to discuss CMNS topics. Yes, I know, everything is interconnected. Yes, I know that CMNS was nearly killed by politically-oriented people. But what I would like to see more often are message describing work in progress, and issues generated by ongoing projects."

3) Added on 5/8/06:
Two messages I posted during the weekend did not generate any replies. Does it mean that people agree with me about a possibility of illusive COP>1? I do not think that it means this. It probably means that people do not care. They prefer to express opinion on topics which belong to sociology and history of science rather than on scientific topics per sei. About ten such messages appeared in the last two days. Here is an interesting quote from one of them. Responding to what others wrote Jed Rothwell observed:

". . . I think that on balance, at least with regard to revolutionary discoveries such as cold fusion, if we must choose between anarchy and excessive control, anarchy is better. Perhaps that is not the case with more conventional or incremental breakthroughs.

Consider this. If people such as Mizuno and Miles had not been fully tenured, and a Fellow of the Institute in Miles' case, they would not have been allowed to do cold fusion research. They had the freedom to defy the norms and do "forbidden" research. They paid a high price for their actions, by the way. The people in charge would have stopped them if they could have, just as they tried to stop Bockris and others.

Anarchy also works better in free market capitalism. In fact, I doubt anything else can work. If large companies such as IBM were given any control over one-man startups, such as Microsoft in 1975, progress would come to a halt. Gates succeeded because no one had any right to tell him what to do. If IBM or any other established company had been aware of his activities, or if they had been allowed to interfere, we would still be using mainframe operating systems..... "

4) Added on 5/9/06:
Responding to a message posted last night I wrote: "I think that X, who is new to this list, is correct. We need more sharing about what we do, and more messages focusing on science, as opposed to sociology of science. Why our best electrochemists, mentioned by Jed -- McKubre, Storms, Miles and Oriani -- did not share their ideas about the GDPE (glow discharge plasma electrolysis) on this forum? Mizuno-type cells of Naudin, Favaurque, Iorio and Slaughter yielded reproducible results. But can we be sure are these results demonstrated reality of excess heat? What should be done to make these results more convincing, as far as excess heat is concerned? Only highly qualified chemists can help us. Why are they consistently silent on that topic? Yes, I am also a newcomer; perhaps there were debates about this in the past and people do not want to repeat themselves. Please summarize past debates for newcomers. Or maybe our experts feel that criticizing each other is not appropriate when we are surrounded by enemies. Something seems to be wrong among us. But I do not know what it is. Yes, this message is also about sociology rather than about science. I am sorry about this.

5) Post Scriptum
I was ready to press the SEND button what this message from McKubre arrived. It was a reply to Jed's message:

On May 8, 2006, at 2:44 PM, Jed Rothwell wrote:

It is a safe bet that the present experts in cold fusion, such as McKubre and Storms, are the people most likely to find the answer. Shockley and his coworkers had the best chance of developing practical transistors in 1948. But you can never be *certain* of that. Someone might come out of left field with the theory or procedure that makes no sense to McKubre -- something that seems counterintuitive, or downright crazy. And yet that oddball person might be right. Such things have happened many times in history.

Mike McKubre wrote:
Thanks for the plug Jed, but the answer to our hidden variable(s) almost has to come out of left field; we have covered right field and it isn't there. I do think it is a bad strategy and very much counter productive to attempt to get responses on the CMNS forum by verbal bullying or induction of guilt. In such a forum you need to accept any wisdom shared as a gift, but you should not attempt or expect to compel a response. I don't speak for Dardik or the Energetics folks, or Mizuno or Bockris or any of the others mentioned below, but I know something of their character. I can guarantee you that if these folk believed the CMNS forum to be the most important way they could advance their knowledge - and the world's knowledge - of condensed matter nuclear effects, then you would have their full and active participation.

That reminded me of another group of highly qualified experts. Why are they silent on this list? I know that Darik and his colleagues are reasonably fluent in English. And why don't we hear from Mizuno himself, or from John Bockris? Or from Pamela and Stan? Or from John Dash and other material scientists? Yes, "something seems to be wrong among us."

6) Another 5/9/06 addition:
Here is how Jed Rothwell answered my question
"[McKubre, Storms, Miles and Oriani do not comment] because they have nothing to say about it. That's what they tell me. They say they have not done the experiment themselves, so they cannot comment. That's the mark of a good electrochemist: he does not speculate about a remarkable experiment that he has not done." Will each of them answer my question in that way? That remains to be seen.

Replying to Jed I wrote: ". . . I would like to know what Mizuno would say to someone suspecting that the COP=1.24 (st.dev.=0.13), as measured in Colorado2 experiment, might possibly be an illusion due to the temperature dependence of L. You communicate with Mizuno; please ask him this question. Mizuno probably saw my two recent messages about L. But to make things easy I am going to paste them below. Thanks in advance. And give him my regards. ....."

Added on 5/10/06:
Last night Rothwell wrote: "I have been meaning to comment on this for a long time. Whenever there is an effusion of creative effort, a large portion of the work is lousy. That is the nature of things. People make mistakes before they get things right. .....It takes a while to separate the wheat from the chaff." That was a comment about absence of mutual criticism, etc. Someone asked about thousands of papers
in the www.lenr-canr.org archive. What fraction of them contain invalid claims? Will this good question be answered? I do not think so. We are surrounded be enemies and some people might think that mutual criticism will be perceived as weakness. In my opinion, mutual criticism is a sign of strength. It lowers the probability of chaff being mixed with wheat. And here is how the question was answered by Jed Rothwell: "Ooofff ..... That's a delicate question for me. Naturally people should judge which papers are likely to be valid, and which are not. Storms and I listed some of the ones we find particularly impressive in the Special Collections. I think some of the other papers are marginal, sloppy, or farfetched.

However, I would not feel comfortable listing the ones I dislike. That isn't fair to the authors. Of course I am just the librarian. Not an editor or an arbiter! If anything, I resemble the guy who distributes push-pins and tape during a conference poster session. However, a librarian does have some influence over readers. He puts out books in the

"featured" shelf. He selects new books to be purchased. People ask librarians for recommendations, and readers do ask me for papers covering specific topics. Inasmuch as I have any influence I am determined to remain as neutral as I can. I would not want to prejudice readers against an unknown author who happens to have a good idea that I do not appreciate. . . . We have only turned down a handful of papers, mainly from perpetual motion machine inventors . . . This is a library, not a journal. We never turn down papers we disagree with. ..... "

Here is how Edmund Storms explained reasons for not being enthusiastic about Mizuno-type experiments. He wrote: "You ask why I don't share my ideas about the GDPE experiments. Here are several reasons:

a). I'm busy with my own studies which are hard enough to understand without adding an effort to understand various attempts at replication. Some very unexpected processes are operating that are hard to understand even when a person has access to all of the information, which is not the case when helping someone else.

b) The GDPE method, I believe, is not a good method to use to study LENR or to prove that the effect is real. It is too complex to replicate and interpret.

c) I have commented and made suggestions which were not accepted. It takes too much time to show why the suggestions are worth considering for the effort to be useful to me.

d) No additional replication of the effect will be useful to change minds unless the experiment involves measuring radiation and nuclear products along with heat. In addition, the relationship between the amount and nature of the nuclear products, and heat must fit a plausible model. Anything less than that will be just a good science experiment done for personal amusement and education. The field has gone beyond
needing just another replication.

e) Enough information is available in the literature to start such a project. Additional progress is only made by learning from mistakes and being forced to confront results that don't make any sense. My help would not advance this necessary process."

That is interesting. Would I get involved in GDPE experiments if I knew that Ed and other leading CMNS researchers think that results are too difficult to interpret? Probably not. My impression was that a large excess heat from a Mizuno-type cell is
easier to interpret than many excess heat experiments.

7) L for heavy water:
Referring to L (latent heat of evaporation) another leading CMNS researcher, Mel Miles, wrote that "it would be better to call L the ENTHALPY of vaporization. Nevertheless, the term HEAT is often used in textbooks." In another message Mel wrote:

"The symbol L represents the Heat of Vaporization-Often expressed as "Delta H(vap)". It is a state function like any other "Delta H" term. Similar to most thermodynamic quantities, it shows a temperature dependence. In my work for D2O ,I always used the following that I found in the published literature:

L =85263.9-173.429*T+0.2586*T*T-0.000191913*T*T*T-1805569/T

where T is in Kelvin and L is in J/mol. Thus, at 25.000 C or 298.15 K, L is calculated to be 45401.6 J/mol. The L values for the vaporization of D2O can be readily calculated at any other temperature of interest."

Using this formula for T=373 K (100 C) I got L=41753.6 J/mole. This translates into or 2087.7 J/g. For T=473 K (200 C) the formula gives L=36962.1 J/mole. This translates into 1848.1 J/g. There must be a similar formula for H2O and asked for it, and for a reference. It is easier to calculate L from a formula than from a chart I used before.

8) Appended on 5/11/06:
An interesting observation about the CMNS field was made by an stranger, Mr. V. Godbole (who did study some

physics. Life circumstances forced him to work in other fields but he does study theoretical physics on his own. In a message received yesterday V.G. wrote: "The solution of the Schroedinger equation - these strange counter-intuitive orbitals caught my attention. Does the particle (electron) spread itself into cloudy s-p-d-f-lobes, does it whizz around the nucleus along warped trajectories, how do the overlaps and hybrids come about - all that mystery or magic puzzles me (and many others). The deuteron is a (quasi) boson for distances above 10 raised to -15 m. How does that work out for an occasional slightly-mobile deuteron in a homogenous deuteride substrate? Can we devise a substrate-hamiltonian H that "condenses" deuterons into soliton-like or coherent-wave-like entities that emerge as a beam (surely a non-trivial difficult and challenging task)? . . .

Are the physicists a frightened (self-) terrorizing community? How can there be anyone an "expert" regarding a purely speculative new quasi-phantastic idea? . . . I have read many "successful" routine experiments in the journals which were just a waste of time and money (but meticulously done and exquisitely presented on expensive paper only for getting a trivial doctorate or habilitation). It is disappointing to say the least. Gone is a Julian Schwinger or Richard Feynmann. If a man has titles (Dr., Prof., Director etc.) then he wants to protect his "reputation". That's the end of pioneering physics.

Apparently, Mr. V. Godbole made several attempts to approach professional physicists but failed to gain their support. Not too many physicists are trained to discuss advanced theoretical ideas. They probably do what I do, when facing a topic with which they are not familiar -- they try not to expose their ignorance. It is hard to find a person capable to discuss a specialized problem. I do not think the refusals to get involved always indicate desire to protect one's reputation.

9) Added on 5/13/06:
The CEO (chief Executive Officer) of particular company, who is also a researcher, was criticized recently, on the restricted list for CMNS researchers. One of the issues was a promise, to potential investors, that a 1000 W will be on the market in several months. This is not the first time that a commercial application of CMNS is being promoted as nearly ready. But time passed and nothing happened. People feel that such episodes hurt our already bad reputation.

I do not think GDPE cells (glow discharge plasma electrolysis), also known as Mizuno-type cells are ready for practical applications. I am still waiting for an appendix about the expected heat from possible chemical reactions, the issue of the percentage of droplets mixed with steam remains unresolved. I would not invest a penny into a company that would try to commercialize GDPE cells at this stage of development. Several people responded. One of them wrote:

" There has been numerous posts as to the promise of a kilowatt OU unit from D2Fusion. I was waiting for someone to bring up Patterson's CETI. They had
a Palladium bead cell electrolysis column demonstrating this kilowatt ability already back in 1995 at ICCF-5 and the 1995 PowerGen Conference in Southern California. Eugene Mallove, Jed Rothwell and, I am sure, some members now belonging to this CMNS have witnessed the units. CETI was ready then to entertain interested parties then."
Another contributor wrote: ". . . But CETI seems to have faded out, particularly after the premature death of Jim Patterson's grandson, who was then their President. . . . " Jed Rothwell, wrote: ". . . I recall Patterson told me that this [inability to reproduce] is not true. He said he could make more beads "any time." But he never did make any as far as I know. Who knows what to make of it. It was yet another lost opportunity."

This was long before I discovered, in the summer of 2002, that cold fusion research was going on in several countries. Was it the first warning that one should not jump to commercial applications prematurely? That topic generated a long discussion. Let me end this unit with quotes from Mike McKubre and Jed Rothwell:

Mike wrote: " . . . The cf community has been aware of (in Jed's words) "exaggerations" for some time. We have overlooked them until now as we have been assailed from the outside and have naturally protected family first. Newer members do not have the perspective of seeing who did the work and how work groups were organized. Now that the era of commercial interest has arrived we can and should no longer turn a blind eye."

Jed wrote:

".. . As far as I know, nobody has ever been able to run GDPE cells with a steady output. Not 100 W or even 10 W. The excess power fluctuates wildly and uncontrollably. It seldom exceeds 30% of input. If you could generate 100 W steadily then of course it would be easy to asssemble several cells and generate higher power. However, with all of the cells I know about, if you assembled 10 of them you would end up with power varyingi from moment to moment, with input up to 700 W, and excess output ranging from 0 W to ~10,000 W (an explosion). This would be extremely dangerous. .....I do not think anyone knows how to run GDPE cells for a long time. People here have speculated about various methods, but I do not think these methods would work. ....." Then Jed added: "Jim Reding, the president of Patterson's company, was so upset by what I published about his experiments and business plans that he threatened to sue me. Krivit's expose of IESI was FAR more damning than what he has said about D2Fusion." In the last message Jed ,wrote"

"There is a claim at the D2Fusion website about kilowatt scale devices. It is here:

http://www.d2fusion.com/index.php?option=com_content&task=blogcategory&id=22&Itemid=49

This hyperlink in his message may be difficult to use. Here is the text: 'SUMMARY - D2Fusion Inc., a US company, is now preparing a fast track product development program to build and test solid state fusion energy modules. The first modules will produce a few kilowatts of thermal energy in a compact device comparable to a common household electric space heater. We plan to build and exhaustively test these prototype thermal modules during our first year of operation.'

That is audacious. If they can pull that off, I suppose they must have made a major breakthrough. Nobody I know could pull this off in a year This website is none of my business, and the people at D2Fusion have every right to ignore what I say, but I think they are asking for trouble with statements like this." Somebody asked about what to expect from a company that succeeded to make a big discovery but is still working on practical details. Once again, Jed was the first to reply and I am quoting him again.

"I am *not* dismissing that [possibiity]! Nobody here is dismissing that. We have only said that if they have accomplished this (or something similar), the way they are presenting it on the web looks suspicious. They are putting themselves in a bad light. I also said the accomplishment would be astounding, but: '.....maybe I read the wrong papers. Since I have not read D2Fusion's papers, I cannot judge the situation." [I also wrote:] "That is audacious. If they can pull that off, I suppose they must have made a major breakthrough.' [A also wrote] 'Of course there may be breakthroughs which I have not heard about. .....I interpret the statement at D2Fusion to mean there *has been* such a breakthrough. I hope this is the case.'

They [in the company] seem to be telling the world they stumbled on the clue. That's how I read the web page. It is a bad idea to tell the world that without offering any proof. It makes you look like a perpetual motion machine scam, even if you are actually legitimate. If they wanted to, and they felt it is advisable, they could easily publish loads of convincing proof without revealing any technical secrets. All they have to do is sign up independent experts to test the thing under NDAs, and then publish the technical outcome of the tests (calorimetry) without describing the hardware: "Prof. Smith tested the machine and found it produced 500 MJ with no input power ..... Here is a summary of his report....." Performance can easily be made public without revealing specifics about how that performance is achieved. .....If D2Fusion wishes to keep everything about this strictly secret, including the calorimetry, I think it would be best to say nothing. But apparently they disagree with me. That's okay. It's their website."

Added on 6/6/06:
This is another message from Jed Rothwell. It posted on the list as a reply to a suggestion made by another subscriber. The suggestion was to form a cold fusion party and to change this list into a platform of that party. I do not like this idea; I would unsubscribe if political topics became more important on this list than scientific topics. Jed does not have formal scientific background but his knowledge is some areas is impressive for a self-educated person. Here is his reply to a suggestion to form a "Cold Fusion political party."

It sounds a lot absurd to me. Third parties have often been formed around single-issue groups. They usually give the public the impression they are fanatics. I cannot imagine why we need a conventional political party. We do not need

millions of votes. We need only one vote of confidence from one influential person such as Bill Gates. As Margaret Mead put it: "Never doubt that a small group of thoughtful, committed people can change the world. Indeed, it is the only thing that ever has."

I think history shows how we may succeed -- if we can succeed at all. We must use conventional outreach strategies that have worked for scientists in the past, when they brought about unpopular reforms against entrenched opposition. Scientists pushed through sanitation, pasteurization, air pollution control, dietary reform in the U.S. Deep South to eliminate pellagra, child labor, automobile safety, and countless other science-based, science-led reforms. They fought ruthless opposition by vested interests. Sometimes it took decades. In New York City, the dairy industry prevented effective pasteurization of milk from the 1860s until 1917.

We have the tools to accomplish this task. We have much more to work with than the lonely public health warriors who fought pellagra did. We face a small number of angry physicists at the APS; they faced the Klu Klux Klan! We have many potential supporters; they had to overcome 300-year-old habits and economics that led to malnutrition and vitamin D deficiency then (and obesity today, alas). We have a body of impressive papers; distinguished researchers who could get a modicum of attention if they tried; a large latent group of supporters in the public; and we have been blessed by some of the stupidest opponents in history. Evolution's first great foe, "Soapy Sam" Wilberforce, was a genius compared to Park, Huizenga and Taubes. As Beaudette showed, Huizenga handed us victory on silver platter in his book. Why have we not made better use of this? We should work to appeal to students and young people, and at the other end of the scale, we must break through the wall of opposition to reach one or two influential people who can start the flow of funding.

There are simple rules and time-tested techniques for accomplishing this sort of thing, and cold fusion researchers have violated just about all of them. Instead of showing their results, they have hidden them. Instead of cooperating with one another they have fought over trivial matters. Instead of asking young people and others for support, they have alienated the public. Instead of appealing to hope and idealism, some have pandered to greed and the false promise of "cornering the market." As I said in the book:

"History has been a test of strength between the rapacious, foolish, greedy, shortsighted minority and the sensible majority. I expect the future will be the same. So far, in cold fusion, the fools have won every round, suppressing nearly all research. I have had a ringside seat at this fiasco. No one knows better than I how powerful the fools can be, and how badly the cold fusion researchers have muffed the few opportunities that have come their way. Without public support, researchers will never receive funding, yet they have often scorned opportunities to convince the public of the validity of their work. ....." Cold fusion researchers could do far more to help themselves and their cause. They have enormous latent power to influence society and win friends. Yet I have struggled for years -- begging, pleading, cajoling -- just to get them to send me 500 papers!

Appended on 6/14/06
Technical and scientific topics are occasionally discussed on our CMNS lists but comments on sociological aspects of science represent much more than one half of what is being posted. Let me describe another thread that belongs to the same category.

Two days ago Scott Little posted this message: “Group, I would appreciate some criticism on the following statement describing cold fusion and our work thereon that I have drafted for a paper I am giving at the upcoming AIAA Joint Propulsion Conference entitled, "Null Tests of Breakthrough Energy Claims". (no, I am not particularly proud of that title but that is how our cards have played out thus far). I will be discussing a wide variety of claims that we have tested at Earthtech and I feel that cold fusion should be included.

If possible I would like my statement to be acceptable to both camps; those that believe that cold fusion is real, and those that sneer scornfully at it. The statement comes after I have explained in general how energy claims are tested, including a brief discussion of calorimetry:

‘In March of 1989 Martin Fleischmann and Stanley Pons of the University of Utah announced that they had succeeded in making the D-D fusion reaction occur in an electrochemical cell near room temperature. Compared to the ordinary

conditions required for this reaction, this claim was aptly named “cold fusion”. The announcement of cold fusion generated intense interest as it promised to solve most if not all of our energy problems here on Earth. The fuel is plentiful and the waste products are relatively benign. However, widespread failure to replicate the phenomenon soon resulted in rejection of cold fusion by the mainstream scientific community.

Despite this rejection a number of scientists continue to investigate cold fusion. Hundreds of papers reporting positive results have been published and international conferences are held every couple of years. However, to this day, there exists no cold fusion demonstration experiment. That is because either the cold fusion phenomenon is extremely difficult to reproduce or the phenomenon does not exist. Notwithstanding the significant positive evidence that has been amassed, the latter possibility must remain until cold fusion can be reproduced on demand.

The primary signature of the cold fusion phenomenon is excess heat, which means that the electrochemical cell produces more heat energy than the electrical energy used to stimulate it. Thus calorimetry is necessarily involved in testing cold fusion experiments. In our laboratory we have expended a great deal of effort on the development of calorimeters suitable for cold fusion experiments. Over the years we have had the opportunity to test a relatively small number of cold fusion experiments, some that we constructed ourselves and some that were brought to our laboratory by other investigators who had seen positive signs of excess heat in their own labs. None of these cold fusion experiments have shown any convincing evidence of excess heat in our calorimeters. We cannot say that we have never seen *any* signs of excess heat in our laboratory because all calorimeters drift somewhat and, inevitably, that drift sometimes goes in a positive direction and looks just like a low level of genuine excess heat. When that occurs we strive to check the calorimeter’s calibration as quickly and thoroughly as possible. Usually the drift in calibration is evident and its magnitude matches, and thus explains, the apparent excess heat signal. In a few cases the calibration check did not explain the apparent excess heat signal. But when we returned the cell to the calorimeter after the calibration check, the excess heat signal did not reappear. This tantalizing behavior either means that the cell did produce low levels of excess heat for a while or the calorimeter was simply drifting up and down in unfortunate synchrony with our observations.

To put this drift issue into perspective, the calorimeter in question was designed with an accuracy goal of +/- 0.1% relative. At the typical input power level of 10 watts, that is equivalent to +/- 0.01 watts. One a good day, when freshly calibrated, this accuracy is actually achieved. A month after calibration, the system typically drifts off calibration by about 0.03 watts. The largest apparent excess heat signal we have ever seen that was not subsequently explained by a calibration check was about 0.05 watts, or 0.5% of the 10 watt input power.

Despite our disappointing experiences with cold fusion we feel that the potential importance of this phenomenon warrants further effort. We are actively involved in efforts to observe the excess heat phenomenon.’ Thanks for your consideration, . . .”

What follows shows how some people, reacted to Scott’s statement on cold fusion. The immediate reply from Jed Rothwell was:
”

[You wrote:] Notwithstanding the significant positive evidence that has been amassed, the latter possibility must remain until cold fusion can be reproduced on demand.

That is complete and utter nonsense. First of all, cold fusion has been reproduced on demand hundreds of times, including 100% of all experiments at Mitsubishi for the last 5 or 10 years. Second, even if it were not reproducible, this has no bearing on whether it is real or not. Reproducibility on demand has never been held as relevant to whether an effect is real or not. I can list dozens of discoveries and claims which are either extremely difficult or utterly impossible to reproduce. Examples include: Many of Martin Fleischmann's previous breakthroughs -- extremely difficult.

Cloning. The success rate is less than 1% -- far lower than CF ever was. Making a Pentium processor -- only two companies on earth have sufficient expertise and capital. Thermonuclear bombs -- very difficult and expensive to reproduce, fortunately. The top quark -- could only be done once, in one lab. Earthquakes, supernovas, the creation of life on earth -- can only be observed. Frankly, this assertion reflects appalling ignorance of the scientific method and

the history of science. Many people have said this kind of thing, even in the pages of so-called science magazines and journals, but still, the people who say this are completely wrong and they do not understand the scientific method.

Yesterday, replying to Jed I wrote: "Recent reading about the so-called 'scientific method' made me aware that it is mostly about validations of hypotheses and theories. It does not explicitly deal with validation of experimental facts. Scott's message is about facts, not about explanations. Yes, some experimental facts are difficult to confirm, especially at the beginning. What we need is at least one simple 'reproducible on demand' demo that can be studied in most science labs. ....." But nobody commented on this.

Replying to Scott, Edmund Storms wrote: " Universal reproducibility has no relationship to proving an event to be real. If a sufficient number of people see the same event under the same circumstances, the event is real even though the event can not be produced every time. Too many examples exist to make this concept even a debatable issue. The issue is not proving reality, but having sufficient control over experimental conditions to allow the event to be studied. LENR has been observed so often that the reality of the phenomena has been demonstrated. However, insufficient understanding is available to make it work very often. This has nothing to do with proving the phenomenon is real."

As I wrote in another item, CMNS (a more recent name for what used to be known as LENR or Cold Fusion) is not yet science; it is still protoscience. A protoscience will be recognized as science when at least one of its simple experiments becomes "reproducible on demand" in the hands of experts. Ed. is correct that "sufficient control over experimental conditions" is a prerequisite for reproducibility on demand.

Steven Krivit wrote: "Scott, you've asked a question which can't be truly answered. What you wrote is your view, and your experience. And that is the truth for you. Some of the people on this list have significantly different backgrounds, experience, skill and understanding than you relative to CMNS. Consequently, you asking for their opinions on your thoughts and views is a non sequitur.

I would only say this - and honestly I only read it once and I don't have it anymore so bear with me - I think you underrepresent the possible conditions that a) some researchers who have not submitted to your evaluative process may have more significant results and b) the cmns effects could just very well be extremely difficult to create and observe and if so, may only appear clearly in the primary researcher's lab as a result of these complexities.

I would say that you might be better off in the long run emphasizing that it is still to early to tell. If you are going to bother to publish a paper with 'negative results' and you are not intending to appear hostile, you may want to express some of these possible conditions I just stated. .....I'd say that too many people have wasted their time trying 'prove' that "cold fusion" is real and too many people have wasted other's time trying to demand proof, which they often vaguely refer to as extraordinary evidence. "Proof" for one person is not for another. 'Proof' is a personal thing."

Replying to the above comments, Scott Little wrote: "After consideration of the few responses to my statement, I have decided to revise the objectionable paragraph in an attempt to reduce the tendency of my words to appear as complete and utter nonsense to the reader. The new version says:

'Despite this rejection a number of scientists continue to investigate cold fusion. Hundreds of papers reporting positive results have been published and international conferences are held every couple of years. However, to this day, there exists no cold fusion demonstration experiment. That is because the cold fusion phenomenon is extremely difficult to reproduce. There is no experimental recipe that will yield positive excess heat results even 50% of the time. This situation greatly hampers cold fusion research and, in the eyes of some, supports the possibility that the cold fusion phenomenon does not actually exist.' Is this better?"

Steven Krivit wrote: "Alright Scott, Here you go. I'm gonna lay into you - nothing personal, okay, but you've asked for critique. You're not an expert in cold fusion, you're not a journalist and you're not a representative of this research community so you're on very thin ice with a few of your statements.

This sentence is a problem: 'However, to this day, there exists no cold fusion demonstration experiment.' You know just as well as I that if there were a demonstration experiment that were publicly known, it would have been on the

front page of the NYT -yesterday. You're asking for trouble with it and I know you better than to think you are someone who is being intentionally provocative. Why don't you drop the sentence? I don't see what it gains for you except to display unfavorable characteristics.

This sentence is correct and helpful: 'the cold fusion phenomenon is extremely difficult to reproduce.'

This sentence is a problem: 'There is no experimental recipe that will yield positive excess heat results even 50% of the time.' You cannot speak from a position of absolute authority - nor can I. It is erroneous and offensive if you would consider doing so. I presume you are just not aware of the sensitivities of text and editing. You could make this change to it and have a completely better effect: 'ETI is not aware of an experimental recipe that will yield positive excess heat results on a regular basis.'

This sentence is a problem: 'This situation greatly hampers cold fusion research and, in the eyes of some, supports the possibility that the cold fusion phenomenon does not actually exist.' This sentence is shallow and biased. First of all, the fact that it is difficult to repeat and replicate does not hamper the research, it hampers only the commercial viability of the work. The fact that it's a difficult science problem means, drum roll please, *it's a difficult science problem*. Period. . . .

What hampers cold fusion research is **1)** the fact that the claims are so astounding, people have difficulty considering it and its proponents seriously **2)** an institutionalized set of myths http://www.newenergytimes.com/PR/CFMythsFacts.htm **3)** a lack of significant funding http://newenergytimes.com/PR/FusionAdvantages.htm **4)** a stigma that not only affects non-CMNS researchers but also those in the field. (For example, a researcher with the Naval Research Laboratory, was not permitted by his commanding officer to use the words "cold fusion" in his forthcoming paper, accepted in the *Journal of Surface & Coatings Technology*.) This is the real cold fusion problem: http://newenergytimes.com/Reports/ColdFusionProblem.htm .

Responding to someone else Jed Rothwell wrote: "This is true, but there are any number of indisputable experiments (not demonstrations). The Iwamura experiment is indisputable, 100% reproducible, and it has been independently replicated and independently confirmed by examining the isotopes. But it cannot be run by anyone in front of a crowd of people at a conference.

It should be noted that most scientific experiments and industrial processes cannot be demonstrated. You cannot demonstrate a Tokamak plasma fusion reactor, a steel mill or a new computer chip fabrication machine, but nobody claims these things do not exist. I think this demand for a demonstration to prove the effect is real is absurd, and a demonstration would not convince a single one of the pathological skeptics. Even though a demonstration would add nothing to believability or scientific validity, it would still be valuable, for other reasons:

As Storms pointed out, easy reproducibility would be a huge advantage because it would allow more research, and it would be a step toward commercialization. A demonstration would convince many people who are not pathological skeptics. These people are not scientists and they do not understand the ABCs of the scientific method. If they did, they would be convinced already. However, it would be great to win their support. A demonstration might also convince investors to fund research. Some of these investors are convinced that cold fusion is real, but they do not think it has near-term profit potential. (I agree with them.)."

Responding to someone else Scott wrote: "I appreciate your thoughts. Yes, the title could have been better, but it is not nonsensical. The paper is a report on my efforts in testing energy claims and all of those tests have turned out negative. . i.e. null. Here's the abstract for the paper (remember the audience is a bunch of aerospace types):

'Null tests of breakthrough energy claims

Mankind desperately needs a better source of energy both for space travel and for terrestrial uses. This need spawns invention and claims of new energy devices abound. A number of such devices have been evaluated, with a singular lack of success. Interesting case histories are presented with the goal of promoting a better

understanding of the problems encountered in the evaluation of energy devices.'

Much of the report deals with our efforts to extract energy from the zero-point field. But I feel that our efforts in cold fusion also deserve mention, mainly because cold fusion has such enormous potential value to mankind. I'm not saying cold fusion is bogus but I want the audience to realize where it really is today and to appreciate some of the difficulties in the way of progress in the field."

Replying to another message from Scott, Jed wrote: ". . . As you have often done in the past, you are putting your own personal experience and your own personal skills above the whole history of science and technology, and above the textbook basis of the scientific method, and about the expertise of thousands of professionals. You were saying that because *you personally* have often made experimental errors, or seen other people make them, you think it is plausible that 500 to 1000 professional scientists worldwide over a 17 years have made error after error in observing high sigma data. You think that the people at BARC, who are world-class experts in detecting tritium, may have been wrong, even though they used three different instruments types and made hundreds of autoradiographs. For some utterly inexplicable reason there was no tritium. Every major method of detecting tritium developed over the last 100 years was repeated by three different divisions within BARC. Tritium was detected at thousands of times over background. You can see that for yourself: the autoradiograph was black. Yet it was all a mistake! All of these methods failed utterly, and nobody noticed. The safety division experts failed, even thought their very lives depend upon detecting tritium at much lower concentrations, and even though they have devoted their careers to dealing with tritium. You give no reason -- but you think they are wrong. Plus you really, honestly, seriously think that Fritz Will, Bockris, and Storms and 100 others were also wrong about the tritium they detected.

And you base this assertion entirely on your own personal experience! That is not a defensible point of view. Frankly I do not know what to make of it. It appears to be blind self-assurance based on overweening chutzpah. It is not rational. It is not falsifiable. It is not a scientific point of view by any standard. No textbook or philosophy of science treatise gives you even the slightest grounds for advocating this bizarre notion that hundreds of replicated experiments might be wrong. ....."

In another message Scott wrote: " My apologies to the list owners. Thanks for the constructive criticism. Please resume productive usage of this list." And here is the last message from Jed: "Let us be clear about this. In this exchange Scott Little represents the voice of the self-willed, self-satisfied, and self-deluded -- people like Close and Huizenga who betrayed science, and those who never understood it in the first place. They are the outcasts. We are the mainstream. They trash tradition and ignore precedent. We uphold tradition. They offer no falsifiable argument and they make assertions without evidence, while we publish irrefutable autoradiographs and data from hundreds of experiments. They are the perpetual motion machine fanatics, not us.

Little has made important contributions to cold fusion, and he is smart and capable, but in this case he fails to understand the bedrock principle of the scientific method: that *replicated experiments are the only standard of truth*. No one can second guess autoradiographs and instrument readings. No one can dispute them or outvote them, or claim on the basis of his own experience and gut feelings that they may be wrong. If you allow subjective personal experience to overrule facts, you open to the door to chaos. No question will be settled conclusively.

By every standard and by all the traditions of science, we stand on the inside and they are outside. Because the times are out of joint, and because the barbarians at *Nature* have temporarily taken over mainstream science, people do not see it that way. But Schwinger, Fleischmann, Gerisher and the others are right, and *Nature* is wrong."

And my last comment on this thread was extracted from a message on global warming. That message appeared on Phys-L discussion list for physics teachers.

"As every lawyer knows, it is always possible to make an argument on any side of any proposition. As many physicists know, not all such arguments are credible."

Earlier in the thread I wrote that a simple Mizuno-type cell (Glow Discharge Plasma Electrolysis) seem to be the best candidate for a demo. Somebody also made this suggestion. To which I responded by showing this:

List of successful replications of Mizino-type excess heat:

1) J.L. Naudin see his website --> http://jlnlabs.imars.com/cfr/
2) V. D. Cirillo et al. see their ICCF11 report
3) J.F. Fauvarque et al. see their ICCF12 report
4) R. Slaughter et al. Colorado2 experiment (we are working on the report)
5) J.F. Fauvarque et al. -- Paris2 (preliminary results after Colorado2)
6) University Labs in Japan

Experiments in preparation or in progress:
7) R.Slaugther -- Colorado3 (preliminary observations of excess heat)
8) J.P. Biberian -- Marselles1
9) J.F. Fauvarque et al. + another lab at CNAM -- Paris3
10) S. Little -- Texas2

What about your lab? I am not going to list names; many researchers on this list are highly qualified to replicate Mizuno-type experiments. And some are likely to have what is needed. Working on nearly identical experiments, and sharing results from our non-patent-motivated work, we are likely to promote the CMNS field faster than working on separate experiments and keeping things secret. Naturally, one must be aware of possible dangers, as described earlier on this thread.

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298) Nuclear alchemy aspect of CMNS

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Introduction:

It is not the first time that I am describing a report claiming that nuclear reaction products are produced by certain chemical reactions. T. Mizuno, a Japanese electrochemist published a book on that subject several years ago (1). It is unfortunate that, due to his medical condition, I missed a chance of working with Mizuno after the last cold fusion conference in Japan. But I was lucky to work with another scientist, J. Kasagi. Based on that experience, and on other observations, I can say that Japanese scientists are highly qualified and extremely careful about what they publish. Yes, I know that a generalization from one example might be wrong. But it is better than generalization from zero examples. In any case, the purpose of this piece is to review a paper coauthored by Mizuno (2); that paper will be published in the ICCF12 proceedings.

What attracted my attention to this report was a plasma-electrolysis cell that operated continuously for 15 days; the cell we used in Colorado2 experiments (see items #271 and #270) operated for less than half hour. After that time our tungsten cathode would erode. The cathode and anode used in (2) were made from Pd (whose melting point is considerably lower than that of W) and they did not erode during 15 days of operation. After that the electrodes were examined and chemical elements, presumably created during the electrolysis, were found on the cathode. The potential difference between electrodes was approximatively 50 V. This is considerably lower than voltages at which excess heat was measured during plasma electrolysis. The low voltage reminded me of another paper (3) devoted to plasma electrolysis. The authors of that paper cooperated with E. Stroms and with J. Rothwell. But their contribution to CMNS phenomena (excess heat, excess hydrogen and excess elements) was minimal.

Experimental Setup:

The volume of the teflon cell used by Abe et al. (2) was only 300 cc, as illustrated in Figure 6 of their paper. It had a lid with small hole. The hole prevented accumulation of hydrogen and oxygen; it was also used to periodically add water (to keep the volume of the electrolyte constant). The electrodes and the thermocouples were mounted on the lid. The electrolyte was the 1M solution of K_2CO_3 in distilled water. Its nearly constant volume was 200 cc. Palladium rods inserted into the cell had the diameter of 1 mm. They were partially coated with teflon. The distance between the electrodes, according to Figure 6, was about 6 cm. Extreme precautions were made to keep the cell components as clean as possible.

Main results:

The surfaces of electrodes, after 15 days of operation, were examined with a scanning electron microscope (to observe structural details) and with energy dispersive X-ray analyzer (EDX to identify elements). Crater-like features, found on the cathode but not on the anode are considered to be evidence of nuclear reactions. The energy spectra of scattered X rays (Figure 7) show peaks due to well known elements. These peaks were not present when electrodes were examined before the electrolysis. That is an indication that they were produced during the electrolysis. Furthermore, some elements, such as Cu, Zn and Mg, were found on both electrodes while others, such as Fe, Ti, and Cr, were found on the cathode only. The height of the iron peak, for the cathode, is nearly three times above the background. This can be contrasted with the height of the same peak for the anode. The iron peak (if any) for the anode is nearly negligible in comparison with the background.

Peak appearing on the cathode only are identified by bold letters (in Figure 7) while peaks appearing on both electrodes are identified by italic letters. The authors conclude: "If [the identified elements] were transmutation products, at least two processes may explain their existence. The first [process is a] nuclear reaction presumably occurred at the cathode and only produced the bold letter elements, while the second reaction occurred at both electrodes, and produced the italic letter elements. Since we have not analyzed the samples by other methods, we cannot determine the origin of these elements yet." The "yet" implies that work is in progress; I hope the isotopic composition of iron will be reported at the next cold fusion conference (ICCF13). Will that composition be the same as that reported by Karabut (see item #13) or will it be different? That remains to be seen.

Unfortunately, peaks in Figure 7 are quantified in terms of arbitrary units (counts). It would be more useful to identify them in terms of absolute units, such as numbers of atoms per square centimeter, averaged over the entire surface. As far as know nobody was able to demonstrate that excess heat produced in Mizuno-type cells is commensurate with the number atoms produced via nuclear reactions. By the word "commensurable" I mean at least 0.01 MeV of excess heat per atom. Any other evidence that excess heat is due to nuclear reactions is much less convincing.

Plasma protocols:
The issue of "the right kind of plasma" was mentioned in the unit #271. Mizuno told me, at ICCF12, that the current must be very low and that it should be decreasing with voltage. Figure 2 of the paper is a plot of the voltage and current versus time, for about 3 hours. During the first 9000 seconds the applied potential increases nearly linearly (in small steps). The rate is about 2.5 volts per minute. The current also grows, up to about 2.5 A. After that the current goes down when the voltage goes up. After about one hour the potential difference is about 120 V while the current is about 0.6 A. The current becomes ~ 0.4 A when the potential becomes ~300 V.

The above description refers to a cell whose electrolyte was 0.2M K_2CO_3 and whose cathode was a tungsten wire of 1.5 mm diameter. The anode geometry is not mentioned. Assuming the cathode was cylindrical one might say that the cell was not very different from the cell used in Colorado2 experiments. The Colorado2 cathode, however, was about two times thicker. I suppose that this was the reason for which our current at 300 V was several times higher than 0.4 A. The "right plasma region" corresponds to currents below 1 A (when potentials are between 60 and 300 V).

The term "right plasma" means right for experiments in which excess heat is generated. That region is characterized by rapid deterioration of cathodes. That kind of operation, however is not appropriate for accumulation of transmutation products. To study such products one must operate the cell under ". . . another, milder type of electrolysis, which does not severely damage or disintegrates the electrode. . . " In Figure 4 that operation region is near the point at which the current is maximum (2.5 A at 45 V). The cell geometry to which the Figure 4 refers is not identified, it seems to refer to the cell with the tungsten cathode and with the 0.2 M electrolyte.

References:
1) Tadahiko Mizuno, "Nuclear Transmutations: The Reality of Cold Fusion;" Infinite Energy Press, 1998.

2) "Elemental Analysis of palladium electrodes after Pd/Pd light water critical electrolysis;" by Yutoriy Abe, Tadahiko Mizuno, Tadayoshi Ohmori and Yoshiaki Aoki. This report can be downloaded from the library at www.lenr-canr.org.

3) N. A. Reiter and S.P. Faile. Their unpublished report can be downloaded from www.geocities.com/spfaile/plasma/Plasma.html

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299) Nuclear or Exotic-Chemical Reactions?

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Introduction:

This morning someone e-mailed me two audio files containing a recent interview with Randy Mills. Randy was a cold fusion researcher. Then he developed a theory according to which excess heat, usually attributed to cold fusion of atomic nuclei, is supposed to be due to chemical reactions involving unusual hydrogen atoms. These atoms were named "hydrinos." A company, BlackLight. Inc., has been created by Dr. Mills in New Jersey. The mission of that company is to conduct hydrino research and to promote hydrino technology.

The 20-minutes interview, conducted by the energy analyst and consultant, Alan M. Lammey, took place on May 6, 2006. (***Energy Week,*** FM Radio News Channel 97.5 Houston, TX). The files sent to me can be downloaded from (www.FmNewsChannel975.com) AND (www.HoustonEnergyAnalyst.com)

Unfortunately, the focus was on speculations about possible future applications of BlackLight technologies, and on social issues of acceptance. I would prefer more emphasis on experimentally confirmed facts and on their significance. The files were e-mailed to me by John Neergaard. Some of his messages on the CMNS list made me aware that he is well qualified to describe scientific findings of Mills. Therefore I asked him for an essay on that topic.

After seeing his rough draft (several hours later) I replied: "My general comment is that the essay is too theoretical (which does not mean too mathematical). What experimental fact are being explained by his theory? How well are these facts established? My impression was that hydrogen states with negative 'excitation[' energies, described by fractional quantum numbers, play the central role in Mills' explanations. Am I wrong? I will let you to decide how well your description matches the essence of Mills' findings. I know practically nothing about his experimental work. The size of the essay is about right and I will post it in the form you send it to me next time. Thanks for being prompt."

John modified his essay and sent me the new version. It is better organized and less theoretical, as you can see below.

Randall Mills as an Innovative Chemist

John Neergaard (BS ChE, PhD Ph)

Introduction

The principles of Randal Mills experiments can be understood by considering a vacuum containing a wire into which system is injected H_2. Voltage is then applied to the wire to carry current. During the time when each atom in the wire is missing an electron, it is an ion with a stronger electric field than the atom. The heat from the wire disassociates the

H_2 near it. The hydrogen is then attracted to the ions at the surface of the wire. One then lets the experiment progress for an adequate time, turns the current off, examines the system, and finds absolutely no change. Did anything happen? There is no end product to examine. Each element has returned to its original state.

Chemistry reactions are analyzed in terns of an end product. If H_2 reacts with O_2, then H_2O can be found and measured to determine how much H_2 and O_2 reacted. The water is the end product, also known as chemical ash. Randall Mills, predominantly, works with ashless chemistry, simply studying behavior of H in the presence a catalyst ION. What is observed before he turns the current off, before the catalyst ion returns to its atomic state, coming from a location away from the wire, coming from the location where H leaving the wire would mix with H not exposed to the wire, is previously unknown light and heat in significant excess of the energy supplied to the wire. All this returns to its original form after the current is removed, albeit it does persist for a time without the catalyst. There is no permanent ash. (There are minimum ion states for success.)

When he forms a reaction product from a similar circumstance, by injecting a third element into a system similar to this, the end product, the ash, is completely new, never before seen in a chemistry lab. The hydrogen in this ash has properties never before seen in the lab. Other than in these synthesis conditions, the fuel, hydrogen or the catalyst ion, is not consumed.

Randall Mills claims that the energy from this process can be 1000 x the typical energy of normal hydrogen reactions. Since the per event energy from such is still similar to chemistry reactions, tens of eV (or perhaps this x10 in some cases), one expects that this includes development such as driving the process, accelerating the reaction process, perhaps by returning the catalyst to neutrality, freeing the hydrogen more quickly than would be expected under dc conditions in the above wire.

One can imagine the rejection which chemistry would normally experience for such claims. Even now, knowing what I know, I am reluctant to discuss the radical claims of Randall Mills. Unless one runs such experiments for himself, even though the Mills lab is open for inspection of experiments and results, only illuminata can be expected. The Novelties? There are three new properties for this odd hydrogen.

1. Previously unknown photons are found.

2. The new H can generate a self starting plasma with previously unknown heat and light when interacting with normal H,

3. The new H appears to gain the ability to bond twice.

The Experimental Facts

www.blacklightpower.com/theory/theorypapers/PhysicalSolutions032306.pdf and other.

Unknown photons

Experiments 108-112 in http://www.blacklightpower.com/Abstracts.pdf describe methods used to find the new photons. They are quite real and can be observed in any lab which runs the experiments using many different techniques. They occur when the, let's call it 'new H', when the new H makes contact with H. These photons, usually (there are H energies which do not), follow an inverse Bohr model. Bohr photon energies are proportional to 1/n. The Mills photon energies are proportional to 1/1/n = n. They are said to be 2 x 13.6eV, 3 x 13.6eV, etc. There are photons which do follow this model into the ultraviolet range, not consistent with, and opposite to, normal atomic photon emissions. The photons in this energy scheme are real. There is something profoundly new involved in this emission circumstance.

Self starting plasma

Look at http://www.iop.org/EJ/abstract/0963-0252/12/3/312 . This abstract describes the consequences of heating a wire coated with K_2CO_3 in the presence of hydrogen. It is one reference of the 'capable of generating its own plasma' remark in the audio file interview. The iR heat of the wire ionizes the K atom in K_2CO_3. Subsequently, there is a glow

apart from the wire. The photon emissions are arising from the interaction of H, after it has bonded with the K ion and departed, with 'normal' hydrogen separated from the wire. There is no known explanation of this property within existing text hydrogen literature. Thus, this odd property exists and is available for viewing reproducibly on demand in his lab or any lab which runs the experiment. It is a reasonably simple experiment to run for those who have vacuum equipment which allows current carrying wire access. The temperature for this K ionization is ~ 600 - 700C.

Hot Hydrogen
This is as close to isolation of the beastie as Mills has, to the best of my knowledge. He ionizes inert gases in the presence of H_2 . Hyrogen is detected with excess energy, as 'hot', with unexplained energy throughout the system. It's a spread out variation of the clustered plasma above. The experiment was run to exclude various objections to the conversion of H being the source of the energy.

Extreme Ultraviolet Spectroscopy of He - Hydrogen plasma
See in http://www.iop.org/EJ/abstract/0022-3727/36/13/316

New Compounds
Mills statement with respect to the odd H compounds is that ' the H appears to gain the ability to bond twice'. The compounds include various compounds of the KHI form and apparently an inorganic mer group. The compound KHI illustrates the point. This compound can be synthesized in any appropriate lab although the introduction timing of the Iodine is not specifically given. The K is ionized at a given T in the presence of H, apparently followed by the introduction of Iodine. This is mostly an efficiency of reaction concern. In principle the H must bond with K (ION) before I dominates the K. These compounds are illustrated by 'Synthesis and Characterization of Lithium

Chloro Hydride', #44
Look at http://www.blacklightpower.com/pdf/REFERENCES%20042006.pdf
Such compounds exist on the shelf in his lab. Many other reproducible on demand experiments for these properties exist. http://www.blacklightpower.com/techpapers.shtml

A Fundamental Question of Theory

[L.K. wrote]'My impression was that energy states with negative "excitation" energies, described by fractional quantum numbers, play the central role in Mill's explanations. Am I wrong?' No. That's exactly what he uses. In order to account for the photons, the sequence, the photon energies, and their 'relative quantities', he assumes the new beast is a hydrogen atom, extends QM from principle quantum number n to 1/n. He does this by imposing boundary conditions on Maxwell's equation. He calls such a hydrogen atom a hydrino. However, he goes significantly beyond that with non proven ideas that just boggle the mind.

In philosophical terms the math of the theory is nothing more than a shorthand method of remembering the emission scheme. It contains no new epistemologically certain knowledge. This is also a criticism of QM. Further criticism exists. Further, the theory does not address the implied charge on the H in compounds such as KHI. The odd H charge has never been studied (in public) in isolation. The new photons have not been observed to arise from a true H = p + e atom. All the H did was contact a catalyst ion. What is the cause of any newness? If it is in a shrunken orbital, why does contact with an ion do that? Or what is it that contact with an apropos ion really does?

For example, the implied charge on the new H can imply new H = p + e+. This hypothesis leads into cold nuclear fusion, in part because two of the three above properties, and possibly the third, are present in the conditions of the excess producing Mizuno plasma. (Discussed elsewhere on this site to produce some transmutation.) Isolating the new H for a simple charge check would distinguish and might attract huge interest depending on the result.

In short, the shrunken orbitals can be used to describe the photons but the premise / assumption has not been empirically verified, and does not explain KHI, inter alia. The new H empirically exists with the above properties with or without explanation. Thus one can employ the empirical properties for prediction to better use than the theory. That is, the conditions in which new chemistry arises in Mills' lab will produce new chemistry effects in other experiments also with or without a theory.

There are many other reasons for being wary of Mills as a theorist /physicist. (On the physics side, he does produce uses for Maxwell's equation under his terms which have not been used before.) He is a very legitimate pioneer in the chemistry lab, a future chemistry Nobel prize winner, assuming he is not ostracized forever for producing 'impossible' results, as the cold nuclear fusion community also presently is.

P.S.
What are the odds that two such anathema experimental circumstances, Mills and CF, are unrelated? The experiments of Mills and those of cold fusion relate to the original disagreement between Bohr and Einstein regarding (Einstein's phrase) the 'missing causal substratum'. (Bohr's rules are actually independent of v, meaning c can substitute for it with no change other than mental interpretation.) At the time, and to this day, Physics had no ultimate causal understanding of charge behavior in the bond between p and e, a particle, charge, unified field theory deficiency. (Thus, Physics must either choose Bohr and Schroedinger or stand in complete public atomic theoretical ignorance for nearly a century. They chose B & S. The inability of Mills experiments and cold fusion experiments to be explained by existing theoretical material exposes this ultimate deficiency from the past.)

Appended on 6/12/06:

After seeing the above Mike Carrell wrote: "There are a number of serious problems here. First, Mills is not, and never has been, a "cold fusion" researcher. He has been at great pains to distance himself from the cold fusion world since his earliest work. There is an early patent which alludes to cold fusion, and a number of people who look at the Mills and CF worlds have tried to find links and associations which are not made by Mills, inplying that Mills' results are "really" CF resultes.

Neergaard has tried to 'explain' Mills' reactions and reactor setups in a 'chemical' interpretation and creates unnecessary confusion. He does correctly state that very unusual phenomena occur in the Mills reactors, and the contextual theoretical edifice is audacious in the extreme.

Mills has recently been very active in applying his theory to the calculation of the structure of conventional chemical compounds. His method gives accurate results using simple closed-form equations. I understand that this aspect of his work has attracted intense interest from some quarters, although it does not relate to energy production or the shrunken 'hydrino' state of hydrogen resulting from the strongly exothermic catalytic reactions discovered by Mills. If Ludwik is interested, I can prepare a short essay on Mills & BlackLight Power for his website."

My reply was short: -- "I would be happy to append a short essay about the idea that excess heat in Mizuno-type experiments might be the same thing as excess heat measured by Mills. Arguments for or against this idea would be equally welcome." The first version of Mike's essay came at once. Carrell wrote: I will here respond to X comments and provide a compact statement of Mills' Blacklight Power (BLP) reactions, based an years of close observation of his published work and public statements." I posted that version. Another version was received next day. What follows is that second version. Mike prefers it to be a set of comments inserted into John's essay. John's essay is in black and Mike's comments are in blue. Yes, this created some repetitions. But that is OK with me.

Introduction

The principles of Randal Mills experiments can be understood by considering a vacuum containing a wire into which system is injected H_2. Voltage is then applied to the wire to carry current. During the time when each atom in the wire is missing an electron, it is an ion with a stronger electric field than the atom. The heat from the wire disassociates the H_2 near it. The hydrogen is then attracted to the ions at the surface of the wire. One then lets the experiment progress for an adequate time, turns the current off, examines the system, and finds absolutely no change. Did anything happen? There is no end product to examine. Each element has returned to its original state.

Only in the thermally driven experimental cells is a heated wire in a hydrogen atmosphere a starting point. Hot tungsten wire functions as a dissociator to split H_2 molecules from a supply source into two H atoms which can participate in the BlackLight Power (BLP) reactions. The proper starting point for understanding is an isolated H atom

in a gas at about 1 Torr with catalyst ions also present.

The statement that there is no end product to examine is false. An end product, shrunken H atoms, called hydrinos, is produced, detectable by signature lines in the UV emission spectra from the reactor, and by mass spectrometry of gases from certain specially designed experiments.

Chemistry reactions are analyzed in terns of an end product. If H_2 reacts with O_2, then H_2O can be found and measured to determine how much H_2 and O_2 reacted. The water is the end product, also known as chemical ash. Randall Mills, predominantly, works with ashless chemistry, simply studying behavior of H in the presence a catalyst ION. What is observed before he turns the current off, before the catalyst ion returns to its atomic state, coming from a location away from the wire, coming from the location where H leaving the wire would mix with H not exposed to the wire, is previously unknown light and heat in significant excess of the energy supplied to the wire. All this returns to its original form after the current is removed, albeit it does persist for a time without the catalyst. There is no permanent ash. (There are minimum ion states for success.)

It is not true that Mills works with "ashless" chemistry, if "ash" is understood to be the tangible end product of a chemical reaction. The 'resonant transfer' reaction between an H atom and a catalyst ion changes the H atom to a lower energy state, called a hydrino. Hydrinos have been detected by diffusion through a thin walled cathode in an electrolytic cell into a vacuum system coupled to a mass spectrometer. With gas-phase cells, the exit gases when condensed in a liquid nitrogen trap contain atoms chemically identifiable as hydrogen, but with other abnormal characteristics identifiable as hydrinos.

When he forms a reaction product from a similar circumstance, by injecting a third element into a system similar to this, the end product, the ash, is completely new, never before seen in a chemistry lab. The hydrogen in this ash has properties never before seen in the lab. Other than in these synthesis conditions, the fuel, hydrogen or the catalyst ion, is not consumed.

The abstracts cited below include experiments in which hydrino hydride compounds are formed with other elements. These exist in macroscopic quantities and have been shown in photographs [I have seen these in Mills' conference room]. The statement that otherwise the hydrogen fuel is not consumed is not correct. H atoms are transformed to the hydrino state and are so "consumed" as H but conserved as hydrinos. The proton and electron count remains unchanged.

Randall Mills claims that the energy from this process can be 1000 x the typical energy of normal hydrogen reactions. Since the per event energy from such is still similar to chemistry reactions, tens of eV (or perhaps this x10 in some cases), one expects that this includes development such as driving the process, accelerating the reaction process, perhaps by returning the catalyst to neutrality, freeing the hydrogen more quickly than would be expected under dc conditions in the above wire.

The phrase "in the above wire" indicates a serious misconception of the BLP reactions. Neergaard appears to still be thinking of BLP reactions like CF reactions occurring in the solid state. They do not. Preferentially, the gas state is used, at about 1 Torr. Examination of the hydrogen alpha line in a typical BLP reactor shows H atom energies in the tens of eV, equivalent to temperatures in the range of 100,000 K. As I have stated elsewhere, water bath calorimetry of a gas-phase reaction between H and He+ [as catalyst] shows 'excess heat' energy release 100 X that of combustion. Other observed reactions indicate still higher ratios, but these have not yet been demonstrated in calorimeters.

One can imagine the rejection which chemistry would normally experience for such claims. Even now, knowing what I know, I am reluctant to discuss the radical claims of Randall Mills. Unless one runs such experiments for himself, even though the Mills lab is open for inspection of experiments and results, only illuminata can be expected. The Novelties? There are three new properties for this odd hydrogen.

1. Previously unknown photons are found.

I am not sure what is meant by 'unknown photons', unless he is referring to novel spectral lines seen from BLP reators.

2. The new H can generate a self starting plasma with previously unknown heat and light when interacting with normal H,

I am not sure what he refers to. In the thermal reactor a high energy plasma is produced when the right conditions are present, where normal chemistry would not produce a plasma at all. It is not self-starting, it has to be heated, along with the hot tungsten dissociator.

3. The new H appears to gain the ability to bond twice.

Again, I am not sure what is meant. Hydrinos can gain an electron and become hydrides. Hydrinos can form diatomic molecules as well.

The Experimental Facts

www.blacklightpower.com/theory/theory **papers/PhysicalSolutions032306.pdf and other.**

Unknown photons

Experiments 108-112 in http://www.blacklightpower.com/Abstracts.pdf describe methods used to find the new photons. They are quite real and can be observed in any lab which runs the experiments using many different techniques. They occur when the, let's call it 'new H', when the new H makes contact with H. These photons, usually (there are H energies which do not), follow an inverse Bohr model. Bohr photon energies are proportional to 1/n. The Mills photon energies are proportional to 1/1/n = n. They are said to be 2 x 13.6eV, 3 x 13.6eV, etc. There are photons which do follow this model into the ultraviolet range, not consistent with, and opposite to, normal atomic photon emissions. The photons in this energy scheme are real. There is something profoundly new involved in this emission circumstance.

This is quite correct. I am accustomed to thinking in terms of emission lines, but the photon terminology is also correct.

Self starting plasma

Look at http://www.iop.org/EJ/abstract/0963-0252/12/3/312 . This abstract describes the consequences of heating a wire coated with K_2CO_3 in the presence of hydrogen. It is one reference of the 'capable of generating its own plasma' remark in the audio file interview. The iR heat of the wire ionizes the K atom in K_2CO_3 . Subsequently, there is a glow apart from the wire. The photon emissions are arising from the interaction of H, after it has bonded with the K ion and departed, with 'normal' hydrogen separated from the wire. There is no known explanation of this property within existing text hydrogen literature. Thus, this odd property exists and is available for viewing reproducibly on demand in his lab or any lab which runs the experiment. It is a reasonably simple experiment to run for those who have vacuum equipment which allows current carrying wire access. The temperature for this K ionization is ~ 600 - 700C.

This is the Conrads paper, an important independent confirmation of the BLP thermal reactor. Neergaard does not have the experimental structure quite right. Hot tungsten dissociates H_2 to 2H. K_2CO_3 crystals are coated on a titanium sleeve, which when heated dissociates the K_2CO_3 , releasing K+++ which is a BLP catalyst with the H atoms. Conrads substitutes halogen lamps for the bare tungsten wire, and the plasma does not appear until the lamps are wrapped with tungsten wire, which when heated dissociates the H_2 to 2H. He removes the titanium sleeve and no plasma. He substitute Na2CO3, and no plasma; Na is not a catalyst.

Hot Hydrogen

This is as close to isolation of the beastie as Mills has, to the best of my knowledge. He ionizes inert gases in the presence of H_2. Hydrogen is detected with excess energy, as 'hot', with unexplained energy throughout the system. It's a spread out variation of the clustered plasma above. The experiment was run to exclude various objections to the conversion of H being the source of the energy.

The input to the reactor is He, Ar, plus H_2, or simply H_2O. Microwave energy ionizes the gases except H, with He+, Ar+ and O++ becoming catalysts. Critics have cited field acceleration as the source of 'hot' hydrogen observed in glow discharge cells; the microwave cells do not have high field acceleration regions.

Extreme Ultraviolet Spectroscopy of He - Hydrogen plasma
See in http://www.iop.org/EJ/abstract/0022-3727/36/13/316

New Compounds
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Such compounds exist on the shelf in his lab. Many other reproducible on demand experiments for these properties exist. http://www.blacklightpower.com/techpapers.shtml

A Fundamental Question of Theory

[L.K. wrote]'My impression was that energy states with negative "excitation" energies, described by fractional quantum numbers, play the central role in Mill's explanations. Am I wrong?' No. That's exactly what he uses. In order to account for the photons, the sequence, the photon energies, and their 'relative quantities', he assumes the new beast is a hydrogen atom, extends QM from principle quantum number n to 1/n. He does this by imposing boundary conditions on Maxwell's equation. He calls such a hydrogen atom a hydrino. However, he goes significantly beyond that with non proven ideas that just boggle the mind.

The whole theoretical edifice is indeed mind-boggling. It is important to separate the body of experimental evidence from the novel structure of the electron and the rest of Mills theory. Hydrinos have physical reality, the chemistry of hydrinos with other elements is largely unexplored. The high energy produced by the BLP reactions is real. Mills appears to be better positioned to move ahead into commercial development of energy sources than the CF world.

In philosophical terms the math of the theory is nothing more than a shorthand method of remembering the emission scheme. It contains no new epistemologically certain knowledge. This is also a criticism of QM. Further criticism exists. Further, the theory does not address the implied charge on the H in compounds such as KHI. The odd H charge has never been studied (in public) in isolation. The new photons have not been observed to arise from a true H = p + e atom. All the H did was contact a catalyst ion. What is the cause of any newness? If it is in a shrunken orbital, why does contact with an ion do that? Or what is it that contact with an apropos ion really does?

Criticism of Mills' theoretical edifice will continue. What happens in the "resonant transfer" reaction between the H atom and catalyst is not clear and may be the object of years of study. Mills points out that a non-radiative energy transfer between atoms is known in certain phosphors, for example. This is truly new territory, as in a way what happens in a CF reaction is also new territory. As Ludwik states, the perfect must not be the enemy of the good. So if parts of Mills work appears paradoxical, this should be regarded as an invitation to deeper understanding.

For example, the implied charge on the new H can imply new H = p + e+. This hypothesis leads into cold nuclear fusion, in part because two of the three above properties, and possibly the third, are present in the conditions of the excess producing Mizuno plasma. (Discussed elsewhere on this site to produce some transmutation.) Isolating the new H for a simple charge check would distinguish and might attract huge interest depending on the result.

My understanding is that hydrinos are electrically neutral, as are normal H atoms, but that they can weakly bond and extra electron, forming hydrides, which can then form chemical compounds with unusual properties. There is no

thread leading to fusion reactions.

In short, the shrunken orbitals can be used to describe the photons but the premise / assumption has not been empirically verified, and does not explain KHI, inter alia. The new H empirically exists with the above properties with or without explanation. Thus one can employ the empirical properties for prediction to better use than the theory. That is, the conditions in which new chemistry arises in Mills' lab will produce new chemistry effects in other experiments also with or without a theory.

Indeed, with or without a theory. Theories are useful in predicting what to try next.

There are many other reasons for being wary of Mills as a theorist /physicist. (On the physics side, he does produce uses for Maxwell's equation under his terms which have not been used before.) He is a very legitimate pioneer in the chemistry lab, a future chemistry Nobel prize winner, assuming he is not ostracized forever for producing 'impossible' results, as the cold nuclear fusion community also presently is.

In all fairness to John, I must acknowledge his generally positive report on Mills' work and recognition of its seminal nature. I had reservations, based on a quick first reading. There are some errors and terminology I am not accustomed to, but the shape and thrust of his essay is in the right direction.

P.S.

What are the odds that two such anathema experimental circumstances, Mills and CF, are unrelated? The experiments of Mills and those of cold fusion relate to the original disagreement between Bohr and Einstein regarding (Einstein's phrase) the 'missing causal substratum'. (Bohr's rules are actually independent of v, meaning c can substitute for it with no change other than mental interpretation.) At the time, and to this day, Physics had no ultimate causal understanding of charge behavior in the bond between p and e, a particle, charge, unified field theory deficiency. (Thus, Physics must either choose Bohr and Schroedinger or stand in complete public atomic theoretical ignorance for nearly a century. They chose B & S. The inability of Mills experiments and cold fusion experiments to be explained by existing theoretical material exposes this ultimate deficiency from the past.)

My take on all this is that the BLP world and the CF world are two aspects of a new physics to painfully emerge. At some point they may be seen as complimentary, but it may be wise not to force the issue; rather, pursue both to their depths and see what is found. Mills claims to have found a theory consistent over 85 orders of magnitude, including an estimate of the period of an oscillatory universe. Mike Carrell

This website contains other cold fusion items.
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300) Preliminary Colorado2 report

Ludwik Kowalski; 6/28/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

As some of the readers know, about one year ago I decided to devote some time and energy to a relatively simple experiment whose purpose was to either confirm or to refute reality of excess heat in Mizuno-type cells (*1). That decision was influenced by two authors who had already confirmed reality of such heat -- Eugene Mallove (*2) and Jean-Lois Naudin (*3). Their descriptions of experiments made me think that a Mizuno-type cell is an ideal device for low-budget student-oriented investigations possible in many physics labs. As described in the unit #252, I was directly influenced by a report sent to me by Pierre Clauzon from the Laboratoire d'Electrochimie Industrielle at Conservatoire National des Arts et Métiers, Paris (4*). He assured me, in a private message, that a possibility of chemical origin of excess heat, in Mizuno-type cells, has been investigated -- and declared impossible -- by a French chemist. That was a very important point; I am not qualified to deal with such issues.

In unit #252 I wrote: ". . . On the other hand, I anticipate problems with water condensation on the walls of the beaker, and with possible splashing, above 200 W. Each of these two effects, if not accounted for, can contribute to an apparent excess heat." Condensation turned out to be tolerable because water condensing on the inner walls of the beaker releases as much heat per gram as absorbed to create steam in the first place. Significant loss of water resulting from splashing was subsequently eliminated with drop reflectors. Units #253, 255, 256, 258 259 and 260, on this website, were composed before I had a chance to see the Mizuno-type cell in action. That was the cell built by Scott Little, in Austin, Texas. Some of the things I wrote were not clearly expressed. But that reflected the evolution of my thinking.

Work in Austin (Texas1 experiment) and subsequent work with Richard Slaughter in Boulder (Colorado1 experiment) did not reveal any excess heat. The setup used in this experiment was described in the unit #261. The unit recognizes a very important suggestion made by Dennis Cravens -- a possibility that excess heat is an artifact due to the presence of invisible droplets of electrolyte escaping with bubbles of oxygen and hydrogen. I wish this possibility were taken by us more seriously and that some kind of a test were invented to determine the percentage of the electrolyte in the lost liquid. The suggestion was ignored and we simply assumed that the escaping liquid was nothing but dry steam. Preoccupation with tiny droplets became an issue many months later, when Colorado2 results were discussed at the restricted internet list for CMNS researchers. But I am jumping ahead of this little overview

Negative findings, contradicting the conclusion of (*4) were reported at the 12th International Cold Fusion Conference in Japan (*5). Positive findings, confirming the conclusion of (*4), during the next sequence of experiments in Slaughter's lab., were reported over the Internet several weeks later (*6). They were discussed extensively at this list. To avoid confusion the following labels were introduced to identify recent and future Mizuno-type experiments:

Paris1 (*4) French work done before the last conference in Japan
Colorado1 (*) Work in Little's lab before the last conference in Japan
Colorado2 (*) Work in Saughter's lab after the last conference in Japan
Paris2 (*) A sequence of titration tests on the electrolyte used in Colorado2
Paris3 An anticipated new CNAM project in France
Colorado3 An anticipated new project in Slaughter's lab.

Marseilles1 An anticipated new French project in Biberian's lab

After analyzing Colorado2 data we decided to write a paper about our findings. I said that I will try to produce the first draft. The authors would be Richard Slaughter, Pierre Paul Clauzon, Jean-Francois Fauvarque, Ludwik Kowalski, Gerard Jean-Michel Lalleve, and Scott Little. The idea was to focus on experimental results, rather than on possible interpretations. The only electrochemist among us, Professor Fauvarque, would write an appendix showing that the excess heat measured was orders of magnitude larger than what could possibly be contributed to chemical reactions. What follows, after the list of references below, is the beginning of my draft. Do not confuse references with asterisks, which are for this unit, with references for our paper. The paper references are included because our introduction refers to them.

***References:**

*1) Ohmori and Mizuno "Strong Excess Energy Evolution, New Element Production, and Electromagnetic Wave And/Or Neutron Emission in the Light Water Electrolysis with a Tungsten Cathode." presented at ICCF7,1997.

*2) E. Mallove in http://www.amasci.com/weird/anode.txt

*3) Jean-Lois Naudin's web site - "CFR project, a High Temperature Plasma Electrolysis based on the Tadahiko Mizuno work from the Hokkaido University (Japan)" http://jlnlabs.imars.com/cfr/

*4) Jean-Francois Fauvarque, Pierre Paul Clauzon and Gerard Jean Michelle Lalleve. *"Abnormal excess heat observed during Mizuno-type experiments;"* 2005. The report can be downloaded from the library at <http://www.lenr-canr.org>

*5) L. Kowalski et al. November, 2006: "New results and an ongoing excess heat controversy." The report will be published in the proceedings of the 12th International Conference on Cold Fusion.

*6) The URL is http://groups.google.com/group/cmns/ The name of the list is CMNS; Its archive, I believe, is not restricted to the list subscribers.

Is the Unexplained Heat Released During High-Voltage Electrolysis Truly Anomalous?

Abstract:

Reality of unexplained excess heat in glow discharge plasma electrolysis, first described by Ohmori and Mizuno (1), has been confirmed in a sequence of forty one tests. The results are described in terms of the coefficient of performance, COP, the ratio of the thermal energy released in a cell over the electric energy supplied to it, during the same time. The mean COP, and the standard deviation, turned out to be 1.24 and 0.13, respectively, as illustrated in Figure 1. The 24% of excess heat, in our setup, corresponded to about 30 kJ of thermal energy in five minutes (100 W). We refer to excess heat as unexplained because we were not able to attribute it to known chemical reactions, as discussed in Appendix 2. The author of that appendix is a French electrochemist, Dr. Jean-Fracois Fauvarque, from the Laboratoire d'Electrochimie Industriel, CNAM, Paris.

Figure 1 Distribution of COPs resulting from 41 tests performed at several differences of potential between 300 and 350 volts. No significant effect of the voltage on the COP was observed in that region.

****

Introduction:

The controversy surrounding excess heat is part of a broader controversy about the so-called “cold fusion.” The broader controversy started to evolve in 1989 after two electrochemists, M. Fleischmann and S. Pons discovered a strange energy releasing process (2). Their hypothesis, actually a guess, was that the unexplained heat was due to an exothermic nuclear process. Such processes are usually associated with emission of neutrons, and with production of radioactive nuclei. The hypothesis was rejected when it became clear that neither radioactive nuclei nor neutrons were produced in amounts commensurate with excess heat. If the unexplained heat, confirmed in many subsequent experiments, is real then it cannot possibly be due to well known nuclear reactions. Fleischmann and Pons discovered excess heat in conventional electrolytic cells. Several years later reality of unexplained thermal energy was discovered in high-voltage electrolytic cells (1), and in some non-electrolytic processes, for example, when deuterium gas diffuses through palladium (3). Numerous experiments devoted to excess heat are described in (4). Critical reviews of early cold fusion studies can be found in (5,6).

This paper focuses on measurements of excess heat similar to those initially described by Ohmori and Mizuno. Other aspects of high-voltage electrolysis, such as investigation of products of nuclear reactions (7) and abnormal generation of hydrogen (8) were outside the scope of our investigation. In their first high-voltage electrolysis experiment Ohmori and Mizuno reported that “the electrode became incandescent.“ Their COP was 2.6 and their excess energy was generated at the rate of 183 W.

Excess heat generation was confirmed in several Japanese experiments, as described in (20, 21 and 8). In Europe reality of excess heat was confirmed in experiments performed by: J.L. Naudin (9), by D. Cirillo et al. (10), and by J.F. Fauvarque et al.(11). All these studies confirmed reality of excess heat. Scott Little (12), on the other hand, was not able to confirm reality of excess heat in a Mizuno-type experiment. Negative findings were also published by L. Kowalski et al. (13). Their negative report, presented at the conference, was discussed by several participants, including Mizuno. The discussion revealed that plasma in (12) and (13) was considerably different from that in (11). The investigation described here was undertaken to resolve the controversy between (11) and (13). The main conclusion of (11) -- the over unity of COP -- has been confirmed, as illustrated in Figure 1.

Continuation of my draft is below the list of references.

References:

1) Ohmori and Mizuno "Strong Excess Energy Evolution, New Element Production, and Electromagnetic Wave And/Or Neutron Emission in the Light Water Electrolysis with a Tungsten Cathode." presented at ICCF7,1997.

2) Fleischmann, M., S. Pons, and M. Hawkins, *Electrochemically induced nuclear fusion of deuterium.* J. Electroanal. Chem., 1989. **261**: p. 301 and errata in Vol. 263.
3) Li, X.Z. *et al.* (2003) "Correlation between abnormal deuterium flux and heat flow in a D/Pd system." *J. Phys. D: Appl. Phys.,* **36,** 3095

4) C. G. Beaudette, *Excess Heat. Why Cold Fusion Research Prevailed*. 2000, Concord, NH: Oak Grove Press (Infinite Energy, Distributor).

5) J.R. Huizenga, "*Cold Fusion: The Scientific Fiasco of the Century*". second ed. 1993, New York: Oxford University Press

6) E. Mallove, "*Fire from ice", 1991, NY: John Wiley, pp. 246-248*

7) T. Mizuno and Y. Toriyabe, *Proceedings of ICCF12* (2005), Eleventh International Conference on Condensed Matter Nuclear Science. 2004. Marseilles, France. The report can be downloaded from the library at www.lenr-canr.org

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9) Jean-Lois Naudin's web site - “CFR project, a High Temperature Plasma Electrolysis based on the Tadahiko Mizuno work from the Hokkaido University (Japan)” http://jlnlabs.imars.com/cfr/

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Experimental setup:
High-voltage electrolysis, also known as GDPE (glow discharge plasma electrolysis) takes place when the electric field near the cathode is sufficiently high to ionize bubbles of hydrogen and oxygen created during the electrolysis (14,15). As in (11), our tests were performed in an open cell with two electrodes, a tungsten cathode and a platinized titanium anode. The applied differences of potential were in the range between 300 and 350 volts while the currents were in the range between 2 and 3 amperes. Our cell had cylindrical geometry, as illustrated in Figure 2.

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Figure 2 Simplified schematic diagram of the open cell. Holders on which the electrodes and the thermometer were mounted are not shown. Also not shown are the ohmic heater, inserted into the electrolyte, and buffers designed to eliminate consequences of boiling splashes. The tungsten rod was surrounded by a ceramic tube (dotted pattern) whose

purpose was to confine the glow discharge to the lower part of the cathode.
****

The electrolyte was the 0.2 M potassium carbonate, K_2CO_3 in distilled water. The 0.02 M concentration was also tried; the effects were not noticeably different from those observed at 0.2 M. Likewise no noticeable differences were observed when the diameter of our cylindrical anode was changed from 4.4 cm (platinized Ti) and 8 cm (platinized Nb). Our tungsten cathodes were standard welding rods, purchased in a welding supply store. Two kinds of rods were tried: with 2% of thorium and without thorium. No significant difference between them was observed. The diameter of the cathode, on the other hand, had an effect on the voltage at which the plasma column was formed; thinner cathodes ignited at lower voltages than thicker cathodes. This observation is consistent with what has been reported in (16).

Excess heat was generated when the electrolyte was boiling and when the lower end of the cathode (below the ceramic tube) was surrounded by a deep orange column of glowing vapor. Our protocol was similar to that described by Naudin (17) and Mallove (18). The voltage was increased to the desired value slowly, for example, to 300 V in one to several minutes. During that time the current increased and then dropped down progressively, for example, to 2 A. It is reasonable to explain this in terms of the resistance of the plasma column surrounding the cathode. The current starts to decrease when the glow discharge is ignited. This can be interpreted by assuming that the column of the plasma becomes thicker at higher voltages and that its resistance increases with the thickness. A typical experiment lasts several minutes, as in (11). Depending on the voltage, one can perform 5 to 10 consecutive tests before the tungsten cathode is eroded.

Measuring electric energy:
A DC power supply was used as the source of electric energy. A set of two electrolytic capacitors (3100 mF and 450 V each) was connected in parallel with the cell. That was sufficient to keep the voltage constant, as verified with an oscilloscope. The electric current, however, turned out to fluctuate rapidly due to the random nature of the glow discharge. Electric energy Ee, supplied to the setup during a test, was initially measured by the method of fast samplings. This was accomplished by using a computer-based data acquisition system. The rate of sampling was 100 per second. The value of Ee was then calculated as the sum of v(t)*i(t)*dt, where the dt=0.01 seconds is a time between sampling while the v(t) and i(t) are instantaneous values of the voltage and amperage.

The second method of measuring the electric energy consisted of reading the Ee from a digital kWh-meter, U-390 (U stands for Unigore). That commercial instrument was designed to deal with sinusoidal and non-sinusoidal inputs. As expected, the discrepancy between the two methods were less than two percents. Independent digital voltmeter and digital ammeter were also used to visually estimate the mean values of v(t) and i(t) during consecutive minutes. Visual averaging is less reliable than fast electronic averaging but discrepancies between the last method and the first two were usually less than several percents. Unfortunately, our computer-based system became disabled due to an accidental short. For that reason about 80% of measurements of Ee were made with the digital kWh-meter. We believe that these measurements of Ee were accurate to within two percents. That confidence is based on the random nature of plasma current fluctuations. Fluctuations that are not random can lead, under certain conditions, to significant systematic errors, as illustrated in Appendix-1.

According to technical specifications the U-390 is reliable even when the current is non-sinusoidal. Our thermal calibration of the instrument confirmed that claim. The calibration was performed by connecting the power supply in series with an electrolytic cell (beaker #1) and with an ohmic heater of about 100 ohms, immersed in the beaker #2. That second beaker contained boiling water. The only function of the electrolytic cell, in the calibration experiment, was to modulate the electric current randomly. The electric energy, Ee, delivered to the heating resistor in about 20 minutes, was measure with the U-390 kWh-meter. The result was compared with thermal energy, Et, released in the beaker at the same time. The two energies turned out to differ by only 0.43%. Measurements of thermal energy are described in the next section.

Such result would be impossible if measurements of Ee were associated with large systematic errors. It is important to emphasize that the current flowing through the ohmic resistor, during the thermal calibration of U-390, was essentially of the same non-sinusoidal kind as the current flowing through the electrolytic cell when the COPs were measured. Two independent calibrations of our kWh-meter (by measuring thermal energy and by using the fast computer-based

data acquisition setup) confirmed that the instrument is indeed reliable, even when the current is not sinusoidal.

An additional confirmation of the reliability of the U-390 instrument came from the comparison of its readings with the kWh received by the power supply at the a.c. side, as measured with another kWh instrument (DMMetering, model DRM75A). The two energies, measured at the same time, differed by ~ 3%. That difference was more or less consistent with the amount of heat dissipated inside of our simple home-made power supply. In order to bring the mean COP to unity (from 1.24) the true Ee would have to exceed the U-390 readings by 24%. This would conflict with the kWh measured at the a.c side of the rectifier.

Measuring thermal energy:
Thermal energy, Et, released in an open cell during a test, consists of two parts: Ev, used to evaporate water, and Ec, escaping through conduction, convection and radiation. The Ev during the electrolysis was calculated from the evaporated mass, m; the Ev is simply L*m, where L is the latent heat of evaporation, 2260 J/g.
The same approach was used to find the Ev in the tests with the ohmic heater, for example, in the beaker #2, during the calibration of the U-390 kWh meter.
The non-evaporative losses of thermal energy, Ec, were calculated from the Ee and Ev measured when boiling was due the ohmic heater, that is when Ec=Ee-Ev. The heater was connected to the 110 volts a.c. line. Electric energy received by the heater was measured with two kWh-meters: U-390 and a much less sophisticated ??? (designed for sinusoidal currents only). No significant difference between the readings of these two kWh-meters was observed.

The cell shown in Figure 3 was standing on a digital electronic scale. In that way the mass, m, lost through evaporation, during each test, or during calibrations, could be measured with the accuracy better than 1%. If m=10 grams in t=60s then the thermal evaporative power is $Pv=E_v/t=377$ W. Switching from energies to powers is convenient because duration's of experiments are not identical. In what follows the Pe=Ee/t will be called electric power and the Pc=Ec/t will be called the non-evaporative thermal power. The coefficient of performance is

$$COP = (Ev+Ec)/Ee = (Pv+Pc)/Pe$$

Note that under identical boiling and mixing conditions the Pc during ohmic heating is expected to be essentially the same as the Pc during plasma electrolysis. For our two-liter beaker (our electrolytic cell) the values of Pc turned out to change linearly from about 50W to 80W when the Pe changed from 300W to 850W. The values of Pc were measured before and after the electrolysis experiments.

Experimental results
In a typical GDPE experiment, lasting 300 s, the Pe and Pc were close to 300 W and 50 W, respectively. About 40 grams of water was lost during such experiment. That gives Ev=90400 J, Pv= 301 W and COP=1.17. The values of COP fluctuated slightly from one experiment to another. The mean COP, and the standard deviation, from our forty one tests, turned out to be 1.24 and 0.13, respectively, as illustrated in Figure 1.
Experiments were performed at potentials between 300 V and 350 V. Contrary to what was reported in (11) the voltage had no significant effect on the COP.

Discussion
The results were discussed on the restricted Internet list for the CMNS researchers. The acronym stands for Condense Matter Nuclear Science -- it is a new name for what used to be called cold fusion. In our opinion the word “Nuclear” is premature. As far as we know, no one was able to show that the rate of accumulation of nuclear reaction products, in Mizuno-type cells, is close to one atom per several MeV, of excess energy, as in many well known nuclear reactions. Additional experiments will be necessary to clarify a mechanism, or mechanisms, through which excess heat is produced. The only statement we can make, at this time, is that well known chemical reactions cannot possibly be responsible for the excess heat measured. That topic is discussed in the Appendix 2, written by a French electrochemist, Jean Francois Fauvarque.

To illustrate difficulties encountered in trying to attribute execs heat to chemical fuels let us consider tungsten. In one experiment, lasting ten minute, the measured excess heat was 60 kJ (at the rate of 100 W). During that time about 1.3

grams of tungsten was lost by the cathode (diameter 2.4 mm). According to (1) the loss is due to melting; it cannot be due to heat-producing oxidation because negative ions containing oxygen are repelled by the cathode, as explained in the Appendix 2. But suppose that one ignores this fact, as done in (1), and assumes that all excess heat comes from formation of H_2WO_4. It is easy to show that if 60 kJ of thermal energy is due to consumption of 1.3 grams tungsten then each atom contributes 88 eV of the measured excess. In our experiments about 50% of tungsten removed from the cathode was recovered as metallic particles at the bottom of the cell. That means that the number of reacting atoms of tungsten was one half of the 1.3 grams.

Taking this experimental fact under consideration the rate of generation of energy becomes 176 eV per atom. This is 44 times larger than what is expected -- oxidation of one mole of tungsten, with subsequent formation of H_2WO_4, generates 220.84 + 179 = 380 kJ of heat. In other word, attributing our measured excess heat to the oxidation of tungsten would conflict with what is known about the $2W+3O_2=2WO_3$ and $WO_3+H_2O= H_2WO_4$.reactions. This has already been emphasized by Mizuno et al. (1). The only possible exothermic reaction, according to Appendix 2, is decomposition of water. That reaction contributes only 4.3% to the excess heat measured. To exclude its contribution the reported value of the COP, 1.24, must be replaced by 1.21. Other possible reactions, discussed in the Appendix 2, are endothermic and their combined contributions to the COP was estimated as -5%. In other words, the net effect of well known reactions, on the value of the COP, is essentially negligible.

Can the over unity of the COP be due to an erroneous assumption that the Pc measured during the electrolysis was the same that the Pc measured when the current was flowing through the ohmic heater? Suppose that a layer of foam is formed on the surface during the electrolysis but not when the current flows through the ohmic heater immersed into the electrolyte. In that case, as recognized by Michel Julian, convectional losses used to calculate the COP would be exaggerated leading to an overestimation of the COP. We have two reasons to rule out this possibility. The first reason is that no significant differences between surfaces was observed during our experiments. The second reason is based on a control experiment performed to investigate this issue. In that control experiment the electrolyte was in dewar rather than in a much wider beaker. This reduced the Pc but the values of COP was not significantly lower than 1.24.

Can the over unity of the COP be due to an error in the mass m used to calculate thermal losses. We assumption that the liquid lost during an experiment was nothing but pure vapor. But suppose that this was not correct. Suppose that 25% of the lost mass, m, consisted of escaping droplets. In that case our Pv, calculated from the 2260*m, would be strongly exaggerated. An exaggerated Pv, in turn, would create an illusion that COP is larger than unity. Emission of visible drops has often been observed during our experiments. The mass of visible drops was estimated on the basis their volumes. In some cases a paper towel was used to collect drops and to measure their combined masses. The typical estimated mass was less than one gram per test. That was much smaller than the total lost mass m, typically 40 grams per test. On that basis the effect of visible drops on the COP was estimated as less than 2.5%.

But what about the much smaller, invisible droplets? That issue was not addressed during the experiments. Most of such invisible droplets are likely to evaporate before being intercepted by the paper towel. The effect of tiny droplets, however, was assessed after experiments. ..... [WORK IN PROGRESS]

Can excess heat measured be attributed to some kind of energy accumulation process? To answer this question we considered a possibility that our cell acted as a rechargeable electric battery. In a control test the cell, after being connected to the power supply for several hours, was suddenly disconnected and discharged through a resistor. [WORK IN PROGRESS] Another possibility considered was short-time storages and releases of thermal energy. That hypothesis was ruled out the basis of high reproducibility. The mean COP of 1.24, and the standard deviation of 0.13, from 41 tests, would not be possible if such mechanism played a significant role. Furthermore, significant random thermal accumulations and releases would be associated with significant temperature fluctuations during consecutive tests. The actually observed temperature fluctuations, about one degree or so, probably contributed to the size of the standard deviation.

Conclusion:
The overall conclusion is that the GDPE thermal-excess energy, discovered in (1), and confirmed by other investigators (9, 10 and 11), seems to be real. Generation of excess heat, at the 0.1 kW level, turned out to be

reproducible. The origin of that energy is not clear to us. Can several cells be used in a battery generating excess heat at a much higher rate? Not having an accepted theory one must rely on empirical investigations. Systematic studies of effects of various parameters (size, composition, and geometry), on the performance of Mizuno-type cells, seem to be warranted.

Can excess heat be attributed to a well known nuclear reaction, such as D-D fusion? The answer to this question must be negative. Familiar nuclear reactions generate radioactive byproducts. A thermonuclear D-D reaction, for example, generating excess heat at the rate of 0.1 kW, for three hours would produce about $4*10^{19}$ neutrons. That would be sufficient to create radioactive isotopes in many objects near the cell. A Geiger counter we used, however, showed that the level of radioactivity, near the cell, was not higher than natural background. The time of three hours, by the way, is used because that was not very different from the total duration of our 41 experiments. Speculations about the origin of excess heat are less important, at this stage, than establishing its existence.

Appendix1: Measuring electric energy by sampling.
The electric energy E, delivered to a cell, is the integral (over a test duration) of dE = i(t)*v(t)*dt, where i(t) and v(t) are instantaneous values of the current and potential, respectively, while dt is a very short time interval. The glow discharge current is known to fluctuate randomly. This was confirmed by using an oscilloscope. Fluctuations of i(t) were not associated with significant fluctuations of v(t) because a large electrolytic capacitor (0.001F and 500 V) was connected between the cell electrodes. It fact, the v(t) was essentially constant during each test. As previously indicated, digitized samples of v(t) and i(t) were recorded by the data acquisition board controlled by a computer.

Suppose that the sampling frequency is much lower than some frequencies contributing to the shape of the current waveform. Can this lead to a systematic error in a measured value of the electric energy delivered to the cell? The answer is negative, unless the current fluctuations are not random. To justify this answer consider a current waveform shown in Figure 3. It has been generated by using random numbers (from a uniform distribution between 0 and 2). The time intervals between consecutive current changes were 0.1 ms, as indicated. It is clear that the spectral composition of that waveform contains frequencies higher than 10 kHz. The mean current, over a long period of time, is 1 A, by design.

Suppose that the shape of this waveform must be determined by sampling. The rate of sampling would have to be considerably higher than 10 kHz , for example, at least two or three measurements during every 0.1 ms, depending on the desired accuracy. The dark circles show the waveform one would obtain if the sampling rate were only 2 kHz. Connecting the dark points with straight segments one gets a shape that is quite different from the real shape of the waveform. But the situation becomes dramatically different when the purpose of sampling is to determine the mean current over a long time interval, for example between 0 an 20 seconds. In that case the mean value from the two waveforms would be practically identical. This fact was numerically verified by using a computer code. The six mean currents, obtained by executing the code six times (six different waveforms) were: 1.000, 0.999, 0.993, 1.001, 0.992 and 1.004 A.

In other words, the sampling frequency necessary to determine the mean current can be much lower than what would be necessary to determine the shape of the signal. The only requirement, to obtain a reliable mean value, is to gather a sufficiently large number of unbiased samples during an experiment. This conclusion is intuitively obvious for any random waveform. The situation, however, can be different for a periodic waveform, as illustrated in Figure 4. In that example the sampling interval -- 1 ms -- is the same as the period of repetition. The apparent mean current would thus be 0.2 A; that is several times smaller than the true mean current.

Note that the apparent mean current of 0.2 A would occur only at sampling frequencies of 1 kHz, 0.5 kHz, 0.25 kHz, 0.125 kHz, etc. At all other sampling frequencies the apparent and the true means would be nearly identical, provided the sampling interval is negligibly small in comparison with the duration of the waveform. In other words, an error due

to a poor choice of sampling frequency, for a periodic waveform, is possible but not very likely. To eliminate possible systematic errors periodic waveforms must be sampled at randomly distributed intervals (rather than at equal intervals, as in Figures 3 and 4). Using consecutive sampling at two different frequencies, such as 13 kHz and 17 kHz, and showing that the means are nearly identical, would be a good indication that a sampling error did not occur.

Someone who also built a Mizuno-type cell examined the v(t) waveform with a 20 MHz digital oscilloscope. The examination revealed presence of pulses whose duration's were several microseconds. These were said to be similar to the well known Trichel pulses in corona discharge (19). Is it possible that presence of such pulses may lead to an underestimation of the Ee, and consequently, an overestimation of the COP? To answer this question, in the context of sampling, one would have to analyze what happens when a sample is collected. We simply assumed that an instantaneous value of voltage, captured during several milliseconds, is already an average based on what happens at the level of microseconds, nanoseconds and picoseconds. It is unlikely that ultra-fast pulses would lead to nearly identical systematic errors in different kinds of kWh meters used in this investigation. Same observation can be made about possible effects of electromagnetic interference.

Appendix 2:
Contributions of possible chemical reactions to excess heat.
Appendix is going to be sent to me, probably in the middle of July. I hope it will provide ammunition to argue against chemical origin of excess heat, and against presence of the electrolyte in the escaping steam. Can a battery-like effect be responsible for generation of excess heat at the rate of about 0.1 kW? I hope that this question will also be answered in the appendix written by the author, an electrochemistry professor in Paris.

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301) Colorado2 results must be revised

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As described in unit #300, I have been involved, for about one year, in an experiment, named Colorado2. It was similar to several glow discharge plasma electrolysis (GDPE) experiments performed by other investigators in several countries (see references in unit #300). The purpose was to convince myself, and others, that Mizuno-type excess heat is not an illusion due to experimental errors, or to some well known effects. The conclusion we reached was that some unexplained source of energy was responsible for the excess heat measured. The draft of the manuscript was nearly ready to be shared over the Internet when one interpretational error was discovered. Unit #300 shows my draft; as it was about two weeks ago. The purpose of this unit is to follow new disturbing developments. I will assume that the reader is familiar with concepts and terminology introduced in unit #300; this unit is a continuation of what was written there.

A question about invisible droplets of electrolyte escaping from cell has been raised but not answered in unit #300. Two experiments were performed to answer that question, one based on titration of the electrolyte remaining in the cell, after an experiment, and another based on the examination of the wet steam escaping from the boiling electrolyte. The result of the first approach is not clear to me; I hope it will become clear after Appendix 2 is received. But the second approach is more simple. It was nothing more than a determination of exact masses of four samples. The first three samples, at least one gram each, contained drops of steam condensed on a cold surface, the fourth sample was the original electrolyte.

The method was introduced to us by Scott Little; here is his own description of it:
". . . I conducted a simple dissolved-solids test on the samples you [R.S.] sent. First I weighed 4 Al weighing cups. Then I dispensed some of each sample into each cups. I used a new disposable pipette for each sample to prevent contamination. Then I weighed the wet cups. Then I gently dried the samples in a 70C oven for 4 hours This gentle drying should leave the salt as K_2CO_3+1.5H_2O. I recorded the first set of dry weights. Then I returned the samples to the 70C oven for another 8 hours. I recorded a 2nd set of dry weights."

Scott's net masses, for the first three samples turned out to be: 2.06, 1.15 and 1.84 mg, per cubic centimeter of the condensed liquid. The mean and standard deviations are 1.68 and 0.47, respectively. For the sample #4 (the original electrolyte) the mass of the solid deposit was 27.4 mg per cc. What does this mean? Under ideal conditions (no electrolyte escaping with the steam) the net solid masses, from the first three samples would be zero. But the measured mass, 1.68, was 6.1% of 27.4. That means that the amount of the electrolyte, in the escaping liquid, was about 6%. The mean COP, determined when three samples were collected, was 1.15. But that was calculated by assuming that the escaping liquid is pure water. In other words, our mass m, and Pv were overestimated by 6%. This leads a sizable correction, close to 0.05, in the value of COP. The correction would be 0.06 is the Pc were zero. More specifically, the mean COP=1.15 becomes 1.10. That is too close to unity, considering the standard deviation calculated above.

If this preliminary observation of Scott is confirmed then experimental COPs, reported in unit #300, are certainly exaggerated. A simple reduction of the reported results by 0.05 would not be appropriate because percentages of the

electrolyte in the escaping liquid were not measured. Conditions under which experiments were performed to collect three samples were not the same as conditions under which the bulk of Colorado2 data were collected. We are certainly not ready to write a paper that would be difficult to reject.

What is surprising is that researchers who performed similar experiments before us never mentioned, or tested, as far as one can say, a possibility of a systematic error in m due to the escaping electrolyte. They all fell into the same trap as we. How can this be explained?

Appended on July 5, 2006

1) I am disturbed and disappointed. The following questions were asked, on June 29, at our CMNS list: "Is Mizuno a member of this list? Is he aware of new development, Jed? [I know that Jed, who if fluent in Japanese, if a friend of Mizuno] If not you should inform him by phone. Naudin and Iorio are also on this list. I expected all three of them to be busy performing Little-type tests at this time." Four days later, I wrote that absence of messages from them is very disturbing. Two more days passed and we do not hear from them. How can this be explained. How can authors of influential papers remain silent after learning about Little's results? Having the necessary hardware I would simply condense the escaping liquid and measure the percentage of the escaping electrolyte under two kinds of conditions: (a) boiling due to the ohmic heater and (b) boiling due to the current flowing through the electrolyte. Perhaps nearly pure steam comes out in the first (usual) case and steam mixed with electrolyte comes in the second (not very usual) case. That would be much easier to do than measuring the COP.

2) It is only a matter of time before the mystery of "tiny droplets" is solved. I know, from private messages, that the Little-type test is now being conducted in Boulder by Richard Slaughter. Similar tests are planned during the upcoming Paris3 experiment, probably in a month or two. But what about the second main issue? Would I be able to say, referring to the appendix, that "known chemical reactions cannot possibly be responsible for the main part of excess heat? The first draft of the appendix, written by Jean Francois Fauvarque, did not allow me to say so. But some progress in that direction was made yesterday. It came after I posted the following problem on the restricted list for CMNS researchers.

"A cell contains one liter of the 0.2 M K2CO3 electrolyte. The cathode is tungsten and the anode is platinum. The temperature is already 100 C. How much heat is generated when a constant potential of 30 V is applied for 100 seconds? Assume that a constant current of two amperes is flowing through the cell. Part of the answer is obvious, the thermal energy is 30*2*100 + X = 6,000 + X joules, where X is the energy released (positive or negative) via anticipated chemical reactions. What is the value of X?"

Note that 30 V was chosen to avoid complications associated with the glow discharge plasma electrolysis (GDPE) that is ignited above 100 V. The outside temperature of 100 C was chosen to make sure that the cell temperature remains constant. The idea was to establish the value of X in a simple situation before trying to establish it for a much more complicated situation. Two Ph.D. chemists on the CMNS discussion list wrote that the dominant reactions, are: decomposition (electrolysis) of water and production of CO2 from the K2CO3. Not being a chemist I am happy to take this for granted.

3) I will return to this topic a little later below. My impression was that contributions of chemical reactions to excess heat from Fleischmann-type cells have been discussed extensively in papers written by Fleischmann and Pons. Statements that chemical heat was negligible are easy to find but not in F&P papers. The only one that I found today, after browsing through their papers (downloaded from the library at www.lenr-canr.org), is shown below. The title of the paper was "Calorimetry of the palladium-deuterium-heavy water system;" it was published in J. Electroanal. Chem, 1990.

". . . The total specific energy output during the bursts as well as the total specific energy output of fully charged electrodes subjected to prolonged polarization (5-50 MJ cm^3) is 100 - 1000 times larger than the enthalpy of reaction of chemical processes."

Two or three orders of magnitude is impressive. But specific energies are in J/cm^3. Did they divide the total excess heat by the total volume of the cathode or by the volume of a thin layer at the surface of the cathode? The same question can be asked about chemical processes. Are these all conceivable reactions in the cell or only reactions conceivable inside the cathode? It would certainly make no sense to compare MJ/cm ^{3} with MJ. Furthermore, the bursts to which the 1990 paper referred were of very short duration, as illustrated in several figures. Did they produce less ambiguous statements in later publications? I do not know. My search for a better reference will continue; the quote will be inserted here, if found.

4) It is well known that the electrolysis of water is an endothermic reaction. In other words it absorbs heat instead of releasing it. If the current is 2A, as in my illustration above, then the contribution of the electrolysis is -295 J. The negative sign is used to indicate the endothermic nature of the dominate reaction. And what about consumption of K2CO3, I asked. That reaction is also endothermic but the energy absorbed depends on how much of the potassium carbonate is consumed. For each gram of consumed salt the contribution to X is -82 J. Let me assume, to be specific, that only five grams of the K2CO3 is consumed. In that case the contribution of the two reactions to X becomes -295 - 5*82 = -705 J.

Yes, you might say, but what is the purpose of discussing a 30 V experiment when real experiments (Paris1 and Colorado2) were performed at 300 volts? That is certainly a valid concern. My two chemists wanted me to be aware that a large number of unknown reactions become possible under the GDPE. One of them wrote: “what happens at the anode for high voltage electrolysis- is interesting and important. And in great part, new- a place for discoveries.” My 30 V illustration is an instrument for thinking; call it a gedanken-ing tool. I will use it to subdividing all possible reactions, chemical and non-chemical, into two categories: (a) well known to chemists and (b) all others. I am aware that the term "well-known to chemists" is not very precise. But this term must be defined if one wants to make a claim that "the excess heat we measured cannot possibly be attributed to well known chemical reactions."

As indicated before, our Colorado2 report would not be worth publishing without a credible statement of that kind. Perhaps such statement will appear in the new version of the appendix written by Fauvarque. For the time being I am defining well known reactions as those for which the excess heat contribution (X in my illustration) can be estimated. Suppose that someone makes a claim that thermal energy released, during a 30 V experiment, exceeds the electric energy supplied by 4000 J. How large is the unexplained excess heat? My answer would be 4000 + 705 = 4705 J. The 705 J is the heat absorbed by the two well known reactions (for the current of 2 A and for 5 grams of K2CO3 consumed).

Note that 2A can be treated as a measured quantity. But 5 grams was only a guess. So what should the error bar be to account for the uncertainness in the guess? The smallest possible amount of K2CO3 lost was zero grams. In that case the contribution of the second reaction to X would be zero and the unexplained excess heat would be 4000 + 295 =4295 J. The other extreme (highly unlikely) would be that all the K2CO3 present, about 25 grams, was consumed to produce the CO2. In that case the value of X would be -(295+5*410) = -2345 and the unexplained heat would be 6345 J. The well known reactions underestimate excess heat because they are endothermic. Being able to say that excess heat is between 4295 and 6345 joules is due to unknown processes, chemical or non-chemical, would be a great step forward. Note that even under the lower limit the rate of generating excess heat would be close to 43 W.

What is wrong with that kind of speculation? It is based on my own definition of “well known reactions.” Other knowledgeable chemists might be able to extend the range of “well known reactions” by calculating their contribution to X. In that case the value of X would have to be modified. We cannot exclude a possibility of exothermic reactions under conditions of plasma electrolysis. Suppose that X turns out to be close to +4000 J. In that case we would say that the entire excess heat is due to well known chemical reactions. My main point is that excess, as measured in Colorado2 experiments, cannot be attributed to non-chemical reactions unless contributions of chemical reactions is ruled out by chemists.

A much more convincing approach would be to show that the amount of non-chemical byproducts is directly proportional the amount of excess heat generated. No experimental evidence of that nature exists, as far as I know, for Mizuno-type experiments. Presence of products of nuclear reactions has been discovered by Mizuno but the amounts were extremely low. Will these amounts be shown to be correlated with amounts of excess heat measured? That

remains to be seen.

5) I like coincidences. After composing the above I received this junk-mail message:

"A Genuine University Degree in 4-6 weeks! Have you ever thought that the only thing stopping you from a great job and better pay was a few letters behind you name? Well now you can get them!" Well I am certain that my chemical consultants did not graduate from the Genuine University. Only highly knowledgeable experts can estimate X from exotic chemical GDPE reactions.

Appended on July 7, 2006

Let me show extracts from some recent messages posted on the CMNS list. They show interactions among the CMNS investigators. How do these interactions differ from those in other areas of physical sciences? Keep in mind, however, that this is a multi-discipline list; backgrounds of contributors are very different.

Scott Little wrote:
. . . I agree that your scenario above does not violate the 1st Law and I distinctly recall challenging you that it was instead a violation of the 2nd Law. My reasoning was that the heat absorbed from the environment was, in effect, "free air-conditioning" (very valuable here in Texas) and that for a "price" of 1.23V*Q in input energy you were getting out of the cell 1.48*Q in output energy (in the form of H2 and O2 "fuel" gas).

But now that I have the accepted the concept that dG is the maximum amount of work a reaction can perform, it is beginning to look like there is no 2nd Law violation here at all. ..maybe. Can you suggest a reference that discusses these concepts, particularly thermoneutral potential, electrolysis of water, etc. in detail? All my Pchem (e.g. Moore) and thermo (e.g. Sears, Sonntag) books just seem to graze the surface and don't really discuss this very issue.

Mel Miles wrote (responding to a question about two electrolytes**):**
Fleischmann and Pons selected LiOD as their electrolyte to avoid the possibility of other cell reactions. If we assume that K2CO3 is oxidized to form CO2 and O2 at the anode and that water is reduced to form H2 at the cathode, then the cell reaction becomes

2 H2O + K2CO3 = H2 + CO2 + 0.5 O2 + 2 KOH

At standard conditions, Delta H =+395311 J for this reaction. Thus . . .

I wrote:
If I had to start another Mizuno-type experiment I would use fresh electrolyte for each test. Then the amount of the K2CO3 reacted would be measured for each test. Little-type test would be used on samples of the electrolyte. Suppose a sample of the electrolyte, extracted moments before a test, shows that each cm^3 contains 26 mg of the solid deposit (after 1 cm^3 of water is evaporated). Suppose the sample extracted after the experiment has only 22 mg of deposit per cubic centimeter. Then one would probably say that no more that 4 grams of the K2CO3 (from the total of 26 g initially added to one liter of distilled water) was consumed. The "no more" is appropriate because one cannot exclude a possibility that some tungsten, lost by the cathode during a test, is also present in the solid deposit. The amount of tungsten lost during a test would be measured by comparing the weight of the cathode before and after. If I worked with a chemist I would insist on measuring the amount of tungsten in the solid deposit and in the electrolyte. A complete chemical analysis of the sample would be preferable. But focusing on tungsten would be a big step forward. Knowing the result, a competent chemist would be in a much better position to estimate X due to formation of CO2, and probably due to other chemical reactions. I guess that was the essence of what Peter wrote two days ago.

And here is another suggestion, it was made by Pierre Clauzon, in a private message yesterday. Measure temperatures very accurately and do not start collecting excess heat data before thermal equilibrium has been established in the cell. He was probably thinking about power differences. Excess heat must be measured after thermal equilibrium is established inside the cell. The temperature must be constant and the rate of evaporation must be constant. Fortunately,

a fast electronic data acquisition system can be used to make sure that this precondition is met. That is how data were collected during Texas1 experiments. Colorado1, and Colorado2 data were collected without using the fast data acquisition system. But we usually waited several minutes before starting a new run. The rates of evaporation, from run to run fluctuated randomly. A systematic trend would probably be noticed but that is far from being certain. Recent Paris2 measurements were made under the thermal equilibrium. The results were more or less the same as in Paris1 and Colorado2 experiments. Is this correct, Pierre? As you can see, what I write is an extrapolation from what you mentioned yesterday. I am sure that what we learned from previous experiments will be taken under consideration in future experiments, such as Paris3 and Marseilles1.

Peter Gluck wrote:
This is not the essence of what I wrote 2 days and more days ago. The results of this experiment -- as described above cannot be interpreted univoqually -- what's more (less) they are not interpretable:

a) because a part of water is evaporated the concentration of solid salts are increased and because a part of K2CO3 is decomposed, the concentration of solid is increased. Which wins? Who knows? [actually, fresh water was added constantly to replace what was evaporated in Paris1. In Colorado2 we did this periodically, to keep the total volume more or less constant.]

b) only CO2 is lost via decomposition/decarbonation, K remains in the solution or is lost via droplets entrainment and it is not possible to distinguish between all these facts- how much of the residue is carbonate, how much is hydroxide;

c) tungsten gives heavy, bulky, granular precipitate that is absolutely non-uniformly distributed in the reaction vessel. It stays at the bottom. You will not find a representative quantity of W in one cubic cm of sample extracted aleatory from the solution.

What I am trying to tell is that the material balance has to be performed for the integral quantities of all the participants: water, K, CO2, W- before and after the test.

I wrote:
Thanks for good suggestions, Peter. The bottom line is that a well equipped analytical chemist must be part of a better team of researchers. Let me make another suggestion for Paris3 and Marseilles1 experiments. It is based on what Pierre and Richard wrote to me in private messages.

Ideally one wants to operate under thermal equilibrium, that is when the dm/dt (rate of evaporation) remain constant. The way to accomplish this, in an open beaker during the electrolysis, is to use two power supplies, one feeding the electrolysis and another feeding the ohmic heater, immersed in the electrolyte. I am assuming one can measure the electric energy delivered to the cell by each power supply. One would have to add these energies to calculate the COP.

Suppose the mean dm/dt is evaluated every 3 seconds and the values are plotted versus time when an experiment is in progress. To keep the dm/dt constant one simply changes the voltage feeding the ohmic heater. If the dm/dt starts going up the voltage is decreased; if the dm/dt starts going down the voltage is increased. I think that keeping the dm/dt constant, within 2 or 3% would not be too difficult.

The temperature-versus-time plot could be useful below the boiling point. Stirring of the electrolyte is probably worth having in all cases. I thing that the last container in Colorado2 was too big for the power of 300-400 W, especially without stirring. One can probably work without stirring, at such wattages, when the beaker is much smaller, for example, one liter. Under such conditions boiling is sufficiently uniform to get a constant temperature within the cell.

Peter Gluck wrote:
All I want is to contribute to the success of this important experiment and I don't like to be kind of negativist critic. But you wrote: "Ideally one wants to operate under thermal equilibrium..." I think this is both useless and impossible:

- useless - CF/CMNS is created by dynamic dis- or non-equilibrium conditions; and it is not good to sacrifice the intensity of the process for the sake of the precision of the measurement (unfortunately this happens in many

CF/CMNS experiments!!! and this is what I call, unpolitely enough, metrologomania- focus on measurement instead of intensification.)

- impossible due to the huge inherent temperature gradient- 3000 degrees Celsius in the plasma and maximum 100 degrees Celsius in the water, the released gases-hydrogen, oxygen, carbon dioxide at some unknown intermediary temperatures It is such a mess that you need a bunch of angels in order to make some equilibrium. There is no symmetry in this cells, with our without stirring. The unique way to kind of equilibrium -- dynamic is a flow-through cell a la Philip KANAREV but this is an other way; let's keep us in our limits.

REMARK+ - an shortcut, absolutely independent from our troubles with droplets is the examination is the separation and careful examination of the W based residue -- what the devil is it? And the suggestion of our colleague HENRI LEHN has to be treated with priority -- it shows immediately if some nuclear phenomena takes place in the system. [Henri's suggestion was to perform isotopic analysis if iron presumably produced from another element during the electrolysis. That would certainly be an interesting experiment. But our goal was to accomplish something much less spectacular.]

I wrote:
Our goal in Colorado2 was to either to confirm or to refute Paris1 results. And we confirmed them. We showed that by performing a similar experiment one gets similar results, more or less. That was important. But now we are addressing a different issue. How do we know that the excess heat measured was not an illusion due to a prosaic effect, such as well known chemical reactions? That issue has not been answered satisfactory. Will it be answered in the new version of the appendix to our anticipated paper? I hope so. If not then we must wait for results from better-designed experiments. Colorado3, Paris3 and Marseilles1 experiments will probably be designed to show that excess heat is not an illusion. Those who are planning these experiments are probably paying attention to various suggestions made on this list. Are Colorado2 results publishable? I tend to be more and more pessimistc about this. But suppose Little-type tests, or titration tests, or some other test based on a setup similar to that used in Paris1 and Colorado2 show that the percentage of droplets of the electrolyte in the liquid was about 10%. That would be the answer to our question. It would mean that the probability of an illusion due to a prosaic effect is very high. We would have to rewrite the paper and publish it.

Michel Jullian wrote:
Peter I think on the contrary that Pierre's idea of operating the cell at constant thermal power, ensuring a more constant temperature distribution, is excellent. It will not hamper short term plasma instabilities in the least, and it will make sure that no thermal capacitance effects will interfere. Such effects can be responsible for illusion of non-unity COPs, don't you agree?

The scheme will also automatically maintain the vapor barrier I proposed to prevent ingress of possible atmospheric fuels such as N2 (forgotten in recent discussions it seems): stopping the discharge e.g. to replace the eroded cathode will make the ohmic heater step in automatically.

P.S.
Here are some nitrogen and oxygen based compounds which could form in a GDPE cell, together with their formation enthalpies [the units are kJ per mole of the product.]. Some of the formation enthalpies are negative, which tells us that the corresponding compound's formation from N2(g) and O2(g) from air (null formation enthalpies) dissolving into the solution and being processed in the plasma would be exothermic.

N2O(g) 82.05
N2O4(g) 9.16
N2O5(c) -43.1
N2O5(g) 11.3
NO(g) 90.25
NO2(g) 33.18
NO3 -(aq) -205

As I said many times before, it would be presumptuous for anybody to state that no chemical reaction involving gases from air (including common impurities such as ethanol, benzene, silicones etc...) can be responsible for the excess heat in a GDPE cell and it's plasma furnace. Hence my idea to block atmospheric ingress by maintaining a vapor barrier (less dangerous than a lid).

Peter Gluck wrote:
. . . N2 is in some air solved in water at the start. But ingress of air from outside is not possible- it has to go counterstream to the gases released from the cell.

Mitch Swartz wrote:
First, carbonates cannot be removed from the experiments unless the experiment is not exposed to the atmosphere. They enter the water as CO2 in hours no matter how many times the water has been distilled. Second, including for reasons of which we spoke briefly, it might be more important to determine your input electrical power with some accuracy and precision, by careful measurement of V and I, faster than the Nyquist threshold, before focusing on the chemistry at this point. Once you obtain excess energy of sufficient magnitude, then optimizing the chemistry could then be done on an active working system.

I wrote:
1) Is it not true that the concentration of the CO2, entering the LiOH electrolyte, could be estimated by a chemist? I also expect reaction rates to be estimable. That would allow to anticipate chemical contributions to excess heat. My expectation was that this was done and that estimated contributions were found to be orders of magnitude below what was actually measured. But I am no longer certain that this was the case. Please provide references in which impossibility of chemical contributions to excess heat was convincingly ruled by either Fleischmann or Mizuno.

2) Do not assume that everyone on this list knows what the "Nyquist threshold" is. My recollection is that in order to overcome this threshold the sampling rate should be several times higher than the highest frequency of the signal. As I wrote in unit #300 (at my CF website) the frequency of sampling does not have to be very high, when fluctuations are random. The only thing that counts is a sufficiently large total number of samples. One thousand would probably produce sufficiently reliable mean values of V and I. Do you agree with this, Mitch?

Mitch Swartz wrote:
We sample applied voltage, and the voltage across both the cell and the control, and the electrical current, between 100 Hz and a lower limit of, at least, 1 Hertz, for at least 70,000 samples per day, with most runs lasting several days. For some experiments involving specialized studies the sampling rate is much higher.

I wrote:
1) That seems to be perfect when fluctuations of V and I are random. By the way, imposing some randomness on the sampling frequency would extend the reliability of electric measurements to situations in which fluctuations of I and V are not random. I believe that the expensive instrument used by Scott Little, in Texas1 experiments, had the “built in” randomness of sampling. He will probably correct me if I am wrong.

2) What is the overall situation with excess heat from Mizuno-type experiments? The claim is clear -- excess heat is believed to be due to something unknown. That claim is being challenged on this list. It appears that not sufficient amount of information is available to exclude prosaic effects. The number of prosaic effects is large. In principle one should deal with them one after another. That can take a long time. Showing that one or two trivial sources of excess heat are impossible does not guarantee that another trivial source is not going to be suggested by a skeptic. But even a single acceptable evidence against the claim is sufficient to negate it.

Suppose that Little-type tests are independently performed in several laboratories. Presumably, the outcome of each tests gives the percentage of the electrolyte in the steam escaping from the open cell. Knowing that percentage one can calculate a correction for the COP measured. Suppose that corrected COPs turn out to be close to 1.0, in several laboratories. That would definitely negate the initial claim. The only way to rescue the claim would be to show that experimental or logical errors were made in evaluations of COP corrections. In other words, we are in a situation in which confirmation of a claim is much more difficult than showing the the claim is not valid. Justifying our positive

(extraordinary origin of excess heat) is much more difficult than justifying our negative (prosaic origin of excess heat).

That reminds me of two kinds of reasoning: inductive (from more specific to more general) and deductive (from more general to more specific). Conclusions drawn on the basis of inductive reasoning are often questionable. Observing 1000 white swans, and concluding that all swans are white, would be an example of inductive reasoning. Deductive reasoning, like in mathematics, is said to be more convincing; its reliability depends only on the reliability of the already accepted propositions, and on the absence of derivational mistakes.

Sorry for a philosophical digression. What we are after is not philosophy. We want to find the answer to a simple question, can Mizuno-type excess heat, as measured in Colorado2 experiments, be explained by a prosaic effect? At least one such effect has been identified on the CMNS list -- escaping of the electrolyte during electrolysis. Is it reasonable to assume that the COP=1.24 we reported was a mistake resulting from not taking that effect under consideration? The question will probably be answered in coming weeks.

Appended on 12/17/06
In preparing myself for another exciting electrochemical experiment I found this brief description of chromium electroplating: "Electroplating involves immersing the metal parts to be plated in a bath of chromium trioxide (CrO_3), typically prepared by dissolving crystalline CrO_3 in a mix of distilled water and sulfuric acid. A direct current is passed through the solution, and the resulting reaction leaves a deposit of chromium on the piece being plated. One problem in this process is the production of hydrogen and oxygen at the electrodes. The gas bubbles to the surface, creating a mist of the plating solution (which contains hexavalent chromium) that must be controlled. Additionally, mechanical agitation of the bath (used to improve plating quality) can also result in the release of this hazardous mist. . . " The fact that bubble of gas remove chemicals dissolved in the electrolyte is probably known to chemists. In the same way bubbles of steam remove some liquid electrolyte creating wet mist in air. Why were Mizono's and Naudin's results not explained by chemists familiar with wet mist?

I suspect that several CF researchers were aware of the possible explanation and decided to stay away from the Mizuno type effect. I would not do this; I would criticize conclusions based on erroneous assumptions. If Favaurque was aware of the trivial explanation of the "apparent excess heat" then why didn't he address the issue in the paper presented at ICCF12? The CNAM team was going to conduct tests similar to those of Scott Little. I suppose that this was done several months ago, considering the simplicity of the task. Do they still believe that the claim of Mizuno-type excess heat was real? I do not know; no one wrote to me about the outcome of investigations. What would I do if after presenting a paper at an international conference I later found that the claim was wrong? I would certainly try to correct the conclusion and explained why I no longer believe in it. That is why I am puzzled by the absence of messages. The appendix professor Fauvarque promised will probably not materialize. Something is not right. What a waste of time and money it was! But I did enjoy the adventure. We did our best but the paper cannot be submitted without valid chemical arguments supporting the claim of excess heat.

Appended on 2/12/07

Pierre Clauzon sent me the description of their new reproducible results from a better experiment; Perhaps this will be presented at the CCF13 (Sochi, Russia). Here is my reply: ". Hi Pierre, I am glad you continue working and this topic. Did you convince Fauvarque to address the issue of chemical origin of excess heat? You paper would be more convincing if it contained a section devoted to possible chemical reactions and the amount of heat they generate. Is it less than 1% of what you measure? Is it less than 10%? Is it only 50%? Such numbers, justified by an electrochemist, are essential at this stage of research. I would be very interested to read your paper.

I WILL WRITE MORE ABOUT THIS WHEN ADDITIONAL TESTS ARE PERFORMED.

This website contains other cold fusion items.
Click to see the list of links

302) Alarming numbers and comments

This website contains other cold fusion items.
Click to see the list of links

Ludwik Kowalski; 7/10/2006
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This is a continuation of what was in the unit #301. What would happen to the CMNS field if the Mizuno-type excess heat is really shown to be an illusion? Not much. Mizuno-type experiments were only a small fraction of the EE (excess energy) sub-field. The CF and CT sub-fields (cold fusion and cold transmutations) cannot possibly suffer from an interpretational error in the case of GDPE (glow discharge plasma electrolysis). The only negative effect on the entire CMNS field can possibly come from a realization that it took ten years to find a trivial cause of the illusion. The fact that dominants researchers, Mizuno and Naudin, decided to remain silent when their conclusions were criticized will be noticed. People might generalize that all CMNS scientists are irresponsible. I hope that this will not happen.

At the beginning of unit #301 I wrote about preliminary results from Scott Little. Last night Scott send me final results. This morning these results were posted on the CMNS list (restricted Internet list for CMNS researchers). Three different methods were used to determine the percentage of the electrolyte: the method based on weight (already described in unit #301), the method based on concentrations of potassium (K) and the method based on concentration of tungsten (W). The XRF (X-Ray Fluorescence) instrument was used to measure the concentrations. How does this method work?

A narrow beam of X-rays is sent on a sample to be analyzed. The atoms in that substance are excited and emit characteristic X-rays. Like in optical spectroscope, individual elements can be recognized by unique characteristic X-rays. Scott optimized the instrument for the detection of peaks due to K and W. Once this is done he can compare intensities of characteristic rays, for different samples, with the intensity of a standard in which concentrations of atoms are known. An example of an XRF spectrum is shown in Figure 1. The abscissa is the wavelength of X ray photons while the ordinate is the number of accumulated counts. Characteristic wavelengths indicate presence of specific atoms while heights of peaks are proportional to concentrations of these atoms.

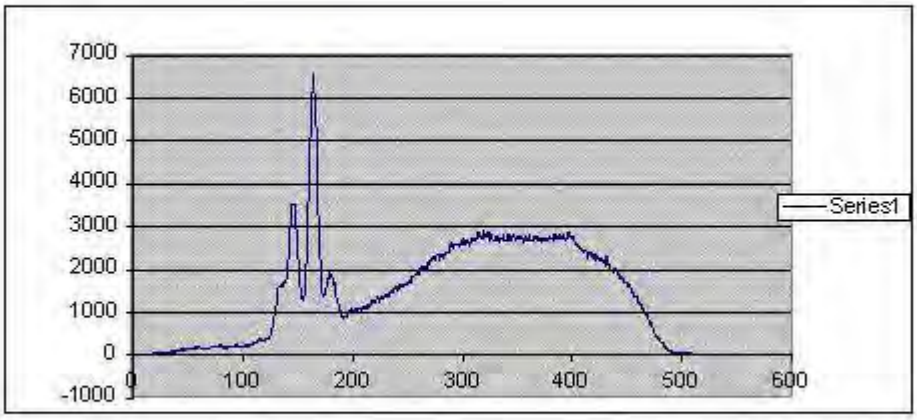

Figure 1
A typical X-ray fluorescence spectrum. Narrow peaks correspond to specific elements. The rest corresponds to the X-rays with which the sample was bombarded. The tail, under narrow peaks, is the background. Heights of peaks do not include that background. Numbers refer to arbitrary units.

Suppose, for example, that a particular peak is due K (potassium) atoms, as established by a preliminary calibration. How can this help us to determine the percentage of the electrolyte in the liquid lost during a Mizuno-type experiment? This can be done by comparing two spectra (a) that due to the sample of the original electrolyte and (b) that due to a samples of condensed liquid escaping from the cell during an experiment. The two samples must have the same volume, for example 27 cubic mm, and be analyzed under identical conditions. Suppose the height of the K peak in (a) is 8 units (after subtracting the background) while the height of the K peak in (b) is 2 units. Then one would say that concentration of K in (a) is 4 times higher than in (b). It is easy to consider two extreme cases: the escaping liquid is nothing but H_2O vapor and the escaping liquid is nothing but original electrolyte. In the first case the height of the peak for (b) would be zero. In the second case the height of the peak in (b) would be the same as in (a). The ratio of the peak height in (b) and the peak height in (a), multiplied by 100, is the percentage we are trying to establish. For the

hypothetical 2/8 ratio the contribution of the electrolyte to the escaping liquid would be 25%. That is the essence of the XRF methodology.

TABLE 1

sample	SCA conctr.	K conctr.	W conctr.	% drops by SCA	% drops by K	% drops by W	% 3 methods
#1	0.21	0.063	0.026	7.66	8.70	6.10	7.49
#2	0.12	0.041	0.026	4.38	5.66	6.10	5.38
#3	0.18	0.054	0.036	6.57	7.46	8.45	7.49
#4	2.74	0.724	0.426				mean = 6.79%
							st.dev=1.21%

Figure 2
Summary of results

Figure 2 shows the summary of results. It is based on what was posted by Scott Little last night. He provided numbers in the first four columns, I added additional columns to show the main conclusions. Sample # 4 was fresh K_2CO_3 electrolyte; its concentration was 0.2 M. Samples #1, 2, 3 were condensed liquids escaping from the electrolytic cell during a Mizuno-type experiment, similar to that in Colorado2. The four samples are identified in the first column. The next three columns show results obtained by using three different methods. Note that “SCA,” in the second column, refers to the method described by Scott at the beginning of unit #301. Numbers in that column are weights of solid deposits, in arbitrary units. SCA stands for a sensitive scale measuring the weights. Likewise, “K” in the third column stands for potassium. The numbers in the third columns are concentrations potassium atoms, in arbitrary units (heights of potassium peaks in the X-ray spectra). And “W” in the fourth column stands for tungsten. Numbers in the fourth column are concentrations of tungsten atoms in arbitrary units (heights of tungsten peaks in the X-ray spectra).

The columns 5, 6 and 7 show percentages of electrolyte in the escaping liquid inferred from numbers in columns 2, 3 and 4, respectively. That is what we really need to calculate the COPs correctly. Let me illustrate this by showing hoe the 8.7% result was obtained for sample #1 by using the K method. The concentration of K for the sample 4 was 0.724 arbitrary units. The concentration of K for the sample 1 was 0.063 arbitrary units. The peak ratio is 0.063/0.724 is 0.087 and this translates into 8.7%. Is this result consistent with what one finds by using the tungsten method. According to that method the percentage of the electrolyte in the escaping liquid is (0.026/0.426)*100 = 6.1%. And what was the percentage of droplets according to the “weight” method? The answer is (0.21/2.74)*100 = 7.7% Ideally all three methods should give the same result for the experiment in which sample #1 was collected. But measurements are not ideal. The mean results is (8.7 + 6.1 + 7.7) / 3 = 7.5, as shown in column 8. Results for the samples #2 and #3 were obtained in the same way.

The main conclusion is that the escaping liquid is not pure H_2O steam; several percents of the lost mass is probably removed as droplets of the electrolyte. We did not take this under consideration when COPs were calculated on the basis of data collected in Colorado2 experiments. All COPs would be significantly lower if percentages of the electrolyte in the escaping liquid was taken under consideration. After posting that conclusion on the CMNS discussion list (percentages from columns 5, 6 and 7) I found an interesting tutorial about steam at

http://www.spiraxsarco.com/learn/modules/2_2_01.asp

Here is a quote from it; it shows that the issue of "tiny droplets" is far from being new. ". . . Steam with a temperature equal to the boiling point at that pressure is known as dry saturated steam. However, to produce 100% dry steam in an industrial boiler designed to produce saturated steam is rarely possible, and the steam will usually contain droplets of water. In practice, because of turbulence and splashing, as bubbles of steam break through the water surface, the steam space contains a mixture of water droplets and steam. Steam produced in any shell-type boiler (see Block 3), where the

heat is supplied only to the water and where the steam remains in contact with the water surface, may typically contain around 5% water by mass."

I suppose this is a well known fact. But it is not mentioned in papers or Internet reports with which I am familiar. This can be explained by isolation in which CMNS research is done. A normal peer review process would certainly forced the authors to take the tiny drops effect under consideration. But reports published at yearly International Conferences on Cold Fusion, or over the Internet, are not scrutinized by selected experts, as papers published in mainstream journals. That is one of many undesirable results of exclusion imposed on the CMNS research about 15 years ago. On one hand no evidence of the pseudoscientific nature of CMNS was offered and on the other hand the field is not recognized as illegitimate. The situation is tragic; how can it be changed? Many think that this will suddenly happen as a result of commercial success; for example after a 100 kW generator, based on the CMNS effect, becomes available. I believe that focusing on scientific investigations, rather than on commercial applications, is more appropriate at this time. The CMNS field should be allow to evolve in the same way as other areas of science.

Let me return to conclusions drawn for Scott's results. These conclusions are based on a simplifying assumption which is most likely not valid. Ideally one need four samples #4, each removed from the electrolyte (with a pipette) only a moment before the experiment during which the corresponding sample (#1, #2 and #3) is condensed. We do not know what concentrations of the electrolyte were at the beginning of these experiments. I simply assumed that the concentration in sample #4 was the same as in the original electrolyte, and that fresh original electrolyte was used before each experiment. One can imagine several scenarios in which the inferred percentages of tiny droplets would be higher than shown in my table above.

For example, suppose that samples #1, #2 and #3 were extracted after a considerable delay. During that delay a significant fraction of the original electrolyte evaporated and replaced by distilled water. Knowing this one would correct concentrations of in sample #4 (for each of methods) , accordingly. The values would become smaller than those shown in my table. Suppose they become two times smaller. That would make all percentages two times larger. The mean of three means, 6.79% would become 13.58%. The COPs obtained during the three experiments, without accounting for the escaping electrolyte were close to 1.1. These values would have to be reduced to less than 1.0 for the 13.58% correction.

The bottom line is that conclusions made in my table are not reliable. Better experiments must be conducted. My suggestion is to use fresh electrolyte in each experiment, to extract samples of liquid electrolyte moments before experiments in which samples #1, #2 and #3 are extracted. Actually, three samples is hardly enough; we need 5 to 10 samples get reliable means and standard deviations (bars of errors). Will this be done in Paris3 and Marseilles1 experiments? I hope so. Will this be done to correct the COP's reported after the Colorado2 experiments (mean 1.24, st.dev = 0.13). It depends on us, the authors of the anticipated Colorado2 paper. I think that a simple CLA method of estimating percentages would be sufficient.

Appended in 7/13/06):

In a message posted today at the CMNS list I wrote that the problem that was labeled "tiny droplets" is far from being resolved. That contradicts what I wrote here earlier.

The mechanisms by which droplets of electrolyte are ejected are certainly worth investigating. But I would not start investigating them at this time, except in the context of our main question. That question is "should the total mass, M, lost during a GDPE experiment, be used to calculate thermal energy lost via vaporization?" The initial answer was "yes," the COPs were calculated from the uncorrected M in Colorado2. Then the question of tiny droplets was raised. We concluded that the mass used to calculate a COP should have been M-m, where m is the mass of escaping droplets of the electrolyte. Scott suggested a method for estimating m on the basis of weights of solid residuals, as explained at the beginning of unit #301.

That method was based on the expectation that the major component of solid remains, in two samples (initial electrolyte and condensed steam), would be K2CO3. But his XRF method revealed presence of tungsten. The original electrolyte cannot possibly contain a lot of tungsten; presence of W in that liquid was a proof that sample 4 was not

extracted from the initial electrolyte. That is why another set of samples must be analyzed. But a significant amount of W in solid remains from samples 1, 2, and 3 is alarming, as recognized by Peter Gluck. Let me elaborate.

First let me refer to the SCA method (explained by Scott at the beginning of unit #301). My conclusion, for example for Sample 1, was that m was 8.8% of M. But that was done by assuming that solid deposit in Sample 1 were chemically the same as in Sample 4 (the original electrolyte). But suppose that one half of the solid remains from Sample 1 was tungsten. Knowing this I would conclude that m was 4.4% of M, not 8.8%. And m would become 2.2% of M if 75% of the solid mass, in remains from Sample 1, were due to tungsten. To remove the ambiguity I would ask a chemist to extract tungsten from the Sample 1, before it is dried and weighted. Would the SCA method become reliable? Not really. It would be reliable only if other elements were not present, AND if only tiny droplets were responsible for the removal of the K and W, from the cell. Without these preconditions Scott's SCA method, as used in my table (see unit #302), could not be trusted, even if Sample 4 were extracted from the initial electrolyte. To make that method useful one must make sure that dominant non-water component, in all samples from condensed steam, is potassium.

But that is not all. How can we be sure that potassium, found in samples 1, 2 and 3, was removed from the beaker by tiny droplets? We simply assumed that this was the case. Suppose that K and W are removed from the cell as tiny colloidal particles present in the boiling electrolyte. The particles can be either dry or wet. Suppose they are dry. In that case a large concentration of K, found in the solid remains of condensed steam, could not be used as an indicator that m was large. One can easily have a situation in which $m \ll M$ while the concentration of K in the sample is not negligible. In other words, contrary to what I wrote in unit #302, we have no convincing evidence that the actually measured M must be corrected. How can one show that colloidal particles are "nothing to worry about?"

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303) Well known reactions or something else?

Ludwik Kowalski; 7/12/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

Unit 302 was devoted to possible interpretational error in calculations of excess heat. But that is only one of our problems. Suppose that all the data used to calculate the percentage of tiny droplets in wet steam are found to be reliable and that the COP (coefficient of performance) was indeed equal to 1.24, as described in unit #300. I will now assume that readers are already convinced that no experimental or interpretational errors were made when excess heat was determined. What is next? We must also convince readers that what was measured (excess heat generated at rates of nearly 100 W, and equal to 24% of the input energy) cannot be attributed to well known chemical reactions. Need for producing convincing arguments has been emphasized before. But no progress was made, I my opinion.

What follows are messages, posted on the CMNS list, devoted that topic. Most of it consists of short exchanges between Peter Gluck and myself. Other messages were from Mel Miles and Michel Jullian. All of them helped me to realize that the issue is not as simple as I imagined. Our messages are shown because I believe that they might be of some interest to others. Will Prof. J.F. Fauvarque, who is an electrochemist, produce a convincing appendix for our anticipated paper? I hope so.

Yes, I know that what is in all our messages can be reduced to one or two pages. But I decided not to do this. First because I am not a chemist and also because I believe that preserving sets of original messages is worth the effort. I wish more people participated; this list has at least several reputable electrochemists. They were probably too busy with their own problems. Perhaps they will later comment on what is now in this unit #303. I would be happy to append their contributions.

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On June 30, 2006, **Peter Gluck** (P.G.) wrote:
“Dear All, Given that the quantity of salt (actually K) lost is so difficult to measure, it has to be determined via a K balance - K initial vs. K left in the beaker. Without an absolute balance for H2O, K and CO2 we will have too much uncertainty for a credible scientific publication, I have said from the start of this discussion and I still think so. This is a difficult case - very hot gases in contact with a solution. The main perturbing factor is not chemical but physical - the loss of solution via entrainment. Gases at say, 3000 degree Celsius, can do a lot of things.

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Ludwik Kowalski (L.K.) responded:
Why should we exclude a possibility that what is escaping in tiny droplets of the liquid mixed with dry steam is [colloidal?] tungsten, or something else different from K? The method used by Scott (measuring masses) seems to be more universal. And it is very simple. Most laboratories have scales which are sensitive enough to detect solid remains. Confirming or refuting Scott's finding is important to all of us. One does not have to measure the COP to demonstrate that a sizable fraction of the lost mass is not pure water. A “solid remains” test, by the way, can be

performed by using stainless steel electrodes, as in many Naudin's experiments.

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P.G.:
Tungsten - and what happens with with it - c'est une autre chose- a different problem. The electrode is eroded and a precipitate forms. What is this? Based on my experience with the Cincinnati Cell I think the precipitate is mainly molten and frozen W droplets that capture inside a quantity of K. The droplets are formed by a known process that's called spark erosion. In the Cinci Cell, all the salts present are fixed physically in the molten metal. being removed from the solution, in the precipitate. The aqueous phase ceases to be electrically conductive.

The Cinci Cell has Zr electrodes - and Zr has a melting point of 2125 C, compared to 3680 C for W, but the mechanism is similar. However - to demonstrate this the precipitate has to be thoroughly analyzed. Anyway, it seems to be improbable that a part of the W electrode is flying with the droplets. Do we have soluble W present? And what's : "something else different from K?"

So **what is Scott weighing?** We know that a part of CO2 is also lost - carbonate, bicarbonate, hydroxide all can be present - K is the only thing measurable equivocally. In this case- the mode of thinking- including a hypothesis how the system works is as important as the execution of the experiment.

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L.K.:
That was my way of saying that "showing that amount of escaping K is negligible" is not the same as saying "what is escaping is pure water." The COPs were calculated by assuming that only pure water escapes. Now this assumption is being questioned.

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P.G. :
Ok, I understand what you say, but flying W could be easily detected. However we are able to know what happens in the system and we can decide - with certainty: there is excess heat, when, when not and perhaps later - why. The system is not awfully complex - the unique strange thing is the huge temperature gradient - molten W in contact with boiling water. How pyrolysis of water interacts with its electrolysis???? Is some synergy that causes the apparent excess heat?

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L.K. :
Yes, well known things and "strange" things do take place. The task is to summarize the well known reactions and to show that amount of heat they can possibly generate is orders of magnitude below what is measured. The system is not too complicated. And no additional experiments are needed to estimate chemical heat from known reactions. Perhaps I am wrong, but my guess is that for people with your background this should be trivial. It is only a matter of making reasonable assumptions about what is known and using the enthalpy table. Am I wrong, Peter? Can you perform such numerical calculations for us? Referees would certainly ask for convincing arguments that excess heat cannot be attributed to well known reactions.

P.S.
How much excess heat is generated in 5 minutes (a typical duration in Paris1 and Colorado2 test)? The answer is 100*5*60=30,000 J. How much gasoline must be burned to generate 30,000 J of heat? The answer is 30,000/50,000=0.6 grams. The heat of combustion is ~50 kJ per gram. Naturally, a Mizuno-type cell has no gasoline

in it. But what about other potential fuels? That is my question. How much can they possibly contribute to 30,000 J from "strange" things. My hope is that it will be a small fraction of one percent. But we need numbers from a knowledgeable chemist. Wishful thinking is not enough. Yes, I know that you will agree with this.

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P.G.:
No problem with the calculations. The problem is not with chemical phenomena. It is with a physical one, entrainment of droplets or mist by the very hot gases and this cannort be calculated yet. Many experimental data actually missing- composition of solution, of the precipitate, of the gases leaving the cell, we don't know a lot of things, sorry. No basis for a realistic calculation exists.

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L.K. :
That is not what I asked you for; sorry for not being clear. The incandescent temperature of the cathode belongs to "strange" things, not to well known and understood things. Here is my request to you again. Assume we are both 20 years younger and CF remains to be discovered.

A cell contains one liter of the 0.2 M K2CO3 electrolyte. The cathode is tungsten and the anode is platinum. The temperature is already 100 C. How much heat is generated when a constant potential of 30 V is applied for 100 seconds? Assume that a constant current of two amperes is flowing through the cell. Part of the answer is obvious, the thermal energy is 30*2*100 + X = 6,000 + X joules, where X is the energy released (positive or negative) via anticipated chemical reactions. Show, by making reasonable assumptions, that X cannot possibly exceed +60 J. That is what I am not able to do. How would you argue that chemical heat in only a negligible fraction of what is expected, Peter?

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P.G.:
What are the expected or possible chemical reactions? Participants are H2O, K2CO3, W - ignore glass-it is inert. Loci for reactions: cathode, anode, hot surface of W, plasma. Nature of reactions electrolysis and pyrolysis. What is unknown and I have not found in the electrochemistry literature - what happens at the anode- in principle carbonate, hydroxyl ions ions are going there but only (?) oxygen is formed. I have asked an electrochemist friend about anodic reactions at carbonate electrolysis but it seems they are not well known- at least in this special form of electrolysis.

However Mizuno has said he found some CO2 in the gases released from the cell too. How is this formed? Pyrolysis of K2CO3 is also a possibility. Carbonate captured by molten W is probably losing all its CO2. Anyway, I have hoped to have more data, systematic and complete after this series of experiments. A complete material balance could reveal what actually happens in this system. We still do not have such a balance.

The positive fact is that these reactions are all endothermic- with high probability endothermic. But unfortunately a physical phenomenon-entrainement of droplets of solution is a possible explanation of the calculated excess heat and we don't know its quantitative contribution to the results.

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P.G. :
I have already answered- we don't know exactly what happens at the anode, no pyrolysis is possible and in this case no droplets of solution are lost. Solution not boiling. (?) So what "anticipated chemical reactions"? This is a case of

logical error - replacing a case with a different one- not justified.

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L.K. :
1) Why trying to find out what happens in a simple case (30 V) before addressing a more complicated case (300 V with GDPE, pyrolysis and CMNS) is called a logical error? One learns by progressing from what is known to what is unknown. I am sure you know this.

2) I agree with "no droplets." In my illustration, the solution was boiling because the outside temperature was assumed to be 100 C. That was a simplification made to avoid changes of temperature inside the cell. Thus, what comes out is the H2O vapor mixed with H2 and O2. What is the upper limit of X, Peter?

3) In your earlier message I see "Participants are H2O, Na2CO3, W - ignore glass . . . " Na was probably a typing error. Right? But then I see " Pyrolysis of CO3Na2 is also a possibility." In any case, I asked you to ignore the 300 V case, in the first round.

4) I want to know if theoretically predicting the upper limit of X, in the case of 30 V, is possible. What is the answer?

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P.G. :
I am not able to find any exothermic reaction taking place in this system. In your opinion, what reactions have to be taken in account and used for the calculation?

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L.K. :
1) In other words, X must be negative. Right? That is an important point. I was not certain of this.

2) My opinion is that only dominant reactions should be taken in account. But I do not know reactions that accompany decomposition of water. And I do not know how to estimate their rates. That is why am asking you, a chemist.

3) Am I wrong in assuming that what is already known to chemists is sufficient to estimate X numerically? We will be nowhere without numbers. What is the value of X?

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P.G. :
I cannot speak for all my colleagues- by the way I am a chemical engineer, with Ph.D. in polymer technology. Now, for the real case I need the experimental results to know what reactions take place for all phases- solid, liquid, gases with analyses done correctly and professionally.

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L.K. :
OK, perhaps someone else will estimate the value of X for me. Knowing that it will negative is important but that is not a numerical answer. Please help !

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P.G. :
Who has tried till now? And what were the obstacles- I have explained that there more unknowns than equations.

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L.K. :
I was referring to an open cell with one liter of the 0.2 M K2CO3 electrolyte. The electrodes were W and Pt, potential 30 V, current 2 A and temperature 100 C.
Was it wrong to assume that the bulk of what happens under such conditions is well known to chemists?

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P.G. :
In this case, **very different from the case of plasma electrolysis,** the unique unknown factor is the fate of the carbonate ion at the anode, but there are no exothermic reactions conceivable. X is smaller than zero.

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L.K. :
Yes, you already wrote this, Peter. Can someone estimate the value of the negative X on the basis of what is known? If X is also negative when the COP is measured, then the apparent COP must be increased on the basis of the value of X. But how can we start talking about X at 300 V when we are not able to get it at 30 V?

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P.G. :
Let's take it stepwise - the main reaction is water electrolysis; its energy consumption depends on the temperature, current density, secondary reactions. How much of the electrical energy introduced in the system is used for water splitting? Theoretically!

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L.K. :
[Hmm, Socratic dialog?] Yes, this should be part of X. What is your answer? And what are contributions to X from other dominant reactions, based on anticipated rates?

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P.G.:
I think we are now near to the answer you wish. Normally electrolysis will take 1.48 V (approx.) from 30 V. The problem is with the "other dominant reactions"- I don't know what are they, where they take place and in which extent.

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L.K. :

Thanks Peter. You probably meant 1.48 eV, not V. Is this per atom of hydrogen produced or per H2 molecule produced? Assuming this is per molecule I concluded that X, for that reaction only, is -295 J. Do you agree?

a) 1.48 eV=1.48*1.6*10^-19 J = 2.36*10^-19 J Therefore X=2.36*10^-19 * number of H2 molecules.

b) How many molecules are produced when I=2 A and t=100 s? The charge is 2*100=200 C = 0.00208 faradays

c) IF (???) one Faraday produces one mole of H2 then the number of molecules is 0.00208*6*10^23 = 1.25*10^21

d) In that case, |X| = 2.36*10^-19*1.25*10^21 = 295 J.

e) That is nearly 5% of the electric energy supplied ! ! ! How significant can this be? Suppose a claim is made, NOT REALISTICALLY FOR 30 V, That the electric energy = 6000 while the thermal energy =7000 J. Dividing 7000 by 6000 one could say that the COP=1.17. But that would be wrong. The correctly calculated COP = 7000/(6000-295) = 1.23.

f) And that is only from one endothermic reaction, decomposition of water. We must also correct for other dominant reactions. I hope someone will calculate the value of X that accounts for all dominant reactions. Is it really so difficult for a chemist?

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P.G. :
No..., splitting of water needs~1.48 volts minimum, in practice 1.6-2.0 volts are used. The difference to 30 V is heating the solution. Approx minus 5-6% (1.5- 1.6 of 30) So much of the energy introduced is consumed for the main endothermic reaction. I don't know any exothermic reaction possible in the system. Do you agree?

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L.K. :
What is your value of X in joules? Volts are not units of energy. How many joules of energy were used in 100 seconds to produce hydrogen? I think that the answer has nothing to do with 30 V. It would be the same for 15 V or for 45 V. Only the total electric charge counts. Do you agree, Peter?

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Mel Miles (M.M.)wrote (7/1/06):
The minimum voltage thermodynamically possible for splitting water must be based on "Delta G" and is 1.23 volts at standard conditions. For enthalpy calculations, "Delta H" is used and is 1.48 volts at standard conditions.

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L.K. :
Does this mean that 1.23 eV is the binding energy of the H2O molecule with respect to its three separated atoms? I am thinking about this as work that has to be done to overcome attractive molecular forces.

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P.G. :
[Incorporated into my message below, in blue]

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L.K. :
On Jul 2, 2006, at 2:22 AM, Peter Gluck wrote:

What's the amperage?

1) Let me post extracts from what I already wrote. This is mostly for people like Mike C, who missed the beginning.

"A cell contains one liter of the 0.2 M K2CO3 electrolyte. The cathode is tungsten and the anode is platinum. The temperature is already 100 C. How much heat is generated when a constant potential of 30 V is applied for 100 seconds? Assume that a constant current of two amperes is flowing through the cell. Part of the answer is obvious, the thermal energy is 30*2*100 + X = 6,000 + X joules, where X is the energy released (positive or negative) via anticipated chemical reactions. Show, by making reasonable assumptions, that X cannot possibly exceed +60 J. That is what I am not able to do. How would you argue that chemical heat in only a negligible fraction of what is expected, Peter?"

It is not established a priori - depends on the experimental conditions.

2) Yes. And my request for X (number of joules) was for a specific (assumed) experimental conditions. Such concrete-operational approach is often helpful.

Excuse me- but I really think this hypothetical case is much too
different from plasma electrolysis in order to be of any use.

3) I strongly disagree. Why did I ask you to calculate X (positive or negative contribution of each dominant chemical reaction to excess heat) for 30 V? Because this is a case of basic electrolysis. It is not complicated by glow discharge plasma electrolysis. How can we be confident that our analysis of a more complex situation is correct if we are not able to get the answer for a much simpler case?

Mike Carrell has presented you a good idea that has to be taken
in account **when** finally we will have a **complete material balance**
of the Mizuno process.

4) Yes, we should not stop this discussion after the X is determined for a simple case. Recall what we decided to do this at the beginning. We agreed that "things" should be divided into two categories; well known and unknown. You used the term "strange" for pyrolysis and I placed the CMNS under the same category. The 30 V case was invented to deal with well known things only.

Till then, good old Kotarbinski, the father of praxeology, will roll
angrily in his grave. Let's be efficient and systematic, friends!

That is right, First 30 V and then 300 V. The Polish philosopher would agree with this. I am really disappointed that other chemists on this list do not help us to estimate X. The only thing we were able to do, so far, was to find the contribution of one reaction (decomposition of H2O) to X. Was my answer, -295 J, correct, Peter? Is it reasonable to assume that contributions from other known reactions (at 30 V) are negligible in comparison with that number?

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Michel Jullian (M.J.) wrote:
Ludwik: here is the explanation for the 1.48V (V, not eV) our friends keep throwing at you.

For water dissociation the enthalpy tables (e.g. those in my calculator) give:

H2O(l) -> 0.5 O2(g) + H2(g) - 285.83 kJ/mol (endothermic)

The delta H 286 KJ per mole is of course Avogadro's number NA (6.02*10^23) times the required electric energy to dissociate one single molecule, which is therefore:

E=(286 * (10^3)) / (6.02 * (10^23)) = 4.75*10-19 J per molecule

We also know that we need to circulate 2 electrons per molecule, so if V is the part of the electrolysis voltage used to dissociate the molecule and e is the electron charge 1.6*10^-19 C, another expression for the required electric energy (work) for the molecule dissociation is: E=V*2e. Thus:

V=E/2e = 4.75*10-19/3.2*10^-19 = 4.75/3.2 = 1.48V

Let me know if it makes sense, and if in this light you agree that the short paragraph I suggested for the "water electrolysis" section of your paper is sufficient.

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L.K. :
I do remember, vaguely, the dG and dH and T*dS. But how does this relate to the fact that 1.48 eV is the "electric energy to dissociate one single molecule?" Assuming that the 1.48 eV is the depth on the potential-energy-versus-distance plot what is the meaning of 1.23 eV mentioned by Mel yesterday? And what is the meaning of the 1.6 to 2.2 V range mentioned by Peter?

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P.G. :
1) [You asked] “and what is the meaning of the 1.6 to 2.2 V range?” That's because the efficiency of water electrolysis is only 70-50%. Working at more higher voltages does not help more.

2) [You asked about 295] In this very case 1.48 X 2X 100= 296 J and you are right. Endothermic reaction. As told I have no idea re possible exothermic reactions but my (our) worry is not reactions but physical phenomena as droplet mist entrainment. I acknowledge my ignorance or lack of imagination re exothermic reaction in the system at 30V.

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L.K. :
1) And what about endothermic reactions, Peter? My impression was that you gave up on calculating the X for them. Is it not true?
2) It appears that you are not the only chemist on this list who did not estimate contributions of well known chemical reactions to X. I was probably wrong in assuming that this should be a trivial problem for most chemists.
3) Do you agree that the Colorado2 claim, COP=1.24, would not be taken seriously unless it is accompany with a credible statement that "contribution of known chemical reactions to excess heat has been shown to be less than 1%," or something like this?

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P.G. :

I am contented with electrolysis as a great entoderm contribution and do not think there are other such reactions. What should motivate me to search for nonexistent reactions when I see that existent physical phenomena make us Great trouble? I need that complete material balance to understand the system. And we are discussing hypotheses as 30 V electrolysis. Not effective and not efficient. Tell please explicitly what real reactions do you will see analyzed dissected and measured.

The Colorado claim will not be taken seriously till we do not know what happens in the system, the real experimental system. We can exercise our fantasy beyond any limits but the paper has to be based on reality. My question is: How great is the contribution of the entrainment of droplets of solutions to the material balance? As soon I know it, plus we make the complete balance, our dialogue will continue. I am seeking for kairos not chronos at CMNS

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L.K. :
[Peter wrote] I am contented with electrolysis as a great endothermic
contribution and do not think there are other such reactions.

1) I am willing to accept this. --> The value of X is close to -295 J; contributions of other reactions to X are negligible. That means we can go to the next step. Suppose we start increasing the voltage. How does the 60 V situation, for example, differs from the 30 V situation? I am assuming that the current is still 2 A. The electrical energy delivered becomes 12000 J (instead of 6000 J in 100 seconds) but X remains essentially the same because the current is the same. Is this an acceptable expectation? I hope that other chemists on this list will agree.

[Peter wrote] The Colorado claim will not be taken seriously till we do not know
what happens in the system, the real experimental system. We can exercise our
fantasy beyond any limits but the paper has to be based on reality.

2) I hope you are not referring to the draft of our paper at:

http://blake.montclair.edu/~kowalskil/cf/300positive.html

Do you see any "fantasy" in that draft?

[Peter wrote] How great is the contribution of the entrainment of droplets of solutions
to the material balance?

3) Scott Little described a method by which this question can be answered. His preliminary answer is that droplets of the electrolyte contributed about 7% to the total mass lost during an experiment. That remains to be independently confirmed. . . .

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M.M. :
It is Delta G and not Delta H that determines if a reaction is thermodynamically possible at constant temperature and pressure (usual laboratory conditions). Consult any Physical Chemistry text for this result.

The splitting of one mole of H2O requires an applied energy of Delta G = +237129 Joules(1 J= 1 V*A*s). Converting to eV per molecule yields 2.45766 eV. Using Delta G=-zFE gives E=-1.22884 V at standard conditions(298.15 K temperature and 100000 Pascals pressure,slightly below one atmosphere). Note that 2.45766 eV and -1.22884 V differ by a factor of two but have different units and sign. The factor of two arises because the splitting of one molecule of water, i.e. H2O = H2 +0.5 O2, involves the transfer of two electrons for the electrode reactions at the anode and cathode. Note that these calculations are for splitting water into hydrogen and oxygen gases and not for the splitting

into H and O atoms.

To state in simple terms, multiplying the 1.23 V by 2 gives the minimum energy in eV(2.46 eV) required to split a single water molecule at standard conditions.

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M.M. :
Peter, for the LiOH-H2O electrolyte, I believe the reaction at the anode would be
2 OH- = H2O +0.5 O2 + 2e-
I cannot think of any other likely anodic reactions for the LiOH-H2O system. For carbonates, various electrode reactions are possible. This is especially true for the high voltages of plasma electrolysis. Reactions of the metal used as the anode should also be considered under the extreme conditions of plasma electrolysis.

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P.G. (A private message 7/13/06)):
Please feel free to use my name -- for everything I wrote and (hopefully) will write at CMNS..... Think about temperatures in plasma, and in the gases bubbling through the water phase -- in the vicinity of the cathode and the anode. This can explain a lot.

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304) Recent representative messages

Ludwik Kowalski; 7/19/2006
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What follows are recent Internet messages posted on the CMNS discussion list. The debate started in 1989 is going on. On what basis are the authors of these messages accused of being pseudo-scientists? In my opinion their methods of validation are 100% scientific. They perform experiments, they discuss results and they develop theories, like in other areas of physical science. But our scientific establishment is practicing an unfair discrimination against the CMNS field. When will this end?

L.K. (7/14/06)
1) Yes, knowing the weight of every substance, before and after a COP test, would be highly desirable. But I would not do this without help from a competent, and well equipped, analytical chemist.

2) What did Fleischmann and Pons know about each chemical substance appearing and disappearing in their setup? Did they measure masses of these substances before and after excess heat experiments? Did those who validated their excess heat conclusions, for example, Longchamps et al., determine contributions of chemical reactions to excess heat? If so then I would very much appreciate a reference, or several references.

3) My impression, based on reading what others wrote about F&P, was that chemical contributions to excess heat was shown by them to be negligible, a very small fraction of one percent. But I was not able to find a paper describing this. Does it exists or everyone simply assumes that it exists?

Ed Storms (E.S.) (7/18/06):
Bockris published a paper addressing this concern. As any chemist can demonstrate, the components in a F-P cell cannot produce energy, they can only consume it because everything is in its lowest chemical state.

L.K. (not posted)
The reference given was a paper published by Kainthla et al. (in Hydrogen Energy,14, #11, 771; 1989). Bockris was listed as the last of eight authors. The title of the paper was "Eight chemical explanations of the Fleischmann-Pons effect." Here is how the issue was discussed by Ed Storms in review published in 2000. The title of the review was “A Critical Evaluation of the Pons-Fleischmann Effect;” it can be downloaded from the library at <www.lenr-canr.org>.

In the section entitled question 2.2, Ed wrote: **“**Can prosaic sources of chemical energy be ruled out? The hard-to-accept claim for a nuclear source is based, in part, on the belief that observed energy production exceeds any known chemical source. Therefore, the potential chemical sources must be examined. Before discussing this subject in detail, the reader should realize that a typical cell contains very few chemical components, all of which are stable with respect to each other. A chemical reaction can only be initiated by applying an electric current, a process which uses energy. Only after the water has been split into deuterium and oxygen can chemical reactions occur. This process causes several chemical reactions, including an uptake of deuterium by the palladium and slow deposition of lithium and platinum on the cathode surface. Each of these reactions involve very little energy. Strain energy accumulates in the palladium and small amounts of reaction products such as D2O2 can accumulate in the solution under proper

conditions.

These processes have the potential to store energy within the cell. Only release of stored energy can be used to explain the anomalous energy, which appears after many hours of electrolysis. The magnitude of such processes was addressed in several papers. Kainthla *et al.*[37] discussed eight possible sources, including recombination, which has been already discussed above. The other sources are the energetics of PdD formation, the energetics of PdLi formation, and energy accumulation as stress. Each of these was found to be much too small to account for even the smallest reported excess energy. Handel[38] proposed that heat could be pumped into the cell by a temperature gradient operating between the anode and cathode lead wires, *i.e.* a Peltier Effect. Two problems exist with this explanation. A significant temperature difference seldom exists between the two lead wires and the necessary difference between the Peltier coefficients of the wires must be unreasonably large to produce an observable effect. Most people use platinum for both wires, which would have a zero difference in the Peltier coefficient.

Balej and Divisek[39] compiled all necessary thermochemical values and calculated the energy involved in electrolytic formation of b-PdD. Measured energy using inert palladium is consistent with these values,[18] as is true of all negative studies. Berlouis *et al.*[40] reported a direct measurement of the heat of dissociation of PdH0.9, formed by electrolysis, using thermal analytical techniques. The result is consistent with values obtained by using other techniques. Consequently, no anomalies have been found in the energetics of b-PdD. However, it is possible for an unknown compound to form which, when decomposed, could give off the observed energy. While such a compound has been sought, it has not been detected. In addition, such a material would have to be able to store hundreds of megajoules of energy in the few grams of palladium normally used. If this were the energy source, the material would have an energy density greater than the most powerful chemical explosive.

References:
11) Kainthla, R.C., et al., Eight chemical explanations of the Fleischmann-Pons effect. J. Hydrogen Energy, 1989. **14**(11): p. 771.
12) McKubre, M.C.H., et al., Development of Advanced Concepts for Nuclear Processes in Deuterated Metals. 1994, EPRI.

Bockris is a highly respected electrochemist. I will accept his testimony (that the F&P excess heat could not be attributed to chemical reactions) on the basis of authority. Ed wrote that chemical composition of F&P cells is always the same. That is why other researchers took the initial findings for granted. Here is what I found about the above reference 12. According <www.tvwiki.tv/wiki/Cold_fusion> " McKubre et al. [12] have conducted careful inventories of chemical fuel and potential storage mechanisms in cold fusion cells, and they have found neither fuel nor spent ash that could account for more than a tiny fraction of the excess heat." That is also reassuring. Mike McKubre is one of those researchers whose excess heat data, in F&P type cells were highly reproducible at levels below 2 W.

Post Scriptum: Referring to the above quote from the wikipedia, Mike McKubre wrote: " On several occasions we did find and reported (at ICCF8 and elsewhere) a nuclear "ash" quantitatively (and temporally) correlated to the excess heat - Helium-4. With regard to chemical "ash" our statement is actually stronger than the one written above. We never saw any evidence of any net chemical reaction accompanying excess heat production. The statement of less than 1 part in 100 (or 1000) refers to any conceivable chemical reaction of the cell constituents - no matter how unlikely or unobserved."

5) Generally speaking, one can identify two ways of dealing with the issue of what is going on in a F&P cell. Let me name them "negative" and "positive." The "negative" approach, suggested by Peter and Michel, (see unit #303) is one of them. The best one can do with that approach is to produce convincing data that excess heat is not due to a well known chemical process. The"positive" approach would be to show that excess energy can be explained by a particular kind of a process, for example, fusion producing neutrons, protons, tritons and helium-3. What must be done, experimentally, to convince ourselves that excess heat is indeed due to emission of such particles? One must show that the total number of nuclear particles, and their energies, match the measured amounts of excess heat.

Many attempts in that direction have been made in the past but they were not successful. Excess heat was always found to be zillions of time larger than what could be made consistent with observed emission of neutrons, protons,

tritons and ^{3}He. Another particular process that was investigated was cold fusion in which dominant products are atoms of ^{4}He. Such process is extremely rare in hot fusion (collisions of isolated deuterons) but that does not mean that it must also be rare in cold fusion (where deuterons are not isolated). I know that accumulation of 4He has been studied by many researchers and that the rate of accumulation of excess heat (about 24 MeV per atom of ^{4}He) was found to be consistent with the expectation. It is well known that the mass-energy of two deuterons exceeds the mass-energy of one ^{4}He by 23.8 MeV.

In my opinion the "positive" approach -- showing that excess energy measured matches amount of "ashes" (products of specific processes) is likely to convince mainstream scientists that something unexpected is going on, than the "negative" approach suggested by Peter and Michel. People who studied accumulation of ^{4}He reported high reproducibility of results. Taking this for granted, I would recommend (if it were up to me) the following approach. Select five recognized experts in ^{4}He detection and ask them to replicate already reported results. If at least one of them confirms the ~24 MeV of excess heat per one ^{4}He atom then it would be reasonable to assume that the effect deserves further investigations. My expectation is that all five experts would report proportionality between accumulated excess heat and accumulated ^{4}He ash. The energies per atom would probably be different, for example, ranging between 15 and 35 MeV.

Even 5 MeV per atom of an ash would be six orders of magnitude higher than what is possible with a chemical reaction. But what would one have to say if reproducible results were 1 to 5 MeV per atom? That would tell us that ^{4}He is not produced via direct fusion of two deuterons. We would say that another nuclear process, or a sequence of processes, is involved. How else can atoms of a new chemical element be produced?

Post Scriptum
Accumulation of ^{4}He was one of the three categories of topics identified for the 2004 DOE review. The panel of appointed experts was asked about "production of ^{4}He as an ash associated with this excess heat, in amounts commensurate with a reaction mechanism consistent with D+D --> ^{4}He + 23.8 MeV (heat)." Unfortunately, not a single reexamination request was made by the DOE. What they did was to collect comments from experts who were asked to read existing reports. I was very disappointed by this. Here is how comments made by the appointed experts were summarized in the DOE report:

"The hypothesis that excess energy production in electrolytic cells is due to low energy nuclear reactions was tested in some experiments by looking for D + D fusion reaction products, in particular ^{4}He normally produced in about 1 in 10,000,000 in hot D + D fusion reactions. Results reported in the review document purported to show that ^{4}He was detected in five out of sixteen cases where electrolytic cells were reported to be producing excess heat. The detected ^{4}He was typically very close to, but reportedly above background levels. This evidence was taken as convincing or somewhat convincing by some reviewers; for others the lack of consistency was an indication that the overall hypothesis was not justified. Contamination of apparatus or samples by air containing ^{4}He was cited as one possible cause for false positive results in some measurements." Why was a reexamination of the effect not ordered by the DOE after some of the panel experts reported that the evidence was convincing?

Jacques Dufour (J.D.). . . the worst of our enemies is ourselves. We think our experiments perfect, we are sure that we have answered theoretically all the questions. This is very far to be true. Submitting the work you do to critics and having the lucidity to accept remarks when they are valuable is essential. It is a great error not to seak interaction with people on the ground that they are our enemies and that we don't have to take into account their advices. . . .

E.S.:
I agree with Jacques on a few of his points. Of course talking with people who do not yet accept cold fusion is important. It is essential to understand just what they lack to allow them to arrive at a rational conclusion. We are long past the situation where the reality of cold fusion is in doubt. Too many of the required questions have been answered and too many studies from which the main errors have been removed have been reported. I'm in the process of examining this literature in detail for a book, so I can say with confidence that the claims are true. I'm also aware that most people who do not believe are simply ignorant of this extensive literature, as are many people in the field.

Consequently, when dealing with skeptics, you have to realize you are dealing with someone who has a very limited and selective knowledge. Their attitude of rejecting what you say because you are a believer, hence can not be trusted, is totally illogical. All scientists have basic beliefs based on what they have been taught or have observed in nature. These beliefs do not make them untrustworthy when describing the evidence for these beliefs. ..... The challenge now is to evaluate what is real and what is not real in an effort to understand just how the effect works. We have a mixture of good and bad work, and a mixture of bits and pieces of the total puzzle. The challenge is to sort this collection into pieces that make sense and can advance our understanding about how the amazing process works.

Michel Jullian (M.J.):
Ed to reconcile your two statements above, there must be at least one experiment of which you are 100% sure, which one is that?

E.S.:
Well, aside from my own work :-), I pick the McKubre heat measurements, the Iwamura transmutation studies, the Miles helium studies, and the Claytor tritium studies. I pick these because the work is so well done and described to allow critical evaluation. Many other studies would be considered very good once the CF phenomenon is accepted. All of the radiation studies are important because it is impossible to miss radiation and none should be produced by the low energy CF cells. All of the neutron measurements are useless and irrelevant to the issue. In addition, 185 credible heat measurements, 58 reports of significant tritium, and 67 studies of transmutation are published. Any other field of study would be embarrassed to have rejected this much work by well known and competent scientists.

J.D.:
. . . I have tried to pass the message that the most important thing in the field was the heat measurement and that the interpretation would come later. But every body sticks to the nuclear interpretation.

E.S.:
All of the methods are difficult to make work every time. When the effect is made to occur and the experiment is well designed, any of the methods would be accepted by normal scientists. However, some methods are more complex than the others, the Mizuno method for example. Consequently, this method is more difficult to demonstrate, as you are discovering. The best method is to design a good calorimeter that you can demonstrate is accurate, use it to measure heat by the classic F-P approach, and measure another expected nuclear product at the same time - helium or radiation for example. If you can show a one-one correlation between heat and another nuclear product, the work gains in credibility, but also gains in difficulty. I suggest, no matter how well you do the Mizuno experiment, the apparatus will be too complex to convince the average person. The work needs to be understandable by an average 10 year old for many people in physics to believe the results.

L.K.: (7/18/06):
"..... I suppose that many of the 185 heat measurements, mentioned by Ed, were in electrolytic cells similar to those used by Fleischmann and Pons. As far as I know, excess heat, in highly reproducible experiments, was most often generated at rates below two watts. Is this correct? And the emphasis was to show that excess heat was not an illusion due to experimental errors.

What I would like to see are reports claiming that excess heat was extraordinary. That is the most interesting (and most controversial) part, as far as science is concerned. Like Jacques, I am willing to wait for convincing demonstration that the measured excess heat is nuclear. Showing that it cannot be explained by prosaic effects would already by a big step forward. I am taking it for granted that calorimetry was essentially error-free, and that electric energy supplied to cells was measured properly. But how did the authors of reports convince themselves, and others, that their highly reproducible results could not be attributed to a chemical reaction? Which reports are the most convincing in that respect?

M.J.
Ludwik, I disagree with your comment that calorimetry and input power measurement can be trusted and only chemical heat should be distrusted, we must check carefully every single possible artefact, or the house of cards falls

down. . . .

L.K.:
1) Nothing should be trusted in a single report. But tens of highly qualified scientists reported positive excess heat results. That is why I am confident that excess heat, generated at the rates below 2 W, is not an illusion due to experimental error. Experimental errors would produce as many positive as negative results. My impression was that results above 2 W are not yet reproducible in F&P cells. Please correct me if I am wrong.

E.S.
1) That is correct. [many of the 185 heat measurements, were in electrolytic cells similar to those used by Fleischmann and Pons]. However, several other methods have been used including low energy plasma and exposure of the NAE [nuclear active environment] to ambient gas pressure.

2) [Ludwik wrote]: "As far as I know, excess heat, in highly reproducible experiments, was most often generated at rates below two watts. Is this correct? And the emphasis was to show that excess heat was not an illusion due to experimental errors." A wide range of maximum heating power has been reported. However, the results can only be properly evaluated when the expected error is included. For example, excess power of 5 mW is reported, but this was obtained using a calorimeter accurate to a microwatt. The issue is not the amount of power, because this is determined by the amount of NAE present. The interesting result is how the NAE can be produced.

3) If you study the Miles work about the He-heat relationship and add the 6 other studies that agree with his results, you will see that a nuclear reaction has to be the explanation.

4) Not every study can prove the results are not chemical or are even caused by a nuclear reaction. However, if someone does an experiment and gets the same anomalous result as someone who has shown their anomalous results to have a nuclear source, it is reasonable to conclude that you both are seeing the same phenomenon. After all, when chemists study a chemical reaction, they are not required to prove each time the existence of atoms or the laws of thermodynamics. At some point, people acknowledge the reality of the underlying process.

L.K.:
1) I suppose that demonstrating the non-chemical origin of excess heat generated at the rate of 5 mW would be much more difficult than at 1000 mW.

2) In an earlier message you wrote: "Two issues exist that are frequently confused. The first is whether an anomalous effect has actually been observed. .....The second issue is the process causing the anomalous behaviors." That is true. I am familiar with many reports describing anomalous effects; emission of charged particles and Iwamura's transmutations are good examples. My question was about something very different. I asked for chemical data supporting the anomalous nature of excess heat generated in highly reproducible experiments, such as those reported by McKubre. As I wrote before, I believe that reality of excess heat was demonstrated in these experiments. But what was done to show that excess heat measured could not be attributed to prosaic effects?

3) You also mentioned anomalous helium demonstrated by Miles, and by other researchers. Generation of 4He at the rate of about one atom per 24 MeV of excess heat is indeed a very convincing argument that a nuclear process was taking place. You are correct that this automatically rules out chemical explanations. Even ten times lower rate would exceed the expected chemical rate by six orders of magnitude (MeV versus eV). I wish we were sufficiently equipped to demonstrate the unusually high rate of accumulation of reaction products in Colorado2. But we were not. The only method available to us, if we want to demonstrate the anomalous nature of excess heat, is chemical analysis of reaction products, as argued by Peter Gluck. Our manuscript would have no chance of being published without an appendix with arguments against trivial origin of excess heat. Do you agree?

E.S.:
1)all of the F-P cells use the same material, the same chemical arguments apply to all of them, as above.
2) [Concerning "our manuscript would have no chance of being published . . "] The Mizuno cell is more complex chemically than the F-P cell. Consequently, it requires more discussion. As I suggested before, the best defense is to

prevent anything from leaving or entering the cell. This way, chemical effects can be easily ruled out.

Mitchell Swartz (M.S.):
Referring to F&P cells Ed wrote that “everything is in its lowest chemical state.” Actually this is not correct. There is energy given off by the loading of palladium by the deuterons which we have been measuring for years with our sensitive calorimeters. That heat given off with loading ranges from circa 50 to several hundred joules or so, but in any case the heat given off is DWARFED by both the excess energy during the experiment and the "heat after death" (when the systems are correctly operated at their optimal operating points), and is recovered when deloading occurs. However, this demonstrates that they are not in their "lowest chemical state" at t=0+.

Also, since the Gibbs free energy involves BOTH enthalpy and entropy, I do not believe all other materials are in their lowest chemical state during the run. For example, I reported that both gold, palladium, and platinum anodes decompose very slightly producing an overlay on the cathode which is capable of, in some cases of palladium with heavy water (and certain other cases) of producing slight excess heat (as I discussed at LENR-2 Texas, which you attended, and thereafter termed "crud"). FWIW that slight excess heat is DWARFED by the excess heat achieved when the reactions are correctly carried out.

In addition, there appear contamination issues arising from the containers (etc.) which are an entropic effect also demonstrating that the systems are not in their "lowest chemical state". However, again, although the contamination markedly lowers the excess heat, as we demonstrated at ICCF-10 (2), and although the energy involved is DWARFED by the excess heat achieved when the reactions are correctly carried out, it is not strictly accurate to make the claim that all materials are in their "lowest chemical state" at t=0+.

The bottom line is that careful calorimetry with full controls, waveform reconstruction, and time integration, and with monitoring of loading energy, etc. is required to prove excess heat. Handwaving alone is probably not sufficient.

Dr. Mitchell Swartz
JET Energy

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(2) Swartz. M., G. Verner, "Excess Heat from Low Electrical
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Cold Fusion Times http://world.std.com/~mica/cft.html
The journal of the scientific aspects of loading isotopic fuels
into materials ISSN# 1072-2874

JET Energy http://world.std.com/~mica/jet.html
Working for Safe and More Efficient Heat Products to Serve You

E.S.:
Actually, formation of PdD produces energy but the need to decompose D2O to get the deuterium results in a loss of energy, i.e. the net effect is an endothermic reaction.

M.J.:
. . . We don't need tens nor thousands of experiments, we need a single one that anyone could trust 100%, agreed? Do we have it?

E.S.
Actually, five criteria determine what is accepted as reality in science.

1. As you say, a perfect experiment would be nice. Unfortunately these are very rare in all fields of science. Clever people can always imagine alternative explanations. Nevertheless, we have several that are close.

2. A collection of measurements that all show the same patterns of behavior. For example, everyone who makes the necessary measurements sees that greater heat is associated with a greater D/Pd ratio.

3. Finding measurements that give values in agreement with other independent studies. For example, the measured He atom/watt-sec agrees with energy values calculated from mass defect.

4. The measurement agrees with accepted theory.

5. 100% reproducibility is possible.

Criteria #2 and #3 have been met by several different types of behavior. We are working toward the last two. No observation ever starts out meeting all five criteria, yet novel observations are frequently accepted and studied until all five criteria are met. Why should cold fusion be different?

L.K.:
. . . Fleischmann's papers show no evidence of analytical studies of reactants and all products (before versus after). Was such analysis perform for at least one McKubre's experiment ? You referred to them as highly reliable. Perhaps Mike will tell us something about this.

Mike McKubre (M.M.):
Our experiments were designed so that the issue of potential chemical energy sources could be very easily bounded and a complete (or sufficient) chemical balance was attained in every case of reported excess heat. I discussed this many times in tedious detail in 1989-92 but let me summarize here.

The SRI heat producing cells were thermodynamically closed, physically sealed, and contained only Pt (anode and wiring), Pd (cathode), D2O + LI (electrolyte), fused silica and PTFE (walls and inner construction). Sometimes there were known trace additions of deliberate additives (most commonly Al and Si, occasionally B and Be - the last I would not advise). The recombiner catalyst was Pt (or Pd) on alumina. Cells were precharged with D2 gas in an amount calculated to leave a positive gas pressure (sometimes large - mostly small) after full D/Pd loading.

It is standard calorimetric procedure to ensure that final state = initial state. This was done by deloading the Pd and checking that the electrolyte concentration (directly reflected in the conductivity) is the same at the end as the beginning. With these constraints it is easy to show that the sum of all chemical reactions is VERY small compared to the two elephants: the integrated input IV and any excess heat. I have often stated publicly that the ratio of excess energy we observed to the sum of all conceivable chemical sources is 100 - 1000 (or more). A shorthand way of understanding this is that we have measured excess energy corresponding to more than 1000 eV / Pd atom* [footnote below].

Both Ed & Mitch have spoken about the transient enthalpy of D loading (then unloading) in Pd. In our scheme the net enthalpy in both directions is very nearly zero. When D is derived from D2 the loading process is at first exothermic (the partial molar enthalpy negative). That is, the D prefers to be inside the Pd until a loading of D/Pd~0.725 (I am working from memory here). At this point the sign reverses and it becomes increasingly more difficult (endothermic) to insert each D atom** [second footnote]. At a loading D/Pd ~ 0.95 (or maybe a little higher) the integral enthalpy is zero. Independent of sign the one way enthalpy is (as Mitch stated) DWARFED by input and excess enthalpies. In

the method that Ed described where cells do not operate with excess D2, all loaded D must come from D2O (H2O) and Ed is correct that the net enthalpy of D (and H) absorption is always negative.

Each experiment needs to be evaluated separately and no general rule should be assumed to apply. Even as (presumably) competent electrochemists with colleagues who are (presumably) competent analytical chemists we chose at the outset to make our lives easier by designing simple experiments with few moving (i.e. reacting) parts. The analytical difficulties of open cells, particularly those designed (or permitted) to have carbonate electrolytes are much more severe. Without direct experience I don't feel qualified to discuss them.

* Our maximum is >2000 eV per Pd atom - Energetics have recorded over twice this number.

** This was a point discussed by Fleischmann & Preparata at ICCF5

M.J.:
Could we try and concentrate the _whole group_'s brain power on achieving 1/, see if together with all the collective know-how and creativity and mutual criticism we are capable of unleashing we can design this irrefutable excess heat experiment we all need? It seems to me we already have some great building blocks waiting to be assembled.

Ok, depending on the original contributions which will be actually used (which can only be determined when the job's done) the author list may end up several tens of names long (alphabetical order obviously), and the Nobel prize podium may be overcrowded, so what?

E.S.:
The problem with your idea, Michel, is that two problems need to be solved. One problem is to design an experiment without significant error. This is the problem you are addressing and the one that can be and has been largely solved. The other problem is the creation of the nuclear active environment. This is presently produced by nature when she is in a good mood, which is not very often. No one knows how to make this material on purpose, although some methods are more successful than others. Anyone who figures this out will have caught the brass ring and will obtain a patent before discussing the method. Consequently, your suggestion will not be followed.

L.K. (7/19/06):
1) I still think that this "irrefutable excess heat experiment" might be based on a Mizuno-type cell. Why? Because 50-100 W is very impressive. The most challenging task, for the moment, is to convince ourselves, and others, that excess heat cannot possibly be due to well known chemical reactions.

2) I would be happy to see Mizuno alone on that podium. Or Fleischmann+ Pons, or anyone else who made an essential contribution. The most important thing is to end the unjustified discrimination against the CMNS field. We are not pseudo-scientists; our motivation is to contribute to scientific and technological progress.

M.J.:
I too think the magnitude of the effect makes GDPE the best candidate, but Ed doesn't, mainly because, as often discussed here, it is difficult/dangerous to close the system as would be required for a total chemical balance. . . .

E.S.: (7/20/06)
Michel, I have been designing experiments in an attempt to eliminate significant error for the last 17 years in this field. I have designed and built most of the calorimeter types used in this field. Even if I say so myself, I probably know more about calorimeter error than anyone in the field, except perhaps Mike McKubre. I now have two very sensitive and very error-free Seebeck calorimeters. So, your suggestion has been implemented. The problem is to find a sample that actually makes heat.

L.K (not posted).;
Why is this a problem? If I had a working F&P-type cell I would certainly bring it to you. Most researchers would probably be very happy to cooperate with you.

E.S.:
1) [L.K., referring to W/cm^3] asked: “Does this mean that arguments presented by Bockris and his coworkers (in the paper you recommended) should now be rejected?” The use of the term W/cm3 and the Bockris arguments have no relationship to each other. W/cm3 is a method to normalize the power to the volume of the sample, as I said. The Bockris arguments apply to the energy being produced by chemical processes.

2) [L.K. wrote about eV/atom.] “ . . . The ratio turns out to be much larger than 1000 eV/atom. What is wrong with saying "a chemical reaction cannot possibly produce such result?"

You are assuming there is a relationship between the amount of metal removed from the cathode and the amount of energy produced. In fact, the energy was produced by a nuclear reaction and material was lost by a chemical-mechanical process that is completely independent of the nuclear reaction. The absolute amount of energy is the only important value.

L.K.
1) OK, I understand E.S.. Division by a constant number does not change the ratio of two heats (chemical/excess). The answer is in the ratio, not in power densities.

2) Concerning “The absolute amount of energy is the only important value.” That depends on the context. My purpose was to rule out a possibility that excess heat might be chemical. In that context I assumed that tungsten is a chemical fuel. This assumption cannot possibly be defended because the eV/atom are too high. What is wrong with this method of reasoning? Yes, I know the same should be done for any other chemical substance whose mass was reduced considerably during the experiment. Chemists in Paris will probably do this. I suspect that eV per atom of consumed potassium would also not be consistent that K is a chemical fuel. W and K are probably the only substances whose masses are reduced significantly when excess heat is measured. But that must be verified by chemists. Our Colorado2 appendix would be sufficiently convincing if masses lost are determined and if eV/atom are shown to be excessive for every potential chemical fuel. Will this be done in Fauvarque's lab? I hope so.

J.D.
Gentlemen, there is obviously something wrong in your calculation of the energy of reaction per atom. I assume the cathode is palladium (atomic weight : 106,42). So the number of mole involved is 5/106,42 = 0,047 mole. This gives an enthalpy of reaction of some 50000/0,047 = 1064000 J/mole (1064 kJ/mole which is close to the upper limit for chemical reactions). If you want the figure in eV per atom, then number of atom 0,047*6*1E23 = 2.82*1E23. Energy en eV = 50000/1.6*1E-19 Hence energy in eV per atom : 11,08 eV.

L.K.
I wrote: ". . . Assume that 15 grams of the cathodic metal is found at the bottom of the cell. That would mean that only 5 grams of the cathode reacted. " I have no idea how much metallic W was really found at the bottom of the cell by Pierre. 5 grams was purely hypothetical. **I am sorry for not making this clear.** The only numerical calculation performed was 20-15=5. The rest was speculation.

If 5 grams leads to 11 eV/atom, as calculated by Jacques, then 0.05 grams would lead to 1100 eV/atom. According to Mizuno, if my memory can be trusted, 99.99% of lost tungsten was recovered as metal, at the bottom of the cell. The number 1000 eV/atom was posted by Mike; that is why I wrote that 1000 eV/atom would be highly convincing. The main point was to show that we do have ammunition to argue that the COP = 1.24 was not due to chemical processes. It is a matter of measuring, or estimating, masses of lost chemical substances. I hope this path will be followed.

E.S.
Ludwik Kowalski wrote: “That depends on the context. My purpose was to rule out a possibility that excess heat might be chemical.” Yes, I understand the purpose. You need to show that the TOTAL amount of energy is greater than the sum TOTAL of all possible chemical reactions producing heat. This means you need to identify all chemical reactants and products, and determine their quantity. This is relatively easy because only a few reactants are present and their reaction products are well known, or at least can be easily determined. You don't even need to speculate about what is made. This is normal, well understood chemistry. Forget about eV/atom. This has no meaning unless it is associated

with a demonstrated chemical reaction. For example, you might make a little WO3 and when this is divided into a lot of heat the result is a big ev/atom value for that particular ratio. But this means nothing because the actual heat producing reaction might generate many atoms you have not identified.

L.K. (not posted):
I still think that the only chemical substances to be considered, for the eV/atom, are those that were present before the experiment, and whose masses were reduced considerably. The WO_3 is the intermediate product. But all this makes sense only in the context of ruling out chemical origin of excess heat. Heats of combustion of most fuels are below 5 eV per atom, or per molecule. That is why numbers like 1000 eV/at should be convincing.

Let me show another exchange of messages that appeared on the CMNS yesterday. They show that list members are aware of people who might be tempted to exploit the existing controversy and promote nonscientific agendas. Most CMNS researchers do not want to be associated with such agendas.

L.K.:
Am I the only person to receive this message? It came this morning.

Dear Sir,

Glad to see your article. Now we have some news and I hope you'll be interested. Our web site is <http://www.faraday.ru>

Moller and Naudin are wrong way. Best regards, Alexander V. Frolov Faraday Lab Ltd.

Who is Frolov? I do not remember communicating with him before. But I clicked on the above link and read about two commercially available products. Very impressive, if true. Comments and information will be appreciated.

M.J.
I heard of Frolov in the field of "Lifters" (there was talk of a "Frolov hat" device supposedly exhibiting "reactionless" forces or something of the kind), as far as I can tell he is at least as much "wrong way" as the competitors he denigrates. I suggest to wait until he actually delivers what he promises before buying any "fuel less" electricity generator from him :) You could also try a Google search . . .

William Beaty (W.B.):
A. Frolov is a longtime member of the fringe science community. In Russia in ?1996? he organized a conference on CF, antigravity, "torsion" and other fringe physics. My overall impression: he is not a scientist, he lacks the brutal self-honesty required.

Re. the products. The one on the left looks suspiciously like the "Potapov device," a water heater based upon a pump driven by an electric motor. It was claimed to put out more thermal watts than the motor watts input. Potapov brought one to the USA for informal testing, and it only gave normal results with no anomalous excess output. Yet his company was selling them in Russia without being able to support their claims that it was better than a simple electric water heater. We never decided if Potapov was running a scam, or if he'd fallen into "pathological science" and was honestly fooling himself. And that Frolov is selling a perhaps-fradulent device doesn't surprise me. . . .

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305) Science or Proto-science ?

Ludwik Kowalski; 7/28/2006
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Results from interesting experiments were recently described in Physical Review Letters (PRL **96,** 034301, January, 2006). The paper, "Nuclear Emissions During Self-Nucleated Acoustic Cavitation;" was published by R. P. Taleyarkhan et al. The six authors, affiliated with four institutions, observed nuclear particles emitted from a small jar with a liquid. They believe that what was observed was ordinary hot fusion; the main argument for this was emission of 2.45 MeV neutrons, gamma rays and generation of tritium. The neutron emission rate is reported as between 5000 and 7000 per second. That is indeed much more than in the normal background due to cosmic rays.

The fact that hot fusion can be induced in a small jar, as opposed to a large tokamac, is indeed remarkable. But the jar was not an electrolytic cell, the authors do not claim that what they studied was cold fusion. They think that stellar temperatures are created in tiny cavitational bubbles containing deuterons. The test cell -- height ~15 cm and diameter ~8 cm -- contained carefully selected mixtures of liquid, such as deuterated benzene or deuterated acetone. A standing ultrasonic wave (maximum amplitude of 15 bars) was established in the jar by using a lead-zirconate-titanate piezoelectric driver ring. A small amount of natural uranium nitrate was dissolved in the liquid to induce generation of desirable bubbles. Nuclear products were observed in liquids containing deuterium but not in liquids containing common hydrogen.

Here is how this was described in the paper: "Results of control experiments with a C_6 H_6-C_2Cl_4-C_3H $_6$O-UN mixture indicated [9] that there was no statistically significant net change in counts between cavitation on and off. In contrast, the results of experiments with a deuterated C_6D_6-C $_2Cl_4$-C_3D_6O-UN mixture produced a significant increase (400%) in neutron counts and 100% increase of ray counts as seen in Fig. 2 and in Table I. [UN stands for the uranium nitrate.] Based on the calibrated detector efficiency, the neutron emission rate was estimated to be 5000–7000 per second." Experiments with H_2O and D_2O liquids, containing UN, showed no sign of unexpected nuclear activity under otherwise identical conditions.

Similar conclusion was reached by using the CR-39 detectors. One year ago, while working on the Oriani effect, I did place CR-39 detectors close to a Pu-Be neutron source. And I saw smaller tracks due to recoiling protons and occasional two to three times larger tracks, probably due to alpha particles from the (n,a) reactions in the CR-39. I also noticed that the number of tracks increased with etching time. My interpretation of this effect was simple, most latent tracks due neutrons do not begin at the surface, as latent tracks due to external alpha particles. The longer one etches, up to a limit, the more latent tracks are exposed to the etching fluid. That should rule out a possibility that tracks are due to a surface contamination, for example, from fingers that were in contact with a liquid containing uranium nitrate.

Implosion of tiny spherical bubbles are accompanied by emission of intense pulses light; for that reason the phenomenon has been named sono-luminnecsence (SL). How reproducible are SL experiments? That question was answered in another publication of Taleyarkhan et al. -- " Supplement (E-PRLTAO-96-019605) to 'Nuclear Emissions During Self Nucleated Acoustic Cavitation')." They wrote: "

Nuclear emissions with cavitation for a deuterated liquid can vary significantly and it is not uncommon to get null

results on a given day if the test cell produced non-spherical and comet-like bubble clusters. The precise reasons behind the absence of recorded nuclear emissions is unknown at present but, as mentioned before, it appears to be closely tied with the behavior of the bubble clusters themselves. Spherical cluster implosions tend to result in nuclear emissions, whereas, non-spherical clusters (especially those tending to comet-like shapes) do not. Nevertheless, nuclear emissions were never noted for control experiments (i.e., with non-deuterated liquids) and also for experiments with water. For unsuccessful campaigns on any given day the test cell operation would result in comet-like bubble streamer formations which would tend to persist despite operation for several hours. This required dismantling of the test reactor to seal leakage pathways, and to filter the test liquid for impurities, or, for persistent problems it was necessary to use a new batch of freshly-drawn test liquids."

That is another similarity between the SL and CMNS; the first similarity being generation of nuclear reactions in a small jar. The issue of reproducibility in CMNS has often been addressed by Ed. Storms. He thinks that the so-called "Nuclear Active Environment" (NAE) has not yet been identified. Here is how this was expressed a message Storms posted at the CMNS list (7/23/06). It was a reply to a message asking which CMNS is the most reproducible.

Ed. Storms wrote:
"As I have been harping on for sometime, the method is not as important as being able to form the NAE on the cathode surface. All of the methods have produced heat on occasion. X1 has claimed co-deposition is completely reproducible, but I have not found this to be true. X2 claims use of a PdB alloy is very reproducible, but this alloy is not available. x3 claims palladium Type A is reproducible, but this is not available and its characteristics are unknown. X4 claims his method is very reproducible, but it has not been described in enough detail to be duplicated. X5 claims Ti is very reproducible, but I have not found this to be true. He also claims placing Ti in the electrolyte improves the behavior of Pd, which I have not found to be true. I have found methods to form a NAE that work on occasion, but then fail to work for unknown reasons. In other words, Scott, you are asking the wrong question. The proper question is, how can the NAE be formed with reproducible results? The answer is, we do no yet know. Whatever you try will suffer the random consequences as experienced by everyone else. If any of us know the solution to this problem, we would be running successful experiments, making demonstrations, and getting rich."

Ludwik Kowalski wrote:
In my opinion, a field of research that does not offer a single truly reproducible demonstration cannot be called scientific. That is why think that the CMNS, and SL are examples of proto-science. My immediate reply to the above was "In other words, CMNS should be replaced by CMNP, where P stands for proto-science. Right or wrong?" Then I added: Here is the excerpt from:

http://en.wikipedia.org/wiki/Protoscience#Historical_perspective

"Proto-science is a word with two meanings. It may mean an unscientific field of study which later becomes a science (e.g., astrology becoming astronomy and alchemy becoming chemistry). Or, it may mean a field of study which appears to conform to the scientific method but is either not falsifiable, or if it is, its predictions and principles have not yet been accepted as science or verified by a consensus of scientists."

By my own definition: an experimental field is proto-scientific when scientific methodology of validation is applied but reproducible experiments are not available. I believe that a single reproducible-on-demand demo will turn CMNS into science. That is what Michel Jullian and Scott Little are asking for. Right? And here is an excerpt about pseuo-science, from

http://en.wikipedia.org/wiki/Pseudoscience#Introduction

"What is pseudo-scince? **Pseudoscience** is a term applied to a body of knowledge, methodology, or practice that diverges from the usual standards required for scientific work, or which is unsupported by sufficient, substantial or verifiable scientific evidence and research .....The standards for determining whether a body of alleged knowledge, methodology, field, belief, or practice is truly scientific can vary from field to field, but involve agreed-on principles including reproducibility and intersubjective verifiability. ....."

Scott Little wrote:
"At this point, we would be delighted to simply have the cold fusion "experience". With one good, solid excess heat experiment under our belt, it would change the whole landscape for us. We've been monitoring the cold fusion field every since its inception. I had my first cold fusion experiment running within an month of the original F&P announcement. I attended the first cold fusion conference held in Santa Fe NM and met Ed Storms there for the first time. Over the years, we've constructed a dozen different calorimeter systems for cold fusion research. We've tried a variety of experiment including many of the newsworthy cold fusion ideas that have surfaced, like the Patterson beads, the Case experiment, Stringham's sonofusion, Mizuno, etc. 'Proof' is too much to ask for right now. We just want to observe the excess heat effect."

Ed Storms wrote:
"I want to make sure no one thinks I believe Szpak, Dash, etc. are untruthful and I'm amazed Swartz would come to this conclusion. What I said was that I have not been able to duplicate the claims. This problem is universal in the field and is an accepted part of the experience without hinting that someone did something wrong. We simply do not know enough to replicate the claims. . . .

Steven Krivit wrote:
We could eliminate many arguments and unfortunate misunderstandings within this community and external to this community if we can agree to, and rigorously apply the distinction between REPEATABLE and REPRODUCIBLE.

REPEATABLE: Researcher A can obtain the expected result from his or her experiment every time he or she makes an attempt.
REPRODUCIBLE: Researcher B can obtain the same result from his or her replication of researcher A's experiment.

Ludwik Kowalski wrote:
To be useful an experiment must be reproducible; repeatability would not turn CMNS proto-science into science. A simple experiment cannot remain repeatable-but-not-reproducible for a long time, unless secrecy is involved. Sooner or later A will teach B how to be successful, or B will convince A that something is wrong. An author publishing experimental results tells others "do what I did and you will get similar results." Is this not the main purpose of publishing?

My suggestion for Scott is to replicate excess heat in a Dash-type cell. It was demonstrated by John's students at ICCF10 and everyone could repeat the experiment. Do you still believe that results are reproducible, John? Would you be willing to supply Scott with the recipe, or send him a ready-to-test cell? Scott's MOAC seems to be ideal for measuring excess heat from your kind of cell. If I had such cell I would bring it to Texas and assist Scott -- to be sure that all is done properly. The entire CMNS community would be interested in our result. John's cell was similar to that used by F&P. Therefore, ruling out a prosaic effect would be much easier than in Mizuno-type cell, as emphasized by Ed. I am recommending this because Dash-type instrument was the only low-voltage cell that I that saw delivering excess heat.

Ed Storms wrote:
[Ludwik wrote:] "In other words, CMNS should be replaced by CMNP, where P stands for proto-science. Right or wrong?"

"The problem is one of ignorance. We do not yet know enough to make the effect work every time. This has nothing to do with science. Science is the method by which ignorance is overcome. We are using science to gradually learn what it takes to solve this problem. Someone someday will discover what the NAE looks like and will apply a layer of it to an inert metal substrate, and suddenly huge amounts of heat will be produced and the issue of reproducibly will disappear."

John Dash wrote:
"Ed, Thanks for trying to reproduce our results. Possible reasons why you were not successful may be caused by differences in the details of our protocol vs. yours. In addition, We work at sea level and you work far above sea level. Does that make any difference?

In order to reply correctly, I need to have the recipe which you used in your attempt to reproduce our results. I need to know every detail from the beginning to the end of the experiment (vendor of the cathode and anode materials and recombination catalyst and and the chemicals used to make the electrolyte, including lot numbers and chemical analyses, thickness or diameter of the anode and cathode materials; also include composition of the cell body and pH of the electrolyte). I need to know the initial condition of the cathode (cold rolled, including %reduction in thickness, annealed, including temperature, time, and environment, etc.), temperature of the electrolyte at steady state, data from an experiment, and your analysis of the data. Was there any deformation of the cathode during the experiment ? Was there any difference in the surface composition of the cathode after electrolysis compared with the composition before electrolysis ? Please send me these details of your attempts to reproduce our results."

Ed. Storms wrote:
Dear John, Here is a summary of what I did to replicate the use of Ti to initiate CF using Pd and D2SO4. An electrolyte of 3 ml D2SO4(concentrated) and 30 ml D2O was used. Titanium was used as the cathode and Pt as the anode. This was electrolyzed in a closed cell using the Seebeck calorimeter as follows:

```
0.048A  for  178 min
0.100A       199
0.200A       200
0.500A        79
```

At the end of this treatment the Ti had lost about 3 mg and had turned black. The electrolyte had turned yellow. No excess energy was observed during this time. A palladium cathode was put in the cell. This was made by rolling a piece of palladium to a thickness of 0.082 mm and cleaning with acetone. No further treatment was used. This was electrolyzed as follows:

```
0.047 A for   89 min
0.500A       149
1.00A        258
1.50A        109
2.00A        109
2.5A         639
```

No excess energy was observed within ±10 mW of zero. The sample surface was examined using the SEM and was found to contain no Ti. The absence of Ti is consistent with Ti not plating from such a solution, as expected. If some aspect of this procedure is not right to make it work, please let me know and I will try again - once I finish the book.

Akito Takahashi wrote:
[Mitch] Thank you for teaching your model. Maybe we need to read your papers which are not available in downloadable e-data, on lenr.canr site and your sites. Where are downloadable ones available? One primitive comment back: We know that the biding energy (strong interaction) of deuteron is 2.22 MeV against the breaking-up to neutron plus proton. How is the so high energy excited state as 21 MeV of deuteron possible?

Ed. Storms wrote:
It has come to my attention that a comment I made about being unable to replicate the claims if Szpak and Dash has been misrepresented. I want it clearly known that my failure to make these replications in no way means that I believe Szpak or Dash are dishonest or in any way misrepresented their work. I believe these gentleman are honest and are examples of the highest integrity in the profession. Failure to replicate claims is common in this field and in no way indicates a lack of honesty or skill.

John Dash wrote:
Dear Ed, Thanks for the fast response. My comments are:
1. We use H2SO4, not D2SO4, in the electrolyte.
2. We also use a Seebeck calorimeter, and we detect excess heat from cells with Ti cathodes and also from cells using Pd cathodes.
3. The currents given are not meaningful. We need to know the current density.
4. Our Ti cathodes never turned black, nor did the electrolyte turn yellow.
4. We need to know the % reduction in thickness of the Pd.

5. I don't understand why you did not find Ti on a Ti cathode after electrolysis.

So, there are many differences in our experiments, compared with your attempts to replicate. When you finish your book, I will be pleased to work with you in another attempt at replication.

Ludwik wrote:
Ed and John recognized that the disagreement resulted from not performing the same experiment. They decided to work together to either confirm or refute reality of excess heat in John's setup. Is this not a good way of solving the controversy? What else should be done when mistakes are made in such difficult situations?

Let me return to protoscience. The concept of NAE -- nuclear active environment -- invented by Ed Storms (and by others, under different names?) -- is very peculiar. Does it belong to theories or does it belong to experiments? Theories (models of reality) are invented to explain facts. Facts are discovered via experiments activities. NAE is an unknown form of matter, it is something to be discovered in the future, perhaps a new nano structure or a new catalyst. Things that may possibly be discovered in the future should not be counted as experimental facts.

Does it mean that the concept of NAE belongs to the realm of physical science theories? I do not think so. Theoretical predictions are made either on the basis of deduction or on the basis of induction. Neither of these are part of NAE, as far as I know. Existence of NAE is based on faith in results of the not-yet-reproducible experiments. Such experiments, performed by recognized experts, are too numerous to be dismissed. Should this kind of attitude be called a physical science theory? I do not think so. NAE is not such theory. In my opinion it is an empty name, a placeholder for something that remains to be discovered.

Unlike in mathematics, a theory in a physical science is validated by showing that it agrees with valid experimental findings, that it has predictive ability, and that it is experimentally falsifiable. The NAE “theory” does not refer to particular findings, it does not have predictive abilities, and it is not falsifiable. The only prediction of the NAE “theory” is that some experimental results will become reproducible, sooner or later. And what about being falsifiable; how can the only NAE prediction shown to be wrong? Suppose that results remain irreproducible for another century. That would certainly not be a proof that the "theory" is wrong. Next suppose that a claim, for example, excess heat accompanied by accumulation of 4He, becomes verifiable on demand. That would possibly validate NAE. But that is not what is needed now, if one wants to elevate NAE to the level of a scientific theory. I cannot think of an experiment capable to falsify validity of the only NAE prediction.

I see nothing wrong with believing in NAE; but importance of this attitude should not be overemphasized. Our rules of validation are not different from those used by mainstream scientists. Like all of them, we know that being patient is a virtue. We should emphasize what our protoscience has in common with science, not what makes it unique. Overemphasizing NAE can hurt our reputation.

In principle, CMNS can remain protoscience for another century or longer. But in practice it will not survive without injection of new resources (young scientists and some financial support). It might disappear in ten or twenty years and reappear naturally much later. In that respect CMNS is not different from other protosciences.

Jim Giles discusses reproducibility in the most recent issue of Nature (July 27, 2006). But that is about science, not protoscience. Here is his opening statement: “The idea that readers should be able to replicate published scientific results is seen as the bedrock of modern science. But what if replication proves difficult or impossible? ” How should editors of leading journals, and referees, deal with undesirable consequences of irreproducibility? That was followed by an explanation of why failures to replicate are unavoidable, even in physical sciences. Several suggestions for dealing with this problem are made by the author. Unfortunately, due to discrimination, CMNS publications are deprived the scrutiny of the refereed journals.

Edmund Storms wrote:
The NAE is neither a theory nor an experiment, but the recognition that for the CF reactions to occur, a novel structure is required. This fact has been supported by every attempt to produce CF. This same concept is applied to many phenomenon, such as superconductivity. For a material to be a superconductor, a special structure is required. This

idea says nothing about what that structure must be. The idea is important in discussing the phenomenon because without this concept, many theories have been applied to the normal structure of PdD. Naturally t hese theories have no value because the normal PdD structure can not support nuclear reactions, as the skeptics are quick to point out. The concept forces people to look at special structures to find an environment that can support CF.

Ludwik Kowalski (not posted):
If NAE is neither a theory nor an experiment then what is it? In our present situation it seems to be a substitution for a recognized theory. How can a recognized theory emerge when not a single CMNS demo is reproducible on demand? Most researchers are aware that what they know is always a small fraction of what can possibly be known. That is part of general philosophy. It goes together with believes that laws of nature exist, that physical science theories are idealized models of reality, and that models should be validated by empirical evidence.

In my opinion NAE is a substitution for a recognized theory. The role of theories in science and technology is essential. We want to understand nature; collecting unrelated empirical facts is not sufficient. Several CMNS theories have been suggested but, as far as I know, none of them was declared to be a winner. So when someone asks for an explanation of a particular CMNS effect; we say it is due to a hidden effect (HE), such as NAE that must be discovered. Our protoscience will become science what its HE, such as NAE, new chemistry, etc. is discovered.

In static electricity, for example, the HE was humidity. In humid air dielectric surfaces are often covered with thin conducing layers through which discharging is much more rapid than it is when the layers are not present. Physics teachers are advised to keep electrostatic demonstration apparatus in dry environments (hotboxes). In that way they can be sure of high reproducibility. Hot air from a blow-dryer is often used to remove undesirable layers on dielectric surfaces.

Post Scriptum:
On a list for physics teachers I posted this message: “When was the the effect of humidity recognized as a factor influencing electrostatic demonstrations? I suspect that Ben Franklin was already familiar with the effect of humidity. But I am not sure. Who was the first to write about this?” Here is a reply from one teacher: “Probably Desaguliers [1683=> 1744]. However, Wm. Watson [1715=>1787], ... explained more clearly than Desaguliers had done, that atmospheric moisture destroyed electricity by conduction " I couldn't find the reference, only the quote above. “The leak derives primarily through surface conductivity promoted by the moisture, not from loss to the air, a point not understood until the end of the 19th century.” Ref: Conduction of Electricity through Gasses I", the Thomsons, Cambridge 1928. Quoted: Electricity in the 17th and 18th Centuries, Heilbron, J. L. UC Press (1979)

Another teacher responded: I believe this [see above] quote is from: A Collection of the Electrical Expeririments Communicated to the Royal Society by Wm. Watson, F. R. S. Read at Several Meetings between October 29. 1747. and Jan. 21. Following William Watson Philosophical Transactions (1683-1775), Vol. 45. (1748), pp. 49-120.

Ed. Storms wrote (referring to Giles’ article in Nature):
This is a very interesting article, but it does not address the situation we face in the CF field. The CF or LENR phenomenon has been replicated many times by laboratories all over the world. By the standards described in the article, CF should have been accepted years ago. The problem we have is the difficulty in making the effect work every time and in a predictable way. This is entirely different from the problem of replication. By continuing to describe the problem as being one of replication, we keep the skeptical attitude alive by using their vocabulary. We should call the problem what it is, i.e. a difficulty in making the effect easy to produce.

Ludwik Kowalski (not posted):
How does the ‘problem of replication’ differ from the ‘difficulty in making the effect easy to produce’?” According to the article, problems of replications result from the limited ability to describe experimental protocols in refereed journals. Our difficulty in making the effect easy to produce, on the other hand, is due to not having the protocol. That is how I understand the last message of Ed (see above). I do not share his opinion that avoiding the term replication, in favor of difficultiy, will help us. Replication -- the bedrock of science, is expected from us and we should keep trying to deliver it. Nobody is asking at high precision at this stage.

The underlying assumption of science is that macroscopic phenomena are reproducible, under identical conditions. But conditions are never exactly the same. That is why very precise replications are impossible. The best one can do to overcome precision limitations, in any laboratory, is to calculate averages and to assume that they are close enough to true values. Limited precision did not interfere with scientific progress, or with technological applications of science. The problem has to do with unpredictable, and large, fluctuations of accuracy, not precision. By the way, the term accuracy usually refers to systematic errors while the term precision refers to random errors. I am imagining an instrument whose calibration constant is significantly influenced by solar flares or by coincidences of other rare events. Using such instrument would be like trying to win in a game designed to tease losers. Expecting reproducibility of CMNS effects would not be consistent with chaotic fluctuations of essential parameters.

Michel Jullian wrote:
Michael, if the difficulty of replication is too many unknown unknowns, which makes sense, couldn't the _complete_ original working cell, or at least one replicated by researcher A himself, be lent to researcher B to save time?

Mike McKubre wrote (7/29/06):
That is how we usually begin; the original equipment set up and operated by the original experimenter(s). In this first Phase we just provide technical and plumbing support. When the experiment is running to the satisfaction of the originator - and we believe we understand what is being done - we invite the originator to leave operation to us. Even at this stage it is quite usual for us to seek further help. Only when the experiment is running for us in the same way as for the originator do we allow ourselves to succumb to the siren urge to "improve".

In terms of unknown unknowns it is interesting that recent discussion in this thread has centered on Huggins and Mengoli. At the time of their successes both of these men were at the top of their respective professions (material science and electrochemistry). They knew more than (I would timidly suggest) anyone who attempted their replication - that is, fewer things were unknown to them. At least one of these things might have been important.

As an example look at Huggins repeat melting that Charles started us with, and the issue of lithium (and beating). Ed assumed in his attempt that Huggins' repeat melting was to remove Li. This is not what I recall from my discussions with Bob at the time, but who knows? My point is that we don't know what we don't know. This applies to the originator as well. So you must start with the same equipment, the same procedures in the same hands. This is the beginning of replication.

Ed. Storms wrote:
What was your understanding, Mike, about what Huggins intended to do? My understanding was that the crucible was the only piece if Pd Huggins had easily available. They arc melted it to get it into a form they could use and to purify it. Arc melting will remove any volatile such as Li, C and O. He pounded the button onto a flat sheet because an arc melted button does no have much area and is hard to use, as we discovered. At that time, I don't believe anyone knew what would work best. As you and others discovered, annealing is beneficial, but Huggins did not do this.

As for replicating exactly what a person has done, I find that replicating exactly what I have done in the past does not always result in success. Obviously variables are present about which we have no knowledge and over which we have no control. On the other hand, when I successfully replicated the Letts laser experiment, I prepared my Pd in an entirely different way. It turns out that the nature of the Pd is not important. What is important is the nature of the gold plate. It turns out gold can plate in many different ways depending on subtle conditions, most of which Dennis did not control or even know about. Consequently, he and I were both lucky to make the effect work at all, something neither of us has been able to do again. We are chasing ghosts until someone identifies the important variables and the important conditions that must be present.

Ludwik Kowalski (not posted):
The debate started in 1989 is going on. On what basis are the authors of these messages accused of being pseudo-scientists? They are addressing a difficult problem. In my opinion their methods of validation are 100% scientific. They perform experiments, they discuss results and they develop theories, like in other areas of physical science. But our scientific establishment is practicing an unfair discrimination against the CMNS field. When will this end? Yes, it

is not the first time that I am asking this question. Discrimination against honest and dedicated scientists is totally unjustified.

Ludwik Kowalski (7/30/06 in a thread about systematic errors):
1) I agree with a possibility of that kind of error. One way to check the reliability of the sampling method would be to use a bomb calorimeter, as suggested by Jed. Instead of a Mizuno-type cell (which is expected to produce excess heat) one could use a sparking current interrupter, as in an old door bell, or in a Tesla coil. This setup is not expected to generate excess energy and electrical power is expected to be exactly the same as thermal power.

Suppose the heat capacity of the calorimeter is 1000 J/deg. Suppose the experiment lasted 300 s and the calorimeter temperature increased by 30 C. Then we would know that thermal energy was 1000*30=30000 J, and that the mean thermal power was 30000/300=100 W. The sampling method, used at the same time, should confirm this result. But suppose that the sampling method gives 50 W, instead of 100W, as speculated by Michel. That would show that he is correct; the electric power obtained by sampling would be declared to be strongly underestimated (by the factor of two in this illustration). . . .

2)Scott's attempts to replicate Mizuno's GDPE results are significant in the context of this discussion. His COPs were consistently very close to 1.00. That can be explained by saying that Scott's cells had no NAE in them. Are his results not indicative that the method of sampling is reliable for Mizuno-type cell frequencies? Yes, I know that the COP=1.00 can be purely coincidental. For example, a little bit of NAE perfectly masked by a systematic error in measuring electric energy. But i do not believe that this was the case.

Ed. Storms wrote:
May I suggest you all are beating a dead horse. Eliminating errors in measuring applied power is easy and can be solved three different ways. The first, as observed by Ludwik, if some cells produce a COP of 1.0, this is a good indication that applied power is correct. Although not all cells are identical, they won't be different in this regard if the same power supply and detection equipment are used. Second, if you sample using a low rate, then use a high rate and see no difference, the values can be taken as correct. This can be checked at any time. Third, if calibration is made using a dead cathode, the sampling errors will cancel out and be eliminated as an issue. With too much emphases on trivial issues, the real issues are overlooked. The real question is, what does it take to form the NAE?

Ludwik Kowalski:
1) I like the idea of using “dead cathodes” in control experiments. But this does not apply in Mizuno-type experiments. In these experiments cathodes are destroyed. Solid tungsten rods are turned into tiny metallic particles or compounds (?).

2) I do not think we are beating a dead horse. If submitted, the Colorado2 paper will be scrutinized in terms of possibilities of experimental errors, not in terms something that might possibly be discovered later. We do not speculate about the mechanism by which excess heat is generated in a Mizuno-type cell. We only argue that the COP>1 is not due to a systematic error, or to a well known effect. Even this seems to be more difficult than I expected.

3) May I suggest that the term Nuclear Active Environment be replaced by something more acceptable to mainstream scientists. Suppose we say that NAE stands for New Active Environment or Not Anticipated Environment (or something like this but better -- please suggest better alternatives). Such replacement would be needed for Mizuno-type cells; we have no evidence that their excess heat is due to a nuclear process. A paragraph about NAE (but without the word “nuclear”) is worth adding at the end of our paper.

Ed. Storms wrote:
. . . .You [Ludwik] are playing into the attitude of the skeptics here. We need to stop doing this. Cold fusion is based on initiating nuclear reactions. This fact can not be hidden. If the Mizuno cell does not make nuclear energy, then it is not cold fusion and it does not involve the NAE. Why confuse the issue just to satisfy a few skeptics who will not be influenced no matter what you call the effect.

Ludwik Kowalski (7/31/06, not posted):
Ed wrote: "Cold fusion is based on initiating nuclear reactions. This fact can not be hidden." It is not a fact; it is a statment what cold fusion is. The rest of the message is also not acceptable. The main point is that Mizuno does seem to produce excess enegy. But we have no evidence that a nuclear process is involved. It can be something else very interesting. That is why the word "nuclear" is not appropriate, at least at this time. And I do not believe that all skeptics are dishonest.

Appended later

Several short messages about NAE were posted today (8/10/06) on the CMNS list. I think they are worth adding. The concept of NAE is interesting but strange. Nobody knows what NAE is but the concept is used as if reality of NAE has already been established. When something unexplained is discovered we know that it can be explained, sooner or later, in terms of something else. That something else is a cause of the event. In my opinion NAE (Nuclear Active Environment) is nothing else but a synonym for the word "cause." Believing that NAE exists is like believing that a cause exists for an unexplained phenomenon.

Peter Gluck:
The secret of NAE is -- probably -- in such studies:
http://www.gatech.edu/news-room/release.php?id=1078

Ludwik Kowalski
What is the basis for such expectation? Is it because distances between ions become shorter? Is it because screening is expected to be much stronger? My understanding is that NAE is a word invented in anticipation of something totally different from what is known.

Edmund Storms:
I agree, the features of nanostructures and their subtle methods of formation would fit the experience of trying to make CF work. As for NAE, the concept is only used to describe a general condition, both known and unknown. This is used just like the word "tree" is used as a general concept, which is applied to many different items of the same general type.

Peter Gluck:
I think that Les Case's method- was the most promising CMNS achievement- in a form very adequate for a real energy technology, but it was not studied and developed with sufficient forces and ideas. It was poisoned and forgotten. Nanotechnology is the key- but this is only a slogan today. CMNS is a special form of catalysis- I bet, therefore this paper is valuable.

Ed. Storms:
I worked very hard trying to duplicate the Case method. I understand even Case can not replicate what he had done. Formation of the naoometer-sized particles on carbon is the problem. This was done using the conventional methods for making chemical catalysts. We need methods to achieve the same end, but without the many variables associated with the catalytic method. Some day money and interest will be available to allow a marriage between CF and the many methods now being developed to apply small particles. The child of such a marriage will save the world.

Ed Storms:
Well Peter, I have identified 9 possible candidates for the NAE. With sufficient money, these possibilities could be easily explored and the real NAE identified. The tools and knowledge are available. All that is lacking is the will and the money.

Ludwik Kowalski (referring to Ed.'s first message above):
"Tree" is probably not the best analogy for the "NAE." What about the "EOL" (Elixer of Life). It also was a word reserved for something to be discovered much later. I am thinking about antibiotics and other effective drugs. Ancients believed that such substance will be found, sooner or later.

Ed. Storms:
I agree, EOL is a better analogy. However, we already know a NAE exists. We just don't know what it looks like.

Peret Gluck
The ideal solution would be a palladiumless NAE, made from cheap and abundant materials, delivering "high currency" energy -- hot steam and not warm water.

Jean Paul Biberian:
NAE is very similar to catalysis where people talk about "active sites". After years and years of research with many scientists and lot of money we still don't know exactly what it is. We know how to manufacture catalysts that work, but without knowing precisely how they work.

Cold fusion is very similar to catalysis, in both cases thermodynamics is favorable, the only obstacle is the energy barrier. The only difference is the many orders of magnitude larger barrier height for cold fusion. In catalysis as well as in Cold Fusion we don't know if the active site is localized or de-localized? What is the role of impurities? What kills the catalyst? I am pretty sure that soon enough we'll discover ways to have a reliable device, however, the understanding of the phenomenon might take lot longer.

By the way, Arata's system: a mixture of nano-particles of palladium embedded in zirconium oxide seems promising and reproducible. SRI has duplicated the experiment years ago with the double cathode system. His new design with gas loading alone is even more convincing.

Peter Gluck:
Do you think that we still do not have sufficient experimental and theoretical data in order to develop a strategy for trying to discover the secret of NAE? If yes, what is actually missing?

Peter Gluck:
I think EOL is a "metaphor too far" NAE describes the new function -- triggering nuclear reactions via a special electronic environment. Simple logic says this is possible only via neutral entities- not by forcing nuclei through the Coulomb Barrier but this is the job of theorists. I am convinced that (excuse me for using the old name of our field): "Hot Fusion is force brute, while Cold Fusion is a very smart nuclear ju-jitsu" It seems tooooo smart!

Jacques Dufour:
I fully agree with you. I worked on catalysts for ten years and I can tell you that the basic principles on how a catalyst works are perfectly well known. What is still a kind of an art is : how can you design a multifunction catalytic system (for instance find an hydrogenation catalyst that can at the same time withdraw nitrogen compounds while standing unaffected in the presence of sulfur (I worked 4 year on that kind of system!). The only problem is that these systems involve too many parameters to be correctly modelized and trial and error experiments are needed. Moreover the detailed reaction paths of certain systems would be too long to be studied scientifically and an empiric approach is more efficient from an industrial point of view.I also worked during my carrier in ore separation by flottation and the situation is exactly the same.In both domains the relevant phenomena underlying the industrial realisation are perfectly known and understood.

Ludwik, I suggest a new acronym for NAE. I propose NYUP for Not Yet Uncovered Phenomenon. As long as this new phenomenon is not indentified and scientifically studied, all the rest is just mondaine discussion (not to use the term bla-bla !). Trying to persuade people, that making so call CF work, is only a question of money and number of people working in the field, is not far from being borderline to intellectual dishonesty.

Ed. Storms:
I find this debate about what to call the environment in which CF occurs a distraction from the important idea. It makes no difference what the environment is called. The only idea of importance is the recognition that such a special environment exists. This idea is in contrast to the original thought of F&P that the entire palladium cathode was involved, being limited only by the amount of deuterium present. The word does not imply any thing about the

environment, either known or unknown. It does not imply that the effect occurs on the surface or in the bulk. It does not imply that the mechanism involves resonance or magic. You are free to say that the NAE is unknown, unexplored, unknowable and anything you like. The word only allows a person to speak about a concept, just like the word "tree" allows a person to speak about a concept without saying anything about the kind of tree. As for my suggestion that more money and people are needed to solve the problem, I ask Jacques what he thinks is needed that would not be dishonest?

Jacques Dufour (8/13/06):
The name is very important and not distraction. When you say NAE (standing, as I understand for Nuclear Active Environment), you say, with no possible discussion, contradiction, confrontation, that the phenomenon is nuclear. As far as I am concerned, the certitudes I have are :

1/ there is something in the field and the main phenomenon is thermal (anomalous enthalpy of reaction in certain systems metal/hydrides)
2/ some very weak nuclear signatures are observed (tritium, neutrons, helium4) but they don't explain the bulk of the phenomenon

Hence, the strategy for me is to present an experiment that a physicist will accept (small, with minimum matter, using the more robust equipment you can imagine, eliminating all energy storage problems, measurement problems and so on). In short a really irrefutable experiments (by the way, this was suggested by some reviwer of the DOE survey). At that stage a potential industrial application is of no interrest .It is better to have Pout/Pin=110% with an irrefutable experiment and a few mg of matter, than Pout/pin = 130% with hundred watts, that a physicist can easily prove doubtfull. A working hypothesis can also be presented, with the objective of being confronted to experiments and to all theoretical knowledge available to day (I am speaking of theoretical knowledge of the main stream physicists, not of those who completely mis-interpret something as basic as the Mossbauer effect).

Then I came to intellectual dishonesty. My reaction was triggered by what Jean-Paul [Biberian] wrote about catalysis. Having worked for some 15 years in the field (see my answer to Ludwik), I can tell you that the basic principles are fully understood. Trying to establish a parallel between what is presented as an empirical art only i.e catalysis (which is far from being the case) and the need for money an manpower to generate NAE based on a very controversial interpretation of what I think is a real phenomenon, is not only really exagerated but also very likely to be counterproductive, in terms of getting this new phenomenon accepted and its research financed (which I hope is our common goal).

So, sorry Ed, but I think it very important to name NYUP [not yet uncovered phenomenon] what we are studying, even if the phenomenon occurs as you think (and I think you are right) not in the bulk but in special sites. And we have to fight on the ground of anomalous enthalpies of reaction and not on the ground of very hypothetical nuclear reactions.

I hope we can discuss all that soon and pardon me to have use the term dishonesty (but we clearly have to challenge each other in this community and accept all contradictions : otherwise we shall be worse than what we criticize in the main scientific community ...)

Ed. Storms:
Jacques DUFOUR wrote: “The name is very important and not distraction. When you say NAE . . . but they don't explain the bulk of the phenomenon” Here we have a difference of opinion. My opinion is based on my complete study of the literature and the arguments I have made in the past and will make in greater detail in my book. Consequently, I'm looking for a NAE. If you are looking for something else, then you will have to call it something else and justify the name as I have done with the NAE.
[He also wrote] “Hence, the strategy for me is . . . as the Mossbauer effect).” I agree this is needed. However, from my reading of the literature such experiments have been done as well as they can be done using the tools presently available. Nevertheless, I wish you success.
[He also wrote] “Then I came to intellectual dishonesty. ..... our common goal)” Please do not confuse your personal opinion about what is happening with universal reality. I agree, you are seeing a real behavior. The issue is whether it has any relationship to what other people are seeing, which is called cold fusion. Please try to avoid making your

experience apply to every other observation. You may be observing an entirely different effect.
[He also wrote] “So, sorry Ed, but I think . . . hypothetical nuclear reactions.“ This conclusion would only be true if nuclear reactions were not occurring. However, a huge collection of evidence shows that they are. They may not be occurring in your work, but they do occur in other studies. My definition applies only to conditions where they do occur. You are free to define your studies however you see fit.
[He also wrote] “I hope we can discuss all . . . the main scientific community ...)” No problem. However, we all need to make a clear distinction between dishonesty and a difference of opinion.

Ludwik Kowalski:
My recollection is that only Mizuno reported on presence of transmutation products in GDPE cells. Is this correct? How large would the energy/atom be if the excess energy he measured were due to transmutations he reported? My guess is that it would exceed millions of MeV/atom. Note that that 23 MeV/atom was reported for the F&P-type cells. Unfortunately, I do not have information on the number of atoms produced while excess energy was measured by Mizuno*. I think that N -- in NAE -- is not yet justified, as far as Mizuno-type cells are concerned. Why don't we hear from Mizuno? Is he OK? I know that he was scheduled for an important operation after the ICCF12. Did someone communicate with him this year? I am worrying.

*below 0.00001 MeV/reaction --> chemical
up to 200 MeV/reaction --> known nuclear
above 200 MeV/ reaction --> totally unknown

Talbot Chubb:
Biberian writes, "By the way, Arata's system . . . “ : I call Arata and Zhang's ZrO2 + nano-palladium powder an "oxide-coated nano-metal composite". The NAE in this process would seem to be either the nano-metal Pd crystals or the interface layers between ZrO2 crystal and the Pd metal.

A-Z have shown that oxide-coated ZrO2, nano-Pd composites absorb hydrogen so as to create the remarkably hydrogen rich hydride PdH3, using what appears to be equilibrium chemistry at 100 bar. Their 2002 electrolysis test run produced ~ 10 W of excess heat continuously for over 20 days using a few grams of Pd. This technology seems to solve the damaging problem of nano-crystal growth, which occurs when metal nano-crystals make contact with each other. The A-Z ICCF12 paper suggests cold fusion heat production at 200 deg C without electrolysis.

Scott Little (8/16/06): Is it possible to formulate the hypothesis for cold fusion as a falsifiable one? The concept of falsifiable hypotheses was emphasized by Karl Popper who claimed that unless a hypothesis was falsifiable, it was not a scientific hypothesis. Not everyone shares his viewpoint (see Criticism section in this article)
http://en.wikipedia.org/wiki/Falsifiability

but, as an experimentalist, I find it rather attractive. The simple hypothesis I have been assuming for cold fusion goes something like this: "Nuclear reactions can occur in an electrolysis cell." Clearly this hypothesis is not falsifiable. Only an infinite number of null experiments will falsify it. Can anyone formulate the cold fusion hypothesis so that it is falsifiable?

Ludwik Kowalski:
It is important to emphasize that the concept of falsifiability was develop for theories. The corresponding concept for experimental data is reproducibility. [A hypothesis is often the first step toward a theory. The statement formulated by Scott would become falsifiable if conditions under which a specific nuclear reaction, such as production of alpaha particles, were said to take place under specified conditions in a clearly described kind of cell. But ths is possible only when experiments are reproducible.]

This website contains other cold fusion items.
Click to see the list of links

This website contains other cold fusion items.
Click to see the list of links

306) How to search my website for something ?

Ludwik Kowalski; 7/31/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

I never expected this website to exceed 300 webpages, as it did after about four years. The problem with so may webpages is difficulty to find something. Suppose I want to quickly find what is there about Oriani effect, or about Huizenga, or about the iesiusa company. How to find and to open pages containing such words and phrases? The same question is probably asked by some readers of my pages. The list of titles that appears when one types

http://blake.montclair.edu/~kowalskil/cf/

in the window of an Internet browser was designed to help because each link to a page has a title. But this does not always help. Fortunately, **Google can be used to search for information on a selected server.** The domain name of the server on which my webpages are located is

http://blake.montclair.edu

as indicated in the above URL. Each Internet server has a unique domain name. Suppose I want to find webpages containing the word NAE. By entering this as a keyword of the general Google search I got 12,100,000 hits. But a Google search limited to my server produced only 12 hits -- exactly what I was looking for. To restrict Google searching to a selected server proceed as follows:

a) Using a browser go to www.google.com
b) Click the "advanced search" link. This brings a page with a search box. Type the NAE (or anything else you might be searching for) into the search box.
c) Scroll down to the line labeled "domain." The right side of that line has a box for entering the domain name. Type http://blake.montclair.edu in that box.
d) Then scroll to the top of the window and click the GOOGLE SEARCH button (near the upper right corner).
e) That should produce the widow showing the search results, for example 12 links to webpages containing the desired keyword or phrase.

That is all I have to say. Other convenient restrictions can be imposed on a Google search. But this is not a book about googling.

This website contains other cold fusion items.
Click to see the list of links

This website contains other cold fusion items.
Click to see the list of links

307) More private messages about Colorado2

Ludwik Kowalski; 8/9/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

I believe that private correspondence described in unit #285 is worth preserving. It refers to Mizuno-type experiments in which I personally participated. It started as Texas1 project, became Colorado1 project, became my ICCF12 report and will possibly be participation in Paris3 project. Unfortunately, I am still not convinced that excess heat, reported in Paris1 and confirmed in Colorado2, is due to some new Physics. It may be due to a prozaic effect, such as presence tiny droplets, and colloidal particles, in wet steam. Or it can be due to errors in measuring the input energy. Will Paris3 finally convince me that what we called excess heat is really excessive? This remains to be seen.

Why do I spend time on cutting and pasting so many private messages? Because I believe that what we are doing is very important, no matter how the controversy will be resolved. Private messages provide details which might not be apparent when published reports are read. Our correspondence will certainly be appreciated by future historians of science. We are not famous scientists but our messages might become as important as private letters of Galileo, Pasteur, Fermi and Einstein. We are a small part of something significant; future generations will probably study all aspects of our activity with great interest.

= =

1) Pierre --> Richard + Ludwik (3/8/06 ?)

Before going by the end of this week with our dewar tests, we have done with Gérard a series of experiments with our traditionnal beaker, just to see if we were able to find again abnormal excess heat.
A long run (30 minutes) at 250 volts: COP between 1.05 to 1.10
2 runs at 300 volts (15 and 20 minutes): COP around 1.10 up to 1.20
2 runs at 350 volts (only 12 and 10 min due to cathode burning) COP 1.15 to 1.32
Of course, we made the usual calibration with our ohmic heater ...
We really think with Gérard that we are now able to get abnormal excess heat on demand, within, of course, a particular optimised geometry (beaker, anode and cathode)...except unknown artefact !

I told you that, as in Boulder, we failed in trying to get automatic constant length for the cathode by using gravity. Our small ceramic cup was pierced and even a second one placed on the first one. Happily, the bottom in Nickel stopped the plasma electrolysis. See on the pictures attached. But, however a good result : this cup bottom gives a very good behavior to the electrolyte (no splashing, no waves, no foam...)

As asked by Scott, you will find a small movie giving, I hope, plasma colour and sound of our CFR running (I think here at 300 volts). We will get by the end of the week the Boulder electrolyte analysis...

= =

2) Pierre --> Richard + Ludwik

We tested yesterday our dewar by beginning the traditionnal calibration with our ohmic heater. We got the following values:

At 600 watts: mean value for thermal losses: 40 watts
At 400 watts: --- --- 40 watts
At 300 watts: ----------29 watts

At 200 watts -----------------------------21 watts

Compared to the same tests with our beaker, these values are about one third of the beaker values...

Then, we replaced the ohmic heater by our usual electrodes and we made one test at 300 volts during 20 minutes. We have obtained COP values around 1.15 from time 5mn to time 15 mn and even 1.30 at the end of the test (Pth was about 386 watts at time 20 mn).

We tried then several tests at 350 volts. We got good COP (even> 1.3), but we had splashing. .. The main reason is that, with a beaker, it is very difficult to adjust the length of the active cathode and to see at what depth you really placed the electrodes. A transparent dewar would be a good answer to this problem...

But anyway, we think with Gérard that we have found same results with the dewar and with the beaker. We prefer the beaker, easier for the setting up, and also better than the dewar because we can place together the electrodes and the ohmic heater, due to larger diameter for the beaker...

Three pictures are joined. I hope by to morrow to have some chemistry analysis...

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3) Pierre --> Ludwik
Je viens d'envoyer la note ci-dessous à Richard S. et Ludwik K. comme ultime contribution dans le contexte de ma venue à BOULDER. Ludwik comptait faire de nos travaux une note de synthèse. Je ne sais si c'est toujours son intention, à moins qu'il n'attende une duplication par Scott Little. J'ajoute pour répondre à vos questions que le brouillard qui apparaît au dessus du bécher apparaît de la même façon lorsque seule la résistance est opérationnelle. Par ailleurs, je ne comprend pas le débat sur le CO? pouvant s'échapper du bécher. Le CO? potentiel dans les 27g de CO3K2 mis en place dans l'électrolyte représente 8.6 g (le tiers de la masse en carbonate de K). Or, chaque essai consomme environ 250g de H2O et avant de renouveler l'électrolyte, nous faisons des dizaines d'essais. C'est donc pour moi un faux problème. J'attends cependant les mesures chimiques en cours avec Gérard.

= =
4) Pierre --> Ludwik+Richard+Gerard+ Jean-Francois Fauvarque
Finally, Gérard gives me the following answer (I am not a chemist and I am just trying to translate his note): 1cm3 of electrolyte was mixed with 50 cm3 of distillated water. HCl at 0.05M was used for dosing. You can see the result in the joined curve. First equivalent point for 1.71 cm3 Second equivalent point for 4.91 cm3 that is to say: 4.91-1.71=3.2 cm3

The first point seems to correspond to the neutralisation of a strong base: KOH (0.05x1.71=0.08M) and the second to the neutralisation of the remaining carbonate of potassium (0.05x3.2=0.16M). This electrolyte seems to be a little bit concentrated, but the answer is clear : due to the numerous runs we did with, we have not to be worried by the escape of CO2. !

= =
5) Ludwik --> Gerard+Pierre+Richard+Jean-Francois
I have no idea what is the meaning of the curve sent to us. How does it help us to make the data more trustworthy? Can this curve be used to show that not much water was lost in the form of tiny droplets?

= =
6) Pierre --> Ludwik+Richard+Gerard
My previous mail was not explicit enough. ... One liter of electrolyte contains 27 g of K2CO3. Inside this carbonate, there is about 8 to 9 g of CO2, which may be released by contact with the very hot plasma. This may be a cause of overestimation of the excess heat. This chemical analysis has shown that our Boulder electrolyte contains always 80% of the initial carbonate content in spite of a lot of runs and hundreds grams of evaporated water. Then it is clear that this very small release of CO2 (20% of 8 g, that is 1.6 g) leads to an insignifiant and negligible correction on all the

evaporated water... That's all is said by our chemical analysis (we will do the same with the initial electrolyte, not used). Are you now OK

= =

7) Pierre --> Richard+Ludwik+Gerard
Hi friends, I am to-day at the CNAM Fauvarque lab. and we are preparing with Gérard some experiments Paris2 type to be done on to-morrow friday. The purpose is to answer the questions raised by Ludwik and to help Pr Fauvarque to prepare his appendix on chemical aspects. We are very interested by the projects of Richard, but be careful Richard with the H2 O2 explosions... We plan to do an experiment similar of the CFR of Jean-Louis Naudin within two weeks, we hope. It seems to us that the thermal losses will be smaller and the global analysis simpler... We will see...

= =

8) Ludwik --> CMNS LIST (April 20, 2006):
Colorado2 experiments replicated the COP>1 claim of the Paris-1 team. They showed that by doing the same things we obtain similar results. That was an important step forward. To make Colorado-2 results publishable we must be able to convince honest referees that excess heat cannot be explained in terms of known chemical reactions. That is the main issue. Unfortunately, it has not been addressed by recognized authorities in chemical aspects of high voltage electrolysis. Did Mizuno, or some of his collaborators, publish something along these lines in Japanese?

My understanding was, and still is, that a complete analysis of potentially possible exothermic chemical reactions is now being conducted in Paris. Im I correct? Planning for new setups, ahead of such analysis, does not seem to be productive, at this stage. Writing a paper based on Colorado-2 results also seems to be premature, for the same reason. But we should compare the conductivity of water (from which the electrolyte was made) with conductivity of condensed liquid escaping from the cells (when the COP was measured). In that way we can estimate the percentage of tiny droplets of the electrolyte in the escaping steam. . . .

= =

9) Pierre --> Richard+Ludwik+Gerard+jean-Francois (April 22, 2006)
We are also trying to launch a new experiment "Paris 3" with a loop of ordinary water for cooling our experiment and with presently hand measurements giving water flow and inlet and outlet temperatures. ..I find as a very good idea your choice to take water at 0°C in order to cool the experiment, but of course you must be careful with thermal contributions (instead of thermal losses.). But I do not understand why your electrolyte is not at 100°C (or maybe at 94°C in Erie !). It is easy to do so with an ohmic heater. ..and it seems to me that the experiment analysis is simpler when every data are not all varying (power, temperatures, ...)

You will see in the joined pictures our first experimental set up, here without any heat insulator. Our first tests with the traditionnal ohmic heater show us too large losses of uncondensed steam (easily measured by our balance), about 40% at 400 watts and even more at 550 watts. Due to the fear of H2-O2 explosion, we managed a too large escaping gases area, in spite of screens located in the center in order to deviate steam on the nylon tubes at the peripherical side. We are now writing our chemical appendix... But, Ludwik, don't be too much impatient.. Gerard and Pr Fauvarque are very busy fort the time being.. I will send you soon the summary !

= =

10) Richard --> Pierre + Ludwik+Gerard+Jean-Francois (4/24/2006):
Bonjour Pierre and company, I am also starting up Colorado #3 experiment. I tried a similar set up to what you are showing in your pictures and found that it didn't work very well for me. I ended up putting the condensation coil just above the electrolyte attached to the lid of the cell. This arrangement works well and does a good job of condensing the steam. The lid over the cell only needs a small hole 3.0 cm in the center to let out the accumulated gas. In the pictures you can see that the cathode comes out the hole. I'm able to push the cathode down a little at a time and keep the reaction going for hours. I'm at work right now but I'll send some pictures tonight.

In this new arrangement I am still losing too much water as steam but it's only 50 grams/hour. I'm working on some baffles to force the steam to travel around the coil. I would like to get this down to less than 10 grams/hour. I also need to insulate the lid of the cell. I seem to be loosing a lot of heat to non-evaporated loses and it's almost all though the lid

of the cell.

I understand your concerns about thermal contributions. I have thermistors at the inlet and outlet of the coil to measure the input and output temperatures. I'm using a big ice chest right now for my cooling loop. The ice chest gives me several hours of 1-2degC water for the test.

My initial measurements with this new arrangement look good. I can't claim any excess heat. It looks like with just a few more modification I'll be able to run a test for several hours with continuous measurement of the input and output power. I agree it's very important that we have continuous measurements for several hours showing excess heat.

I think this experiment with little loss of water over the duration of the test and continuous excess heat for several hours will be very convincing. Running the test for several hours and taking the before and after electrolyte samples will gives us ideal data for a paper.

Ludwik, do you want to come to Colorado again and run help run experiment #3? I should be ready with all the new setup in about a week.

= =

11) Richard --> Ludwik (4/25/06):
I'm very encouraged by the results I got last night from my experiment. The heat exchanger is working nicely with good results. I'm seeing a COP of about 1.30 for over 90 minutes at 300 volts and 0.95 amps. I have a constant 3Deg temperature difference in the heat exchanger with a flow of 1.200 liter/minute. The ohmic heater calibration is showing a heat loss due to conductive and radiated of about 40 watts. Even if I only use the heat exchanger values I seen a COP = 1.1. Attached is draft of a paper for Colorado #3. This draft is first a way to make sure that my thought processes for the experiment are correct. I know I'm a long way from publishing anything. Your comments are appreciated.

= =

12) Ludwik --> Richard
See green comments in the attached file.

= =

13) Richard --> Ludwik
Thanks for the comments. Does the experiment sound solid?

= =

14) Ludwik --> Richard
I can judge what you do on the basis of what you write only. The preliminary description is not sufficient to form an opinion. A flow calorimeter seems to be appropriate when excess energy is generated at the rates exceeding ten watts. How accurately do you measure the flow rate? How much liquid does manage to escape? Suppose that the escaping liquid is 50% droplets. Can this alone explain the COP=1.3, for example? These questions must be addressed; devil is in details.

The most important task, for Colorado-2 and Paris-2 experiments is convincing chemical analysis. I am surprised that Fauvarque, who is presumably a top French electrochemist, did not address that issue in describing results of Paris-1 experiments. I hope this has been addressed in 1997 by Ohmori and Mizuno. Otherwise claims of "unexpected heat" cannot be made. Pierre promised Fauvarque's appendix to me but nothing happens.

= =

15) Richard --> Ludwik (4/26/06):
Ice water is feed to the condenser at 1.20 liters/minute.

Starting time 8:10   Ending time 9:20   total time 70 minutes

The delta temperature at the heat exchanger is 5 degree a 10K @ 25c thermistor
The delta temperature of the outlet water from the ice bath compared to the returning water was also 5 degree

These temperature reading are an average of about 15 random data points over the 70 minutes.

Input voltage 355 and input current 1.2 amps == 426 watts
This is also a random average of the input voltage and current.

The initial weight of the vessel was 2123 grams and the final weight was 2033 so losses due to evaporation or disassociation of H20 is 90 grams

Pin = 285
Pout = Pex + Plosses + Pevap
Pex = 5 * 20 *4.178 = 417
Plosses = I'm estimating it at about 25 watts.
Pevap = 2260 * 90 /(70*60) = 50 watts

Pout = 50 + 417 + 25
Pout = 507

COP = 1.18

= =
16) Ludwik --> Richard (4/26/06):
Thanks for numerical information. Congratulation for being able to forge ahead so fast. Can you also describe to me your condenser? Where is it located in the 2 liter container? How is it connected to the water pump, etc.?

= =
17) Richard to Ludwik (4/26/06):
I purchased a 1.5 liter microwave safe plastic container with an air tight lid from the hardware store. I modified the lid to hold a coil of copper tubing 1.5 meters long. There are two pipe fittings on the lid connecting to the coil. Inside these two pipe fittings I have the two thermistors to measure temperature. In the center of the lid is a 3/8 inch hole for letting out steam and H2-02. I also have some baffles below the coil so the steam can not rise straight up but must move in a convoluted way. This prevents any splashing out the hole in the center. I cathode extends out the 3/8" hole so I can just tap it down a little to keep the reaction going. It fits snuggly through the baffles.

I have an ice chest with a submersible pump. I've let the pump fill a 2 liter beaker about 10 times for 30 seconds. The liquid in the beaker was always 0.6 liters. It was right on the 0.6 liter mark. Looks like I can read the beaker to within 0.01 liter easily.

I have plastic hoses going from the ice chest to the beaker lid. These are both covered with pipe insulation. If I fill the ice chest with 20 lbs for ice I can run for about an hour with the inlet coil temperature being at close to 2degC, after that the inlet temperature starts to rise slowly as more of the ice melts. It looks like as longs as the temperature of the water in the ice chest is below about 40C I can still get good data. I'm bidding on several chillers on ebay, just lost the one I really wanted.

I'm really excited about this experimental setup. It really seems to address a lot of the problems with the open beaker. I hope I can get Advance Energy to run the SEM on the electrolye samples and on the cathode.

I would like you to consider helping publish the paper I'm writing and sent you. I should be able to finish up the missing part in the next month. Here is the latest draft I have added some dummy graphs that show what I'm expecting the final data to look like.

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18) Ludwik to Richard + Pierre (4/26/06):
I will start writing a draft of the paper after receiving two essential items:

a) An appendix written by Fauvarque (showing that known chemical reactions cannot possibly be responsible for the COP=1.24 (st. dev. 0.13) that was measured. I take it for granted that he is considered to be a recognized authority in electrochemistry. When should I expect his input. My experience with publishing is a rule: "the longer one waits the more difficult is to write." Sometimes motivation for writing goes away.

b) An estimate of the percentage of tiny droplets, based on some kind of experimental data. If the answer is ~5% then the accuracy with which the number is determined does not have to be excessive. But the open cell approach will be disqualified if the answer turns out to be close to 20%.

I am happy that you are making progress with Colorado-3 and Paris-2 experiments. Does it mean you no longer want to publish the Colorado-2 results in a separate paper? I would go along with your decisions.

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19) Ludwik to Richard:
Sounds like a big step forward. I wish I could assist you again. But not now. After you stop improving the setup you should perform about ten measurments of COP at each voltage. In that way mean values and standard deviations will help to resolve the pending issue. As you know, according to Paris-1 the COP goes up with voltage. But this was not confirmed by Colorado-2 experiments. That contribution alone will be a good reason for publishing an additional paper. On the other hand, considering the problem with droplets, perhaps your new results should be included into the paper we anticipated with Pierre. Just mussing, as you say.

20) Richard --> Ludwik (4/26/06):
I've got a few more things to do and I should be able to start collecting a lot of data for statistical analysis. Like you say we need about 10 runs per voltage level for good stats. This is why I don't understand what Michel is doing or not doing. You've got to have a way to get enough data without taking months.

I have got get some sort of circulating chiller. I keep running out of ice. I'm making a little printed circuit board that will have the 250Mhz AD835 multiplier chip. This will end once and for all the input power measurment controversy. I'm going to change the type of thermistor I'm currently using. I can get a different value that will give 0.1 resolution on the temperature. Repeatable is even better at 0.01.

If I can get AE to do the chemical analysis with their SEM I'll go back and run a couple of test like we did in the Colorado 2 experiment and have them analysis. Apparently they can show a plot with elements as light as carbon. They can't show any kind of consentrations. Hope all is going well with the closing. It's a lot of work to move especailly when your downsizing! Sorry you won't be able to help, but any suggestion on the paper will be appreciated.

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21) Pierre --> Richard+Ludwik (4/27/06):
I will be happy to have your pictures of Colorado#3 and of course to know where you are now in this new experiment... I am going now for two weeks of vacation in the south of France and I will finish these holidays by doing Paris1 test in the lab of JP Biberian (at Marseille) ; that will be Marseille1. Jacques Dufour and maybe Michel Jullian will be there. By the way, what about Scott Little ?

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22) Richard --> Ludwik (4/27/06):
The attache d file is a sample of the type of report Advance Energy can run for me. I'm going to ask them to do a scan of an electrode.

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23) Ludwik --> Richard: (4/27/06):

Information provided in the attached file -- locations of characteristic peaks (in keV) for different elements -- is of no interest to us. This is important before performing the X-ray fluorescence analysis. Once you know which peak (or peaks) to use to identify potassium the information we need is in the heights of peaks. Suppose we are interested in K and we know that it produces a peak at 321 KeV (I am just making it up). First we expose the 1 cc of the original electrolyte to X-rays for 10 seconds. Suppose it produces 10000 counts under that peak. We dilute the electrolyte by the factor of ten and the peak become 1000 counts. We dilute by another factor of ten and this time 1 cc shows a peak close to 100 counts. That is the calibration. It shows that counts under the peak, N, are directly proportional to the molarity, X, of a sample.

X=0.2*N/10000

For example, if N=20000 then X=0.4 M, if N=2000 then X=0.04 M, etc. All this is preparation. Now comes the essential moment. Suppose we take 1 cc of the condensed liquid. Suppose the 321 keV peak has only 2000 counts in 10 seconds. This would mean that the concentration of K ions is about 5 times less than in the original electrolyte. Is it good or bad for us? I think it would mean that about 20% of the lost liquid consists of the droplets of the electrolyte. The COP would be nearly 1.00 if that percentage was taken under consideration. On the other hand, a peak with N=100 would tell us that droplets contributed only ~1% of the lost mass. That is too little to explain the COP>1.2 as an artifact due to droplets.

Scott Little has a setup for that kind of analysis. It took him less that 30 min to demonstrate presence of tungsten in the electrolyte for me. I was planning on asking him to perform the K analysis for us, after you produce a sample of the condensed liquid. My suggestion was to use the cold funnel method. But any other method would be OK. The experiment must be performed in the open cell, as we did when the COP=1.24 was determined. As you know, I prefer to finish Colorado-2 before starting Colorado-3, even when we know that Colorado-3 method is better.

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24) Ludwik --> Scott Little, Pierre and Richard (4/28/06):
Hi Scott:
1) As you know, presence of tiny droplets in the lost liquid is one of the possible explanation of the high COP. One way to show that the COP=1.24 for example, less that ~1% of what is in 1 cc of the original electrolyte. On the other hand ~24% would confirm the suspicion of the illusion. Do you agree?

2) I did talk to a chemistry professor at my university but he said they have no instruments to measure K. This weekend Richard will have samples to be analyzed.

3) Your X-ray fluorescence setup is probably ideal for what we need? Am I correct? If so then perhaps you will be able to analyze Richard's samples.

4) How is your gravitational project? Are you still planning to verify Colorado2 results? Pierre wrote to me that a very convincing appendix, written by Fauvarque, will be sent to me soon. It will presumably convince honest skeptics that excess heat cannot possibly be due to known chemical reactions. Not being a chemist I will simply accept this; Fauvarque is a recognized authority in the field of electrochemistry. I do not want to start writing the draft of our paper before receiving the appendix, and before knowing that the mass of droplets was a negligible fraction of the lost mass. Should I also wait for the Texas2 results? I hope the answer will be positive.

5) Jean-Paul Biberian, if I recall correctly, also has a setup to perform the X-rays fluorescence analysis. He and Pierre are planning on a new plasma electrolysis experiment in France.

6) I am going to be busy this summer with downsizing activities (emptying the house, selling the house, buying an apartment, and moving). But this should not prevent me from writing the draft. In my mind those who worked on Paris1, Texas1, Colorado1 (before the ICCF11), Colorado2, Paris2 and Texas2 experiments will be the authors (listed in alphabetical order). Let me know if you do not like this suggestion.

7) Rising gas prices made me think about your home-based project for the agricultural fuel, Scott. Are you making

progress on that front? Most cars in Brasil, I was told, run of fuel made from sugar can.

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25) Scott --> Ludwik + Pierre + Richard (4/29/06):

"2). . . On the other hand ~24% would confirm the suspicion of the illusion. Do you agree?" Yes, that seems correct and obvious. To analyze the K content easily, how about just weighing a small amount of the sample (i.e. 1 gram), evaporating to dryness, and weighing the dry salt? Yes I know there is some concern that the K2CO3 will turn into KOH but that won't create a huge error in the results.

"3). . .If so then perhaps you will be able to analyze Richard's samples." Yes, I could probably make a reasonably accurate analysis for elemental K content that way. I need about 15 mL of solution to fill the sample cup properly. Actually if that is a great difficulty, I could probably dilute the samples 10:1 and still attain satisfactory accuracy....so I could use as little as 1.5 mL of sample.

"4) How is your gravitational project?" We've completed construction. Now we are at the hard part: making it work properly. It will be difficult, I'm afraid. "Are you still planning to verify Colorado2 results?" Yes, but I don't know exactly when. The gravity project is out top priority right now and it is not finished.

"Should I also wait for the Texas2 results? ....." That depends upon when you want to complete the paper. Perhaps in another month, I'll get to the Texas-2 project.

"6) In my mind those who worked on Paris1, Texas1, Colorado1 (before the ICCF11), Colorado2, Paris2 and Texas2 experiments will be the authors (listed in alphabetical order). Let me know if you do not like this suggestion." You only need to list me as an author it I actually get around to doing Texas2. If I am still busy with the gravity experiment and you want to get the paper finished, you may just mention the Texas1 experiments without including me as an author."

"7) Rising gas prices made me think about your home-based project for the agricultural fuel, Scott. Are you making progress on that front?" No. Our process does not make sufficiently pure biodiesel and we do not know how to improve it. I, too, am not a chemist and that makes me uncomfortable with this kind of experimentation. I just feel like I have no clue what to try next.

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26) Richard --> Ludwik (5/5/06):

I'm not sure how to read this chart but it's suppose to show percent of
wet/dry steam at different pressures and temperatures.

http://www.chemicalogic.com/download/mollier_chart_english.pdf

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27) Ludwik --> Richard: (5/5/06):

My understanding is as follows:

1) Suppose we have a long cylinder with a piston. It contains some water, for example, 1 Lb or 1 kg. Space above water is vapor (air was pumped out and a very small amount of vapor accumulated). The system is said to be in equilibrium, at any given temperature, when the rate of evaporation and the rate of condensation become equal.

2) At room temperature the equilibrium is when nearly 0% of water is vapor (stem) and nearly 100% is liquid (water). Suppose we supply heat but keep the pressure constant. To accomplish this the volume must be increased. We do this by pulling the piston out (as much as necessary to keep the pressure gauge at the same reading). By adding heat (enthalpy) we create more vapor and less liquid. That what the numbers 10%, 20%, etc. are above constant P line.

3) Saturated (100% dry) steam, I guess, is along the 100% line (on the right). Saturated liquid, I guess, is the 0% line

(on the left). The distance from the 0% line to the 100% line is the latent heat of evaporation, L, for any chosen P.

4) Instead of plotting P, along the vertical axis, one can plot temperature (P*V=RT for one mole of an ideal gas). That is what is shown in the second figure in:

http://www.spiraxsarco.com/learn/modules/2_15_01.asp

5) I think that steam / liquid percentages (at constant P or T) have nothing do do with what we want to know (% of tiny droplets lost). But the second figure in the above URL reminded me about something I wrote before. Somebody responded that my concern was not valid. But I was not convinced. The figure shows that the latent heat of evaporation (L~2250 j/g) depends on the temperature. It decreases when T becomes higher. This might be responsible for the excess heat we measured. The temperature at which steam is formed (at the surface of the cathode) is very high and not clearly defined. Our system is certainly not at equilibrium. The assumed L=2260 J/g might be wrong. Suppose L=2000 J/g but we are not aware of it. We multiply the mass lost by 2260 and this gives us 10% larger thermal energy than it really is.

I will address this issue at the CMNS list again. Perhaps this time somebody will convince me that using L=2260 is OK.

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27) Pierre --> Ludwik + Richard (5/25/06):
Pr Fauvarque gave us yesterday the draft of the "chemical" appendix. We hope to send you this note by the end of the next week. I met the same kind of difficulties for Marseille 1 as in Boulder for Colorado 2 during the first days. As soon as we change a little bit the design (for example a new beaker higher than the old one in order to avoid definitively the droplets problem, we have problem with the COP values (only 1.10 to 1.12). And also the wattmeter instability. I hope that Richard will give us soon better news.

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28) Ludwik --> Pierre + Richard (5/26/06):
If taller container gives smaller COP then we must worry about tiny droplets. It is natural to expect less droplets when the container becames taller. In a recent CMNS list posting Michel Jullian wrote: “There was _some_ salted water escaping, by which amount it inflated the COP we don't know yet (among the few excellent qualitative tests performed, . . .) “ What kind of tests were performed and what percentage of droplets was found? I need the answer to this question in order to start drafting our paper. Also how many grams of tungsen is consumed per one kJ of excess heat? I am referring to experiments in which the COP is close to 1.24, as in Colorado2.

Michel also wrote: “Strangely, the addition of a 470µF decoupling capacitor directly across the cell's electrodes in the afternoon yielded sub-unity COP readings (0.8 or so) instead of the over-unity COPs (1.25 or so) which we had obtained in the morning confirming the COPs observed in the previous Fauvarque-ICCF12 type GDPE experiments.” That seems to contradic what Pierre wrote about the COPs in Marseilles -- 1.10 and 1.12. I suppose that the COPs close1.25 were from the original cell, not from the taller cell. I am also puzzled by the effect of the 470µF capacitor on the COP.

How should the last sentence in Pierre’s massage be interpreted? Are we ready to write a paper based on Colorado2 experiment or should we write a paper based on Colorado3 experiment instead?

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29) Pierre to Ludwik + Gerard (5/28/06):
Je ne remets pas en cause nos résultats et notamment ceux de Boulder. Je pense seulement que cette expérience est délicate et que certaines conditions n'étaient pas remplies : lueur jaune-orange du plasma, intensité voisine de 1A à 1A.15, alors que nous avions des valeurs trop élevées, proches de 2A. etc.. Il faut donc reprendre cette expérience calmement et ce ne sont pas les droplets qui doivent modifier les résultats. Je suis bien sûr en faveur d'un papier sur Colorado 2 mais j'attends aussi les résultats de Richard sur Colorado 3 avec beaucoup d'intérêt. J'ai tardé à vous répondre car j'ai du participer à des évènements familiaux, loin de Paris. Je suis de retour et pour gagner du temps , je

vous écris en français...

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30) Ludwik to Richard+Pierre+Gerard (5/29/06):
Pierre wrote in French (see below) to gain time. He is not questioning validity of Colorado2 results. He thinks that "certain conditions were not met," such as color of plasma and the current of ~ 1 A. Our current was ~2A. "Thus we must reconsider these experiments calmly." Pierre does not think that droplets are responsible for the COPs>1. [But that is not enough; we must have data showing that escaping water contained no potassium. That became the central issue when our results were discussed on the CMNS list. My impression was that Gerard was conducting chemical experiments. What did he measure and how can this help us to rule out the effect of droplets? Pierre is in favor of publishing the paper based on Colorado2 but he is eager to see Richard's Colorado3 results.

I am slightly confused by the "we must reconsider these experiments" phrase. What does it imply in practical terms? What should our time-table be? Should we wait or should we try to publish the Colorado2 results as soon as possible? Either way would be OK with me. Let me add some additional observations.

1) What makes the current lower (~1 A instead ~2 A at 350V)? I think it is the thickness of the plasma envelope and concentration of ions in it. The resistance goes up when the layer becomes and the current becomes lower. The current is also directly proportional to the concentration of ions. To get lower current we must decrease the ion density. But I do not know how to control the the layer thickness and degree of ionization. Perhaps the diameter of the cathode has something to do with this. The resistance of the layer goes up when the cathode diameter goes down, provided the layer thickness remains the same.

2) Our temperature (near the wall of the cell) was 89 C, that is because the two-litter container was too large for power we used. I am assuming that others used smaller vessels and their temperatures were uniformly equal to the boiling point. One might suspect that differences between currents are due to differences in temperature. But I do not think so. After all, what counts is the temperature of the plasma and not the temperature near the walls.

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31) Ludwik --> to Pierre + Richard (6/2/06):
1) For some reason I had no replies from you. Perhaps my message was lost; That is why I am pasting it below. . . . .

2) Today is Friday; when should I expect Fauvarque's report?

3) What is happening with our attempts to learn about concentrations of potassium in the escaped water and in the electrolyte? My impression was that this work is in progress -- Richard+Scott in the US and Gerard in France. Where do we stand on this?

4) Most of my draft is ready. I will send it to you very soon. My suggestion is to have Richard as the first author for our Colorado2 paper. The rest of us will be listed alphabetically. For the time being I am listing everyone. But I am not sure about George Luce (who works with Scott). And I do not know if Scott will be willing to be the author.

5) Should Fauvarque be the author of the paper or should he only be the author of the appendix? It does not make any difference to me. I will wait for the input from Pierre about this.

6) We also need a good title. Authors: Richard Slaughter, Pierre Paul Clauzon, Jean-Francois Fauvarque, Ludwik Kowalski, Gerard Jean-Michel Lalleve, Scott Little and George Luce

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32) Pierre --> Ludwik+Richard+Gerard (6/5/06):
I thought that I answered you, Ludwik... We are so much submerged by a lot of e--mails (for instance Michel J.) that I do not know where I am with you. First, for us, we should go ahead with the Colorado 2 paper ! The draft of the chemical appendix you are waiting for is on the desk of Pr Fauvarque since three days at least! But he is a very busy man and last week, he was responsible for an chemical meeting of two days and so, inaccessible for almost all the

week ! Today, it is the Monday of Pentecôte, usually a public holliday... and however, we hope to meet with Gérard in the CNAM Lab. ...We will try to definitively give a final answer to Marseille 1 and we hope to get a COP larger than the value of 1.12 we got recently… To my sense, this is only in order to have an other confirmation but this result is not necessary for Colorado 2.

I know that experts in electronics like Michel J. or Jacques Dufour are trying to be sure that the input energy is correctly measured. Jacques Dufour will inspect with his oscillometer our voltage and current signals… We will see, but I am confident with the measurements of Richard and his own expertise. ..Furthermore, I do not think that droplets or tiny droplets are a real problem. When you get the good operating conditions with only about 1 A and the good plasma color, you do not have droplets at all. ..Did I answer you , Ludwik?

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33) Pierre to Ludwik ((6/5/06):

The paper joined is just for your personnal information... it is still a draft we have to complete! If you have questions about this note, please write only to me...Thanks in advance...

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34) Ludwik --> Pierre (6/5/06):

Thanks for showing Fauvarque's draft. One thing is clear; I do not understand it. And I do not want to be a coauthor of something I do not understand. Therefore, I suggest that Fauvarque is shown to be the author of the appendix in our paper. It often happens that we have to form opinion about something we are not qualified to understand. When this happens I rely of authoritative statements of experts. In this case we should post the appendix (when Fauvarque agrees) on the list and ask for constructive criticism. Do you agree that this would be the best strategy? Is Fauvarque the member of the CMNS list?

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35) Richard --> Ludwik + Pierre (6/5/06):

I have not been getting any private message from you. I too think we should go ahead and publish the Colorado report. I have been busy at work and have not done very much on the experiments. The calorimetry is proving to be more difficult than I anticipated. I've had some good results with the ohmic heater showing the correct output power. I just don't get the consistency that is needed. I'll try once again to get the samples to Scott for checking the amount of K2CO3 in the evaporated water.

36) Ludwik --> Richard + Pierre (6/5/06):

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You wrote: "I have not been getting any private message from you." That means that at least one of my recent messages was lost. I was worrying that you might be sick or something else serious. You also wrote: " I too think we should go ahead and publish the Colorado report. I have been busy at work and have not done very much on the experiments. The calorimetry is proving to be more difficult than I anticipated. I've had some good results with the ohmic heater showing the correct output power. I just don't get the consistency that is needed."

That is Colorado3. I do not think we should be focusing on that experiment. Except, perhaps at the end, to show preliminary results from "work in progress," if you want. My draft will be about Colorado2 only. You also wrote: " I'll try once again to get the samples to Scott for checking the amount of K2CO3 in the evaporated water."

That is good. I will be waiting for Scott's results, and for the chemical appendix of Fauvarque. He must know how much W is consumed during our typical experiment. Did you give him this information, Pierre? Most of the lost tungsten is probably still pure metal, in the deposit, as in Mizuno experiments. But some of it is probably reacted with something to generate heat. I hope he will show that the total number joules from all possible chemical reactions is no more than 150J in 5 minutes. That would be less than 1% of our mean excess heat. We need the number of joules, and a description of data from which the number was obtained. Also some references, to satisfy chemists. It would take no more than two or three days to incorporate the missing information into my first draft. What each of you think about showing the draft to others on our CMNS list (after discussing it in private)? That would help us to end up with a better manuscript.

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37) Pierre --> Ludwik+Richard+Gerard (6/6/06):
Our W cathode is 15 cm long, 13.2 g heavy, and the cross sction diameter is 2.4 mm large. A typical run of 20 to 30 minutes burns about 1.5 cm of the cathode (i.e. 1.32 g). A typical abnormal excess energy is about 60,000 joules (100 w during 10 minutes).

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38) Ludwik --> Pierre + Richard + Gerard :(6/6/06):
My suggestion is to collect the residuals (for example, after evaporating water) and to find out how many grams of metallic tungsten it contains. According to Mizuno only a small fraction of lost tungsten participates in chemical reactions. Most of W simply melts away and remains metallic. That means that the amount of tungsten acting as a fuel for chemical heat is actually much less than 13 grams, from each cathode, perhaps less than 0.13 grams. That information will probably be very useful to Fauvarque. How else can he calculate the upper limit of chemical contribution to excess heat? Another suggestion, for the appendix only, is to express things in terms of joules and not in terms of watts. Most people know how to go from joules to eV.

Suppose we report 150 kJ in 25 minutes and that the actually reacted mass of tungsten was 0.13 grams. The 150,000 J translates into 9.4*10^23 eV. The 0.13 grams of tungsten translates into 4.24*10^20 atoms. In other words 9.4*10^23 / 4.24*10^20 = 2217 eV per atom. Such amount of energy per atom is three orders of magnitude higher than what is usually associated with chemistry. Note, however, that the result would be only 22 eV per atom if all the lost tungsten reacted as fuel. That is less dramatic than 2217 eV/atom.

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39) Richard --> Ludwik + Pierre + Richard :(6/6/06):
I have not been getting any private message from you. I too think we should go ahead and publish the Colorado report. I have been busy at work and have not done very much on the experiments. The calorimetry is proving to be more difficult than I anticipated. I've had some good results with the ohmic heater showing the correct output power. I just don't get the consistency that is needed. I'll try once again to get the samples to Scott for checking the amount of K2CO3 in the evaporated water.

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40) Ludwik --> Richard (6/6/06):
Below is the message that you probably did not receive. . . .

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Ludwik (in a message posted at the CMNS list) 6/8/06:
. . . My draft describing Colorado2 results will be finished as soon as two missing components are received. I intend to expose it to criticism. And, personally, I would have no objection for not publishing it. Waiting for results from Paris3, Colorado3 and Marseilles1 experiments might indeed be preferable, as suggested by Michel.

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41) Ludwik --> Gerard + Richard (6/9/06):
(1) Is it OK to show, in our paper, that Fauvarque is the author of the appendix? Why is Fauvarque not participating in our discussions? I would very much like to read what he thinks about GDPE. He is the only electrochemist among us, as far as I know. His active participation would give me more confidence. I am hesitant to go ahead without his active participation. (2) Since I am totally ignorant in chemistry I would like this appendix to be discussed on our CMNS list? Can I post it today and ask for comments? (3) What should I write about this appendix in the main text of our paper? That text does not expect readers to know chemistry. Please write a paragraph for me. More questions later.

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42) Ludwik --> Pierre + Richard (6/10/06):
I asked Gerard some preliminary questions this morning. But he did not reply at once. Perhaps you can answer them. Here they are:..... Meanwhile I had to relearn what "titration" is. This shows how little chemistry I know. I am waiting

for your OK about posting the appendix on our CMNS list. I hope others will say that the appendix is well written and should be very convincing to a chemist.

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43) Ludwik --> Jean-Francois (Fauvarque) + Pierre + Richard (6/10/06):
Dear Dr. Fauvarque: I am writing a paper about results of our Colorado2 experiment. The appendix you wrote will be an extremely important part of it. Gerard sent me your appendix yesterday but I am in no position to evaluate it -- I am not a chemist. Our paper would not be worth publishing without convincing arguments that excess heat we measured cannot be due to possible chemical reactions.

Publishing a paper (associated with the controversial field of cold fusion) in a mainstream science paper will not be easy. That is why all possible objections should be anticipated. Our statements should be backed by references and by results of numerical calculations. My first impression was that your appendix is vulnerable to criticism of objective referees. That opinion is probably due to my limited familiarity with chemistry. In the main body of the paper I would like to say something like this: "A possibility of chemical origin of excess heat has been carefully examined by one of us (JFF), as shown in the appendix. The conclusion was that chemical fuels present in the cell could not possibly contribute more than 1% (???) to excess heat measured. We definitely need an upper limit, plus a strong appendix to numerically justify it. The fact that the appendix was produced by a well known electrochemist is essential, as far as i am concerned.

P.S. If the only possible fuel were tungsten, and if most of what was eroded during the electrolysis were found as pure metal at the bottom of the beaker, then the chemical contribution would be much less than 1 %. Here is my recent message to Pierre about this:

On Jun 6, 2006, at 9:09 AM, pierre.clauzon@wanadoo.fr wrote:
Ludwik, our W cathode is 15 cm long, 13.2 g heavy, and the cross sction diameter is 2.4 mm.
A typical run of 20 to 30 minutes burns about 1.5 cm of the cathode (i.e. 1.32 g).
A typical abnormal excess energy is about 60,000 joules (100 w during 10 minutes).

Pierre, my suggestion is to collect the residuals and to find out how many grams of metallic tungsten it contains. According to Mizuno only a small fraction of lost tungsten participates in chemical reactions. Most of W simply melts away and remains metallic. That means that the amount of tungsten acting as a fuel for chemical heat is actually much less than 1.3 grams, in a typical run, perhaps less than 0.013 grams. That information will probably be very useful to Fauvarque. How else can he calculate the upper limit of chemical contribution to excess heat? Another suggestion, for the appendix only, is to express things in terms of joules and not in terms of watts. Most people know how to go from joules to eV.

Suppose we report 150 kJ in 25 minutes and that the actually reacted mass of tungsten was 0.013 grams.
The 150,000 J translates into 9.4*10^23 eV
The 0.013 grams of tungsten translates into 4.24*10^19 atoms.

In other words 9.4*10^23 / 4.24*10^19 = 22200 eV per atom. Such amount of energy per atom is three orders of magnitude higher than what is usually associated with chemistry. Note, however, that the result would be only 222 eV per atom if all the lost tungsten reacted as fuel. That is also impressive, but not as impressive than 22200 eV/atom.

We decided to discuss the draft of the paper among the authors first. Then, after making necessary changes, we want to post it on the restricted CMNS discussion list. There are many highly competent and friendly people on that list; they will probably help us to produce a difficult-to-reject paper. Are you among the CMNS subscribers? I will send you the draft as soon as it is ready. To avoid mistakes made by many, including Fleischmann, our paper should not claim that excess heat is nuclear; we have no evidence for this. Here is the last section of my draft:

Conclusion:
The overall conclusion is that the GDPE thermal-excess energy, discovered in (1), and confirmed by other investigators (9, 10 and 11), seems to be real. Generation of excess heat, at the 0.1 kW level, turned out to be

reproducible. The origin of that energy is not clear to us. Can several cells be used in a battery generating excess heat at a much higher rate? Not having an accepted theory one must rely on empirical investigations. Systematic studies of effects of various parameters (sizes, compositions, distances, etc.), on the performance of Mizuno-type cells, seem to be warranted.

Absence of radioactive byproducts is a clear indication that familiar nuclear reactions cannot possibly be responsible for the amount of excess heat measured. A thermonuclear D-D reaction generating excess heat at the rate of 0.1 kW, for three hours (our total electrolysis time), would produce about 4*10^19 neutrons. That would be sufficient to produce radioactive isotopes in many objects near the cell. A Geiger counter we used showed that the level of radioactivity, near the cell, was not higher than natural background.

Selected Referneces:

1) Ohmori and Mizuno "Strong Excess Energy Evolution, New Element Production, and Electromagnetic Wave And/Or Neutron Emission in the Light Water Electrolysis with a Tungsten Cathode." presented at ICCF7,1997.
. . . .
9) Jean-Lois Naudin's web site - “CFR project, a High Temperature Plasma Electrolysis based on the Tadahiko Mizuno work from the Hokkaido University (Japan)” http://jlnlabs.imars.com/cfr/

10) D. Cirillo, A. Dattilo, V. Iorio, “Transmutation of metal to low energy in confined plasma in the water (electrochemical plasma cell),” *,”*. in *Eleventh International Conference on Condensed Matter Nuclear Science*. 2004. Marseille, France. The report can be downloaded from the library at <http://www.lenr-canr.org>

11) Jean-Francois Fauvarque, Pierre Paul Clauzon and Gerard Jean Michelle Lalleve. *“Abnormal excess heat observed during Mizuno-type experiments;”* 2005. The report can be downloaded from the library at <http://www.lenr-canr.org>
. . . .
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44) Ludwik --> Pierre + Richard (6/10/06):
That is OK with me. I just sent an email message to JFF, plus a copy to you and to Richard. That would be a good way to start discussing our paper. Is he a CMNS list subscriber?

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45) Pierre -->Ludwik + Richard (6/11/06):
Up to now, JF Fauvarque was not a member of the CMNS list. As I told you, he is a very busy man and I just make for him copies of the most meaningful papers delivered in the CMNS list. We are working together with Gérard, under the leadership of Pr Fauvarque. When I am in the CNAM labs, I use the computer of Gérard for the mails, for example. But, before sending you any mail, I usually ask Gérard for his approval..

46) Richrd --> Ludwik + Pierre (6/11/06):
I think it is a good idea to post Fauvarque’s appendix. I’m sure he knows we intend to publish the appendix with our paper. This way we/he can get some additional feedback. I would add some comments to the paper saying it is not for distribution outside the CMNS group. I suspect Fauvarque is just very busy, still he did work on the paper. I think we should send a personal thank you note of appreciation. Perhaps in this note we could ask how he would like to be credited with the appendix

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47) Ludwik --> Jean-Francois + Pierre + Richard (6/11/06):

1) Richard, I agree that we should thank Pr. Fauvarque for willing to help us. This morning I looked at the list of his publications, fetched by Google, and I was very impressed. Our claim that the measured excess energy cannot be due to chemical reactions would not be taken seriously by reviewers and editors without a reputable electrochemist. We are lucky to have him. Perhaps he will find time to communicate with us by email during this final stage -- discussing the draft and strategy for publishing (where, what to say, what not to say, etc.). As you know, publishing a paper on a controversial topic is much more difficult than a paper making a contribution to what has already been published.

2) You also received the draft (as an attached file sent by Gerard) of what he wrote. I am reading that appendix again and formulating comments. But before sharing my comments and questions with coauthors I will wait for the reply from JFF. Pierre prefers to discuss the appendix between the coauthors before posting it on the CMNS list. That what we should do for the next two or three days. If nobody comes with suggestions to modify what was written by Jean Francois then I will post the appendix on the CMNS list.

3) Did you send samples of our electrolyte, and of condensed liquid, to Scott? Using his X-ray fluorescence device he promised to determine the percentage of tiny invisible droplets in the escaping liquid. I will be waiting for his input about the droplets. It is also essential. I am no longer counting on Scott's attempt to replicate our results. My draft cannot be finished unless I know that the percentage of droplets was very small, for example one percent or so. We must be absolutely certain that it not close to 24% -- that would show that the COP=1.24 was an illusion. Was the percentage of droplets measured by Gerard, in Paris2 experiments (immediately after Colorado2)? What was the result and how accurate was it? I would like to say something like --> 2% +/- 0.5%. Anything above ~5% would push us away from the comfortable level of high confidence. (that the COP > 1.0).

4) My notebooks are in boxes that were put into self-storage. That is will probably have some questions to you and to Pierre. For now I want to know the capacity of the 500 V electrolytic capacitor we used at the output of your power supply?

5) I also need information about a test we did not perform. Can one of you perform the following test:

a) Start the experiment as usual (by increasing the voltage and performing the first measurement of COP, the one that should always be rejected).

b) Then turn the voltage off suddenly, disconnect the cables, and measure the voltage between the electrodes. We believe that the voltage will be zero. But this should be checked. It will be useful to describe this test in our paper. If the voltage is nearly zero then we will be able to rule out a possibility of a battery-like effect (storing energy during the preliminary steps and releasing it when the COP is measured). That kind of objection can be made by a reviewer. Our answer, even without the suggested test, would be that stored energy would have to be several times larger than 30 kJ (100 W times 300 seconds case after case.) But reporting that we performed such test will show that such possibility was not ignored.

6) In fact, I would not be surprised to see a small remaining potential between the electrodes. If you observe this then treat the cell as if it were a capacitor and calculate the energy stored. Use a two-trace oscilloscope to see the v(t) and i(t) exponential pulses (slow discharging). That what I would do to find the stored energy. I am 99.9% sure it will be much much less than 30 kJ. Perhaps we will be able to say that stored energy turned out to be 0.01 kJ, or something like this.

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48) Ludwik -->Gerard + Jean + Pierre + Richard (6/12/06):
On Jun 9, 2006, at 8:27 AM, gerard.lalleve@laposte.net wrote:

About now the droplets case, we confirm to you that we
have not at all loss of potassium when the experiment
is operated in a right way. In Marseille also, we used
chemical paper to see if the steam was polluted by
very tiny droplets. The result was again negative.

1) What we need are numerical results. Something like "contribution of tiny droplets to the mass of the escaping wet steam was found to be 2.5% (???)." Or something like "less than 1%," if the titration method is not sensitive enough to produce one number. That would appear in the main text; the Appendix-2 is for the methodology used to establish the result.

2) Likewise, the main text would say something like "contribution of known chemical reactions, to the excess heat measured, could not possibly exceed 0.01%" (???). That statement would be justified in the Appendix-3.

3) Note that the (???) indicates wishful thinking; I have no idea what the actual numbers will be. I already wrote Appendix-1; it is about measuring electric energy. In my opinion three short appendices are better than a single long one.

4) Today I will read the long appendix written by JFF again. My general impression was that it does not provide percentages we need to justify the COP=1.2 claim. I would like to know what each of you think about the long appendix. Do you agree that turning it into two shorter appendices is desirable? If so then we should see and discuss them, as soon as possible.

5) Where do we stand on the X-rays fluorescence test of samples, promised by Scott? Where do we stand on the estimation of the fraction of tungsten, lost by the cathod, that remains metallic? According to Mizuno that fraction (found in the deposited residuals) is very large. What remains metallic did not participate in chemical reactions and did not contribute to chemical excess heat. That should probably be taken under consideration in the Appendix-3.
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49) Richard --> Ludwik (6/12/06):
I've been very busy at work and just have not had the time and energy to work on CF. I'll try very diligently to get the sample for Scott this week.

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50) Ludwik --> Richard (6/12/06):
1) I understand your dilemma perfectly. I had a similar problem before retirement. Work connected with teaching had to be my first priority, no matter how much I wanted to do something else.

2) I am puzzled by silence from Fauvarque. The appendix he wrote (Pierre said he gave it to them about two weeks ago) was not very helpful to me. I hope it will be changed. Otherwise our paper will be in trouble. Any referee will see that it is not qualitative, as far as what we want to know is concerned. Most numbers refer to intermediate results from titration.

3) Scott would most likely provide reliable numbers for the Appendix-2 (percentage of droplets in wet steam). But rebuttal of the idea of chemical origin of excess heat (Appendix-3) must be produced by an electrochemist familiar with exotic hot-plasma ebvironement.

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51) Ludwik --> Jean + Richard + Gerard + Pierre (6/13/06):
JFF wrote (in our appendix):
1- Acid-Base titration
5 mL of the initial solution is titrated by HCl 1.0 M. The two successive neutralisations of CO3 2 - at pH 8.5 and of HCO3- at pH 4.5 are clearly visible. As expected, no other neutralisation is detected. The titration gives 1.94 M of K+ (counter ion), the carbonate being initially under the form CO3 2 -. . . .

The molarity of 1.94 seems to be impossible. Our electrolyte was 0.2 M. Assuming nearly 100% ionization of K2CO3 in water I would expect the molarity of K+ to be 0.4, or a little less, but not 1.94. Where am I wrong?

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52) Jean --> Ludwik (6/13/06):
I was wrong, it was a writing lapsus, the K+ molarity is 0.388, as expected.

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53) Ludwik --> Jean (6/13/06):

Thanks for the quick reply. Suppose "wet steam" is condensed when a measurement of COP is in progress. If the K+ molarity of condensed liquid turns out to be close to 0.008 then we would be able to say that the contribution of tiny droplets of electrolyte, to the lost mass, is about 2%. The rest is pure vapor. That would be a strong argument that the COP determined in that experiment is not an illusion due to tiny droplets. And our Colorado2 results (mean COP=1.24, st.dev. =0.13) will be taken more seriously. Do you agree? If so then please ask for the appropriate measurement (or several measurements to determine the precision) to be made.

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54) Ludwik --> Jean + Gerard + Richard + Pierre (6/13/06):

Dear RS, , PC, JFF, LK and GL:

1) These are initials of authors in my draft, in the order shown. For obvious reason, Richard is the main author, the rest of us are listed in alphabetic order, according to last names. Is this OK with everybody. I still do not know what the status of Scott Little will be. It will depend on his promised contributions.

2) I wander if the message sent nearly 40 hours ago was received by everybody. My expectation was that we will start discussing the appendix, as soon as possible. That was the purpose of comments shown below. What does each of you think about my comments?

3) This morning Jean-Francois asked me to replace the 1.94 M by the 0.388 M, in two or three places. I did this in my file and you should do it in yours. In that way we will be discussing identical texts.

Ludwik

On Jun 12, 2006, at 9:43 AM, Ludwik Kowalski wrote:

1) What we need are numerical results. Something like "contribution of tiny droplets to the mass of the escaping wet steam was found to be 2.5% (???)." Or something like "less than 1%," if the titration method is not sensitive enough to produce a measured value. That would appear in the main text; the Appendix-2 is for the methodology used to establish the result.

2) Likewise, the main text would say something like "contribution of known chemical reactions, to the excess heat measured, could not possibly exceed 0.01%" (???). That statement would be justified in the Appendix-3.

3) Note that the (???) indicates wishful thinking; I have no idea what the actual numbers will be. I already wrote Appendix-1; it is about measuring electric energy. In my opinion three short appendices are better than a single long one.

4) Today I will read the long appendix written by JFF again. My general impression was that it does not provide percentages we need to justify the COP=1.2 claim. I would like to know what each of you thinks about the long appendix. Do you agree that turning it into two shorter appendices is desirable? If so then we should see and discuss them, as soon as possible.

5) Where do we stand on the X-rays fluorescence test of samples, promised by Scott? Where do we stand on the estimation of the fraction of tungsten, lost by the cathod, that remains metallic? According to Mizuno that fraction (found in the deposited residuals) is very large. What remains metallic did not participate in chemical reactions and did not contribute to chemical excess heat. That should probably be taken under consideration in the Appendix-3.

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55) Richard --> Ludwik (6/14/06):

I'm glad you are on top of this. It's difficult for me to follow. I've got four samples of condensed liquid tonight. The test when easily but I didn't get any real high COP. The highest was 1.20. I'll send these to Scott tomorrow.I have three samples of the condensed liquid and one sample of the electrolyte from the beaker. That will help keep everyone honest. I'll try for some higher COP this week.

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56) Ludwik --. Richard (6/14/06):
1) On 4/29/06 Scott wrote to us: "Yes, I could probably make a reasonably accurate analysis for elemental K content that way. I need about 15 mL of solution to fill the sample cup properly. Actually if that is a great difficulty, I could probably dilute the samples 10:1 and still attain satisfactory accuracy. .. so I could use as little as 1.5 mL of sample."

2) Keep this in mind. My suggestion is to split each of your two sample into two and to send only one half of each to Scott. The others may become very valuable if a serious discrepancy develops between the two results (Scott versus Jean-Francois). In that case we might send the samples to a commercial lab. Many medical and environmental labs are well equipped for the analysis of potassium -- they do this routinely.

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57) Richard --> Ludwik + Pierre + Scott (6/15/06):
Scott, I sent you four samples today. Each sample is 10 to 15 grams I hope this is enough. Samples 1 to 3 are of 3 runs that had COP's > 1.1. I was not able to get high COP's but I will try again. I sent you all of the sample I took. So you might want to keep part of the sample after you dilute it for further study. The forth sample is just some of the electrolyte after running the tests. Thanks for the help

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58) Pierre --> Ludwik + Richard (6/15/06):
Bonjour Ludwik, Je t'écris du laboratoire de Jacques Dufour, beaucoup mieux équipé que notre ancien laboratoire (enregistrements des mesures, alimentations diverses, etc..). Avec l'accord de nos chefs (Prs Fauvarque et Foos), nous avons lancé l'opération Paris 3 ensemble.

Vous êtes entrés en relations, Fauvarque et toi, pour cette annexe chimique. Après en avoir discuté avec lui, nous pouvons dire qu'à la précision des mesures (0.5 à 1%) nous retrouvons dans l'électrolyte tout le potassium initialement présent. C'est aussi une réponse à la question des « tiny droplets ». Une fois le régime de fonctionnement bien établi, j'estime personnellement qu'il n'y a pas d'entraînement significatif de gouttelettes.(<<1%).

Chaque essai de 20 à 30 minutes fait disparaître environ 1.3 g de Tungstène. Une bonne part tombe au fond du bécher (beaker) , peut-être 50%, sous forme de débris métalliques. Nous n'avons pas conservé ces fonds de bécher, mais, là encore, les résultats d'échauffement en terme de réactions chimiques liées au Tungstène sont négligeables comparés aux échauffements anormaux constatés.

Tu écris, Ludwik : « ne laissons pas le mieux être l'ennemi du bien ». J'ajoute dans le même esprit : « ne coupons pas les cheveux en quatre ! ». Je crois qu'il est temps de publier nos résultats de Colorado 2. Nous espérons pouvoir remplir tous les blancs que tu laisseras dans ton draft. J'espère que cette expérience simple donnera l'idée à des experts de la fusion froide de confirmer nos dires et d'apporter leurs propres améliorations. PS: I hope, Richard, you read the french. ..Sorry, if not !

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59) Ludwik --> Pierre and Richard (6/15/06):
1) In my opinion, as I wrote before, our paper is not worth submitting to a mainstream journal. A good appendix, signed by a reputable electrochemist is essential. That appendix must convince other experts that excess heat we measured cannot be attributed to well known chemical reactions. His words will be taken seriously, my words or your words will not.

2) Pierre, why is Fauvarque not one of those to whom your message was sent? Are there any problems with his willingness to be fully responsible for the thermo-chemical Appendix 3? I wander why he did not comment on what I wrote about his appendix. Are we going to have separate appendices for energy and for tiny droplets? Should I interpret this as an OK for posting his appendix on the CMNS list (after replacing 1.94 m by 0.388 M)? My expectation was, and still is, that each of will write about his appendix, before we show it on the list.

3) Richard, Pierre tells us that Paris3 experiments will be conducted by an enlarged team (people from Fauvarque lab and from Dufour lab at CNAM. Dufour lab is much better equipped for that kind of work).

4) Pierre also gives us a conclusion about the invisible droplets -- their contribution to the lost liquid was found to be between 0.5 and 1 %. That is a great news; I will include that sentence into the main text. The methodology used to establish this result must be described. We can do this in his long appendix or in the Appendix 2, as I suggested. Two or three paragraphs, that any chemist will understand, would probably be sufficient. Fauvarque's appendix does not mention the 1% conclusion; perhaps it is hidden somewhere between the lines.

5) Let us hope that Scott's data will confirm the very low percentage of droplets. Being familiar with his instrument I will add a paragraph or two about the second method to the Appendix 2 (or two Fauvarque's appendix) .

6) No, I am not trying to split a hair, Pierre. We are making an extraordinary claim. Our paper must be nearly nearly perfect in order to have a chance of being published. Please answer as soon as possible; it is frustrating to wait and not being able to finish the draft. I reached the point at which I can do nothing more by myself. I must be satisfied with contributions made by others. And I do not want to share an unfinished draft.

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60) Ludwik --> Pierre and Richard (6/15/06):
In reading Pierre's message more carefully I see that he did not say that droplets were 0.5-1%. He said that the contribution of droplets was much less that 1 %. That is better. The 0.5-1% was their maximum possible error in the statement that the "molarity of the remaining electrolyte did not change." That is a much weaker statement than comparing the 0.2 M of the electrolyte with the molarity of the condensed liquid. If the "much less than 1%" is true that the result should be "much less than 0.002 M." A good referee would certainly ask for a better method of determination of the percentage of droplets. He would also ask if the "much less than 1%" is consistent with the "0.5-1%" accuracy. We should anticipate such questions and answer them in the appendix. Do you agree with this? Does Fauvarque agree with this?

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61) Ludwik --> Pierre and Richard (6/15/06):
1) Pierre; I am terribly confused for the absence of comments about what I wrote to Fauvarque and to you (in two messages). Should I start discussing his appendix on the CMNS list, as we were going to do after discussing it among ourselves?

2) Meanwhile I prepared a message (see below) which will lead to such discussion. I will post it as soon as you and Richard OK it.

MY MESSAGE WILL SAY THIS:

On Jun 14, 2006, at 11:07 PM, Ludwik Kowalski wrote:

List of successful replications of Mizino-type excess heat:

1) J.L. Naudin see his website --> http://jlnlabs.imars.com/cfr/
2) V. D. Cirillo et al. see their ICCF11 report
3) J.F. Fauvarque et al. see their ICCF12 report
4) R. Slaughter et al. Colorado2 experiment (we are working on the report)
5) J.F. Fauvarque et al. -- Paris2 (preliminary results after Colorado2)
6) University Labs in Japan

Experiments in preparation or in progress:
7) R.Slaugther -- Colorado3 (preliminary observations of excess heat)
8) J.P. Biberian -- Marselles1
9) J.F. Fauvarque et al. + another lab at CNAM -- Paris3

10) S. Little -- Texas2

What about your lab? I am not going to list names; many researchers on this list are highly qualified to replicate Mizuno-type experiments. And some are they are likely to have what is needed. Working on nearly identical experiments, and sharing results from our non-patent-motivated work, we are likely to promote the CMNS field faster than working on separate experiments and keeping things secret. Naturally, one must be aware of possible dangers, as described earlier on this thread. . . .

I am slowly realizing that my suggestion would be acceptable only if we had convincing (indisputable) arguments that the reported excess heat could not be explained by well known chemical reactions. We need a straight forward thermochemical analysis of reactions that are expected to take place in GDPE cells. Potential reactants present are known and chemists, I expected, can calculate excess heats associated with each of them. It would be a matter of adding individual excess heats (some positive others negative, according to existing enthalpy tables and according to amounts of consumed reactants). I assumed that such analysis would show that the net excess heat from known chemical reactions is at least two orders of magnitude smaller than what was actually measured. But I have not seen such arguments, so far.

What I did see, in a Mizuno's paper (see below), was a discussion based on one potential fuel, tungsten. Is it reasonable to assume that the role of other potential fuels can be neglected, in comparison with tungsten? Mizuno seems to be making such assumption. But I am not a chemist and validity of that assumption is not obvious to me. I hope that reputable chemists will provide indisputable evidence that chemical excess heat is too small in comparison with what comes from a Mizuno-type cell containing the K2CO2 electrolyte. By the way, I do not think that reliance on authority of experts is forbidden by the so-called "scientific method." Where would we be if we had to validate everything by ourselves?

Here is how Mizuno et al. addressed the issue (in T. Mizuno, T. Ohmori, K. Azumi, T. Akimoto and A. Takahashi; “Confirmation of heat generation and anomalous element caused by plasma electrolysis in the liquid;” Conference Proceedings Vol. 70, ìICCF8î Società Italiana Di Fisica, Bologna, 2000, p. 75)

1) It is well known that 380 kJ of heat is released when one mole (183.85 grams) of W is consumed to produce H2WO 4.
2) In reality only 0.1 grams of tungsten was lost during a test in which excess heat was measured. That, could produce 0.207 kJ of heat. The amount of excess heat measured, 54.4 kJ, was found to be considerably larger. Thus no more that 0.4% of excess heat measured could be due to chemical consumption of tungsten.
3) This 0.4% fraction, they wrote, is actually an exaggeration because the 0.1 grams of tungsten lost from the cathode was found at the bottom of the cell in the form of pure metallic powder. In other words, tungsten did not react with oxygen, it was simply removed “by hydrogen corrosion as well as heat damage.”
4) Another chemical reaction taking place in the cell, during plasma electrolysis, is “the decomposition of the carbonate in a water solution.” That reaction is said to be endothermic; it removes 274 kJ of heat per mole of K2CO3. Is it true that tungsten is the only well known exothermic reaction that might contribute to excess heat measured in Mizuno-type cells?

By the way, loosing only 0.1 grams of tungsten per experiment is not consistent with what I remember from Colorado2. It is also not consistent with more qualitative results from Paris2 experiments. Referring to these experiments in a private message, Pierre Clauzon wrote: "In each sequence of tests lasting 20 to 30 minutes we loose about 1.3 grams of tungsten. A sizable fraction of it, perhaps 50%, is found as residuals, deposited at the bottom of the beaker..... " Note that 1.3 grams is considerably larger than 0.1 grams reported by Mizuno et al. That seems to indicate that experiments in Colorado2 and Paris2 were substantially different from those described by Mizuno. Perhaps the shapes of electrodes have something to do with the difference.

In any case, what conclusion can be drawn from the information provided by Pierre? Following Mizuno et al., I will assume that tungsten is the dominant chemical fuel during the glow discharge electrolysis. I will also assume that excess heat is generated at the average rate of 50 W. That translates into 90,000 joules of excess energy (or . 5.62*10^24 eV) during 30 minutes. The 1.3 grams of tungsten, on the other hand, translates into 2.54*10^22 atoms. If

100% of these atoms reacted chemically, to generate 90 kJ of heat, then the rate is 121 eV per atom. that is already two orders of magnitude higher than what is usually associated with chemical reactions. If Mizuno is correct that tungsten in the residuals is pure metal, and if Pierre is correct that residuals contain 50% of the lost tungsten, then the number reacted atoms becomes two times larger and rate becomes 2*121=242 eV/atom.

Another way of using Pierre's data is to follow Mizuno's approach. The mass of the lost tungsten was 1.3 grams. This is 0.0071 moles. The expected chemical heat, due to reacting tungsten, is 0.0071*380 = 2.69 kJ. That is only 3% of the actually measured excess heat. The result would be 1.5% if only 50% of tungsten reacted, as estimated by Pierre. But what if the actual measurements showed that 90% of the lost tungsten is metallic, at the bottom of the beaker? That the result would be a small fraction of one percent. I hope the amount of metallic tungsten, in the residuals could be determined with the accuracy of 5% or so. Honest referees would appreciate our effort to do the best we can to show that excess heat we measured is indeed abnormal. Publishing a paper on excess heat makes sense only if the abnormality can be established. Abnormality is the central point of our paper and it has to be established as conclusively as possible. We are in no position to argue about nuclear or hydrino's origin of excess heat. That would call for much more information than we were able to gather. Comments and help would be highly appreciated.

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62) Pierre to Ludwik (6/17/06):
I did receive your last e-mails about our absence of comments, Fauvarque and me. I will do my best this w.e. and I will try to get the same from Fauvarque next week. Fauvarque was at the end of this week in Tunisia, for university exams and contracts . He is a quite busy man and please, accept our apology !

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63) Ludwik --> Pierre + Richard (6/17/06):
1) Thanks for the quick reply, Pierre. OK, I will wait. Did Fauvarque talk with you about my comments?
2) Does he agree with the suggestion to have two shorter appendices, one about chemical excess heat and one about tiny droplets?
3) Did he say that he will calculate for us excess heats of chemical reactions (how many kJ each) on the basis of known amounts of reactants, and on the basis of enthalpy tables used by chemists?
4) I will not start discussing his appendix on the CMNS list. But what do you think about the massage I sent you yesterday. Fauvarque is not mentioned in it; I only quote what you wrote about tungsten. Can I post that message today? It might generate helpful comments.

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64) Richard --> Scott (6/17/06):
Ludwik, said you can do X-ray fluorescence on the samples to find out if there is any K2C03 or just K in the samples. We are trying to rule out the possibility of droplets or mist being part of the reason for the over unity COP. Sample 4 is just the electrolyte so it should have 0.2 M K2C03.

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65) Ludwik --> Richard (6/17/06):
Just relative heights of potassium peaks (in condensed liquid versus original electrolyte) would probably be enough, provided the geometries are identical. Also an error bar on the ratio. Scott might suggested something else; he knows exactly about the context. (P.S. Actually, this would be true only if the ratio was 0.1 or more. For a much lower ratio, which we hope for, Scott will probably dilute to original electrolyte, for example, by a factor of ten, in order to minimize the error.

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66) Scott --> Richard + Ludwik (6/17/06):
It may be next Friday before I get a chance to go see the XRF system. It's not at my lab anymore. .. but it's at the place my daughter works. Meanwhile, I want you to calculate what K concentration in these condensate samples would it take to negate, say, a 1.10 COP. That value will give me something to "shoot for". It will tell me whether this is an easy analysis where I have plenty of sensitivity, a borderline analysis where I have to optimize the excitation conditions and use long count times to get the necessary accuracy, or a hopeless analysis where I cannot even detect an

important (to this experiment) amount of K.

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67) Ludwik --> Scott + Richard +Jean + Gerard (6/18/06):
Let me try to answer your question. In the draft of his appendix Fauvarque reported the molarity of K+ ions as 0.388 -- determined by the method of the HCL titration. That is only slightly less than the ideal 0.4 M for our totally ionized K2CO3. To prepare the 0.2 M solution of that salt Richard probably used 26 grams per liter or 26 mg/ml. His condensed liquid samples were from experiments that yielded the COP of 1.1.

If such COP were an illusion due to tiny droplets then the concentration of K+ in samples of condensed liquid would be close to 2.6 mg/ml. To negate the illusion, the condensation would have to be below 0.3, hopefully 0.1 or less. Is the 0.1 mg/ml too low for your instrument? My expectation is that Fauvarque will improve the appendix, and that it will be posted on the CMNS list, probably in several days, after we have a chance to discuss it privately.

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68) Scott --> Ludwik (6/17/06):
0.1 mg/mL is about 0.1 mg/gm = 0.0001 gm/gm = 100 ppm. No, that's not too low. I should be able to get a detection limit down below that somewhere, maybe 20 ppm or so. Thanks. I will need to make a more dilute known standard. I'll probably make up a 2500 ppm standard. No problem. I'll get back to you guys later. BTW, XRF measures total K in the sample, regardless of ionization state, etc.

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69) Ludwik --> Richard, Pierre, Gerard, Jean (6/17/06):
Forwarding the above message from Scott.

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70) Richard --> Ludwik (6/20/06):
This is the first real data from Colorado #3. This is about one hour of continuous data. The voltage and current are normalized using 300volts and 1.333 amps. This just so all the info is on a single plot. The sharp increases in current are where I had to push the cathode down because it had burned away. At about data point 1100 I increased the voltage from 300 volts to 320 because the COP was low. I'm not sure what to make of point 1280 where the current suddenly drops and the COP jumps 1.35. You can ponder the graph like I'm doing and let me know what you think.

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71) Ludwik --> Richard (6/20/06):
The fact that you can run for one hour is very impressive. But the main COP is about 1.05. It is too early to comment on this, or to worry about this. I am trying to finish our Colorado2 paper. My suggestion is to include Scott in the list of authors; his XRF result will be a significant contribution. If he refuses then we will simply thank him for help, at the end of the paper. I would prefer him to be a coauthor. But neither you nor Pierre responded to this. Does it mean you have some reservations?

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72) Richard --> Ludwik (6/20/06):
It's fine to include Scott as a coauthor. Just give him the chance to decline. I suspect he will be reluctant to be a co-author without having help take the data.

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73) Ludwik --> Richard (a comment on a picture he sent me, 6/20/06):
COPs are trying to get up when both powers are going down. Is this significant? Perhaps using lower powers is worth trying. What is the nominal diameter of your cathode?

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74) Richard --> Ludwik (6/20/06):
Interesting question! I'm using two 0.040 dia wires twisted together with 1 twist per inch. I had order some wire so I

could have longer lengths this was what they sent as a sample. Using two wires got me closer to the original diameter but gave a lot more surface area.

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75) Ludwik to Richard (6/20/06):
I need some information for our Colorado2 paper.

1) About capacitors in parallel with the cell.
a) Was it one or more? How many?
b) Was it 500 V and 470 microF each? If not then how many for each?

2) About the little ac kWh-meter (the one we used for the ohmic heater, when it was also plugged in).
a) What model was it? What manufacturer? Was it from Radio shack?
b) what was the approximate cost?

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76) Richard --> Ludwik (6/20/21):
There are two 3100uF 450Volt spraque capacitors
The little plug in watt meter is a Kill A Watt model P4400 designed for seeing how much power household appliance use.
The meter is about $30.00
http://www.spytown.com/kilwatwatkil.html

The other AC watt meter is a DMMetering DRM75A
http://www.dmmetering.com/product_groups/spd.htm#

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77) Ludwik --> Pierre + Richard (6/21/06):
Ten days ago I made a suggestion which was forgotten in our subsequent correspondence. I asked about information on a test that was not performed. "Can one of you perform the following test:

a) Start the experiment as usual (by increasing the voltage and performing the first measurement of COP, the one that should always be rejected).

b) Then turn the voltage off suddenly, disconnect the cables, and measure the voltage between the electrodes. We believe that the voltage will be zero. But this should be checked. It will be useful to describe this test in our paper. If the voltage is nearly zero then we will be able to rule out a possibility of a battery-like effect (storing energy during the preliminary steps and releasing it when the COP is measured). That kind of objection can be made by a reviewer. Our answer, even without the suggested test, would be that stored energy would have to be several times larger than 30 kJ (100 W times 300 seconds case after case.) But reporting that we performed the test will show that such possibility was not ignored.

In fact, I would not be surprised to see a small remaining potential between the electrodes. If you observe this then treat the cell as if it were a capacitor and calculate the energy stored. Use a two-trace oscilloscope to see the v(t) and i(t) exponential pulses (slow discharging). That what I would do to find the stored energy. I am 99.9% sure it will be much much less than 30 kJ. Perhaps we will be able to say that stored energy turned out to be 0.01 kJ, or something like this."

What do you think about this suggestion? In my opinion our Colorado2 paper would be more difficult to reject if we say that an attempt to discover a battery-like effect was made but results were negative," or something like this.

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78) Richard --> Ludwik (6/21/06):
Will do.. I'm sure the voltage goes to zero in a very short time but I'll get some real data. I've done this all the time

when I shut down the supply and the voltage always drops immediately.

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79) Ludwik --> Richard + Pierre (6/21/06):
On Jun 21, 2006, at 8:20 AM, Richard Slaughter wrote:

1) That is good, Richard. Try to get the energy actually delivered through a battery-like effect, even if it is only 5J, or much less. Numbers are always more convincing than qualitative statements, like "much smaller than ..." If the number is smaller than what can be measured then give the upper possible limit.

2) I do not know why Fauvarque is not replying. Are you giving him all messages as soon as they arrive, Pierre? How is he reacting to my questions and comments? We need to hear from him, the more the better.

3) Other electrochemists on the CMNS list, like McKubre, Oriani, Szpak, Mizuno, Biberian, Zhilov, etc. are also not responding. Why is it so? The same is true for material scientists, like Storms and Dash. I am not comfortable in such unexpected situation.

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80) Ludwik --> Pierre + Richard (6/22/06):
1) Pierre, did you receive the message I sent you yesterday? In that message I asked why Fauvarque is not replying to my comments and questions about the appendix. Now you are not replying. Did my questions create some problems? Were they really so silly that JFF decided to ignore them?

2) According to the appendix only 1 gram, out of 18 grams lost in a typical experiment, was in the form of droplets. That is 5.6%. Can I say, in the main text that due to droplets the mass lost m must be reduced by 5.6 +/- 1 percent? That will lead to the about 5% correction of COP (1.17 instead of 1.24). The 1% error is based on this sentence from the appendix: "The precision of the titration can be estimated to be roughly 1%" Am I interpreting this sentence correctly? If not then what should the bar of errors be for the 5.6% ?

Why am not certain about the 1%? Because a pH value is x, from the 10^x. Was the 1% an error for x or was it an error for pH? A one percent change of x would produce a much larger change in pH. That is why I am not sure that the 5.6 +/- 1 is a correct interpreation of JFF's sentence. Not being a chemist I need an error on the percentage of droplets, not an error on some intermediate result from which the percentage of droplets was calculated.

3) Scott will probably measure the content of K (in condensed liquid) tomorrow. Will his result confirm the 5.6%? If so then the issue of tiny droplets being responsible for the big part of our excess heat will be put to rest.

4) The only remaining issue will be to convincingly show that corrections to the COP due to chemical heat (positive and negative contributions combined) are also very small, probably much less that 1%. Where do we stand on this issue?

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81) Pierre --> Ludwik (6/23/06)
Bonjour Ludwik, Je retransmets à JFF tous les e-mails le concernant... Mais c'est une véritable étoile filante en ce moment ! Il est en ce moment en Russie. Lundi, j'essaierai de le voir le matin, car dès l'après-midi, il repart pour Grenoble pour plusieurs jours ! Nous sommes en pleine période d'examens en ce moment.

Je t'envoie un peu plus tard mes réponses plus détaillées à tes questions. Mais dès à présent, je peux te dire que lors d'une expérience avec Dufour hier Jeudi, on a mis un papier chimique dans la vapeur pour vérifier s'il y avait des tiny droplets d'électrolyte. Je persiste à te dire que c'est très très faible et que les valeurs que tu tires du papier de JFF sont des bornes très supérieures! Pour ma part, ce problème est inexistant !

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82) Ludwik --> Richard +Pierre (6/23/06):

1) Richard, Pierre forwards all relevant messages to JFF, who is now in Russia. He will try to see him on Monday morning. On Monday afternoon JFF will leave for a several-days-long visit to Grenoble. It is an exams period in France. Pierre will comment on what I wrote a little later. He wrote: "But I can tell you one thing immediately. The experiment with Dufour [Paris3] started yesterday. We placed a chemical paper into the escaping steam and did not observe any droplets of the electrolyte." Pierre is strongly convinced that the number I extracted from the JFF's appendix are highly exagerated. As far as he is concerned, tiny droplets is not something that we should worry about.

2) But a referee will have right to ask for numbers. What I saw in the appendix was 0.005 M translated into 14 grams of electrolyte. Yes, it was given as an upper limit. But that did not tell me that I should replace 14 grams by 0.14 or 0.014 grams. The upper limit usually refers to what was possible to measure with a given method. Telling the referee that droplets represented much much less than 14 grams of the liquid will not be sufficient. The answers will be "how do you know that it should be 0.14 or 0.014?" or "how do you know it was not 13 grams?"

3) Pierre, what is the realistic limit of detection of tiny droplets when chemical paper is used, as you did yesterday?

4) My suggestion is to design an experiment in which the percentage of the electrolyte in the escaping liquid, for example 1%, is known in advance but not to the person performing the measurements. Measuring the same thing several times (to get the mean value and the standard deviation) would be one way to convince the referee.

5) No, I have no idea how to design an experiment in which the percentage of tiny droplets escaping from the beaker can be controlled. Some kind of a sprayer of pure electrolyte, injecting droplets into steam from pure water, would have to be invented.

6) Yes, I know that most often referees do not ask for numerical data about everything. But we are making an extraordinary claim in a paper that should be "difficult to reject." That is why extraordinary efforts are necessary. Do you agree, Pierre?

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83) Ludwik --> Scott + Richard + Pierre (6/23/06):
1) Scott, I hope other things did not delay you. When you do perform the XFR experiment please try to measure the concentration several times. This will give us not only the mean value but the standard deviation (random error) as well. Also try to estimate the systematic error. We are making an extraordinary claim and referees are expected to ask a lot of unusual questions. In my opinion, your contribution deserves a separate short appendix. Please write if for me.

2) Fauvarque is too busy now to answer questions about the appendix. I am not very happy about this. He will probably give us full attention after the exams in France are over, probably in a week or so.

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84) Richard --> Ludwik (6/23/06):
Hopefully we will be getting numbers from Scott. His detection is less than 20ppm range. Don’t you think this is sufficient with added comments about the chemical paper? We should be able to get a spec on the paper for detectable limits.

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85) Ludwik --> Richard (6/23/06):
Yes, but having a conclusion based on two independant methods would be better. Let us wait and see what will be found by Scott.

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86) Scott --> Ludwik + Richard + Pierre (6/23/06):
I have not found the time to go visit my XRF analyzer yet. However, I have a nice Mettler balance so I conducted a simple dissolved solids test on the samples you sent. First I weighed 4 Al weighing cups. Then I dispensed some of each sample into each cups. I used a new disposable pipette for each sample to prevent contamination. Then I weighed the wet cups. Then I gently dried the samples in a 70C oven for 4 hours This gentle drying should leave the

salt as K2CO3+1.5H2O. I recorded the first set of dry weights. Then I returned the samples to the 70C oven for another 8 hours. I recorded a 2nd set of dry weights. Here are the results:

[I am extracting from Scott's large table: The mass of solid deposit from each cc of condensed wet steam was 1.68 mg. That was the mean for the samples #1, 2 and 3, after 12 hours in the oven. Individual results were 2.06, 1.15 and 1.84. This leads to the standard deviation of 0.47. For the sample #4 (the original electrolyte) the mass of the solid deposit was 27.4 mg per cc. What does this mean? Let me begin with the original electrolyte. The 27.4 mg/ml translates into 27.4 g/liter. This is in very good agreement with 26.5 grams of K2CO3 that was put into distilled water to make the 0.2 M solution.

Each cc of the solution is essentially 1 gram. That solution was supposed to be from pure steam. Finding 1.68 mg of silid deposit, per one gram of condensed liquid, is significant because 1.68 is 6.13% of 27.4. In other word, our evaporative losses were exaggerated by 6.13 %.This 2.45 times larger than the upper limit in Fauvarque's appendix. And, according to last Thursday test in Paris3, the upper limit should be changed to a much lower number. What is the new upper limit?]

For the mg/mL column, I assumed that the solution density was 1.000 (same for the molarity calculations) As you can see, the condensate samples are NOT very free of dissolved solids. Ludwik calculated that 2.6 mg/mL would "erase" the apparent excess heat and these samples range up to 2 mg/mL. Note that the stock solution came out very close to 0.2M in this analysis.These results make it even more important to go to the XRF analyzer to confirm that this is actually K in the samples. I'll do that sometime this weekend.

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87) Ludwik --> Scott + Richard + Pierre (6/23/06):
Scott wrote: "These results make it even more important to go to the XRF analyzer to confirm that this is actually K in the samples. I'll do that sometime this weekend." Yes, indeed. I suspect that some of the net mass is tungsten. The original solution comes as expected (0.2 M) because it does not contain tungsten. If that suspicion is correct then tungsten peaks will appear in the XRF spectra. Knowing concentration of tungsten might help us to deal with the issue of "chemical contributions to excess heat." According to JFF only ~50% tungsten lost by the cathode is recovered as metallic particles at the bottom of the cell. Where is the rest? You might be able to answer this question, Scott.

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88) Ludwik --> Scott + Richard + Pierre (6/24/06):
1) Yesterday, responding to the above, I speculated that solid deposits (samples 1, 2 and 3) might consist of mostly tungsten rather than potassium. Thinking about this again I realized that this would be equally bad for our excess heat conclusion. It would show that the measured lost mass m, that we used to calculate evaporative losses, was not pure water only. The m was overestimated by a significant fraction.

2) My suggestion is to collect samples of condensed steam in Paris3 experiments (now in progress) and to measure the mass of solid deposits in them, like Scott did. The mass of the solid deposit would be zero for pure steam. Richard's scale is sensitive enough, I think, to measure the mass of solid deposits from condensed steam. Measuring the mass with 10% accuracy would be sufficient to confirm Scott's preliminary results. Is the mystery going to be solved in such disappointing way? That remains to be seen; perhaps some kind of mistake was made by Scott. He is a great experimentalist, but no one is perfect.

3) The XRF analysis will also be an independent confirmation. It might also be very useful in the context "chemical excess heat" corrections.

4) Suppose we become convinced that the COP=1.24 (st.dev. 0.13) was mostly an illusion. What should then be done? In my opinion it will be our obligation to publish the result, for example, at the upcoming conference in Italy. Would you be willing to present the paper there, Pierre? Trying to publish a paper on "unexplained excess energy" in a mainstream journal would be out of question.

5) I hope that other researchers: Mizuno, Naudin, Iori, etc., will also reexamine their results. After all, it would not be

difficult for them to do what Richard and Scott did. We should have performed similar analysis before collecting data on the COP. Please share your reflections on that subject.

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89) Scott --> Ludwik + Richard + Pierre (6/24/06):
Agreed [about "tungsten rather than potassium"]. BTW, in one of my Mizuno runs seven years ago I did measure W in the electrolyte and in the swarf (particles on the bottom of the cell) and observed that only 5% of the W lost by the cathode ended up as swarf. 95% of the lost W went into solution. See:

http://www.earthtech.org/experiments/Inc-W/2ndtry/run6.html

[L.K. wrote "but no one is perfect."] Indeed I am not perfect. The list of my own past measurement errors is a mile long. The dry solids measurement does have a few peculiarities. You saw the difference between the 4 hrs and 12 hrs of oven time. All of the sample were HEAVIER after 12 hours than after 4 hours. That may be due to further chemical reactions in the oven involving the Al weighing cups. Each of the Al cups now has a dark stain on it where the solution dried out. ..some sort of oxidation caused by the basic solution, I suppose. I don't expect these stains to be very significant. We'll see when I get the K analyses done (looks like Monday is my earliest opportunity to go see the XRF analyzer). [L.K. also wrote about "our obligation to publish the result."] .But, of course.

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90) Ludwik --> Scott + Richard + Pierre (6/24/06):
Perhaps it would be better to dry samples at room temperature. Doing this under the vacuum (or in a desiccator filled with clean air) would also reduce a chance that something from air reacts with Al and contributes to the net mass difference. Perhaps Al was not the best choice. Just speculating.

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91) Richard --> Ludwik + Scott (6/24/06):
It will be very disappointing if W in the steam turns out to be the cause of the excess heat and of course we should still publish the results. The original Mizuno experiment was a mostly closed cell with little evaporated losses. My initial Colorado #3 experiment is encouraging but very inconclusive. Scott you did not see any white deposits in the evaporated test? At least it did not look like K2C03 deposits? Scott thanks for all the extra effort!

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92) Scott --> Richard + Ludwik (6/24/06):
I am afraid it does look like K2CO3, Richard. Here's the four Al weigh boats in the dried condition. You can see the darkening of the Al but around the outside there is a definite white deposit. [The jpg file attached] The real white one is #4, the stock solution. The boats are about 4cm in diameter.

BTW, I weighed them again just now after about 48 hours at 70C (I stored them in my oven which is left on at 70C normally) and they were all within 0.1 mg of the "after 12 hours" weight....except for #4 which was 0.4 mg heavier. It should be noted that these samples begin gaining weight as soon as you remove them from the oven. I measured the rate on one of the samples and it was about 0.4 mg/minute. I suppose that's water vapor attaching itself to the Al surface and to the K2CO3 as things cool off.

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93) Pierre --> Ludwik + Richard (6/24/06):
I will not add any remarks on what I have already said to Ludwik about the tiny droplets. I hope to see to-morrow Pr Fauvarque for that and may-be other points. ..Do not hesitate, Ludwik, to recall me what are your main concerns.

I would like now to draw your attention on the following question : storage and un-storage of energy, due to the transient between two levels of energy. I was not aware of this effect, because of our lack of registered measurements before. Of course, we have to take into account this effect in analysing the results.. or at least to wait a sufficient time (for us about 5 minutes) before taking into account the values obtained. It is easy, Richard, to see what I mean. You have just to make an experiment with only the ohmic heater and you choose various levels of power between 200 and

800 w for example. At least 15 to 20 minutes for each level, and you plot the COP values (in theory = 1.0). You go of course up and down. What surprises me is that it seems contradictory with the fact that the thermal losses are quite constant on this energy domain...

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94) Ludwik --> Pierre + Richard (6/24/06):
The only thing I would like to have, as soon as possible is the new upper limit on the percentage of droplets based on the Thursday experiment. It would be desirable to know this before we hear the results of Scott measurement of the concentration of K+. On Friday you wrote that the new upper limit was much much less that 1%.

My main concerns were enumerated in messages about the JFF's appendix. You wrote that all of them were forwarded to him. Can I be certain that he really read them carefully before deleting? I would be happy to send these messages again, if you think that this might help. And here is my comment about your "two levels of energy" idea. Anticipating that a referee might also think about a possibility of some kind of storage we should address the issue in our paper. I think I will write something like this: "The possibility of significant short-time storages and releases of energy can be ruled out the basis of high reproducibility. The mean COP of 1.24 and the standard deviation of 0.13, from 41 tests, would not be possible if such mechanism played a significant role."

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95) Richard --> Pierre + Ludwik (6/25/06):
You are seeing the same problem that I'm working. What is your heat loss? I removed all my insulation so that the time lag as the input or output power changed would be less about 2 minutes without the insulation and about 5 minutes with insulation. I found that from 300 watts to 800 watts the heat loss is directly proportional to the delta temperature of the heat exchanger. Lowering the Input power below 275 watts you really see the stored heat energy in the electrolyte and it takes a long time to dissipate. I'm looking into ways to greatly reduce the amount of electrolyte used this should improve the response. Is this a problem with all calorimeter that has a heat storage mechanism?

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96) Pierre --> Richard + Ludwik (6/25/06):
We got a small heat loss of about only 43 watts due, I suppose, to a very good insulator (glass wool). This heat loss seems constant between 250 w to 800 w.I think that we must try to have longer and quite constant power runs by trying to keep the cathode length as constant as possible by motoring it. So this storage effect will no longer be a real problem.You can see our insulated long beaker in the joined picture and the way we choose for diminushing the weigth of the apparatus in order to be able to use our balance ...

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97) Richare --> Pierre + Ludwik (6/25/06):
This all sounds good. You are doing the same things that I'm trying. I making a screw device so I can screw the cathode down into the electrolyte. The screw will let be control how much cathode is lower into the electrolyte.

98) Pierre --> Ludwik + Richard + Jean + Gerard (6/26/06):
I just saw Pr Fauvarque coming back yesterday from Russia. As you know, he is very busy and will be free only by mid-July. We discussed however your main concerns. I noted the following points: (a) The value of 1% is classical for a chemical titration. Nothing to see with the amount of lost electrolyte in tiny droplets. As you know, my own opinion is that tiny droplets are of a negligible amount in the evaporated water, but we were not able to measure it accurately. What is the point of view of Richard on this point? (b) You have participated to the Boulder experiments (Colorado 2). They have confirmed the Paris 1 conclusions and this was a good second step. We have no more to add at this time in our conclusions than you may have to add for the Boulder experiments case. (c) To answer with a great accuracy your demands needs to do again at least one month of tests. We do not have the material and financial means to do so at this time in our laboratory. I am not very pleased to send you this uncompleted e-mail. But, I hope that the Scott tests will give you the answers you are waiting for...
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99) Ludwik --> Pierre + Jean + Richard + Gerard (6/26/06):

1) I am also not very pleased. Should the first section of the appendix be eliminated from the draft we received? I counted on two independent determinations of the percentage of droplets. Now we have to rely on Scott only.
2) What about the second issue -- contributions of chemical heat to our COP=1.24? This seems to be a theoretical question based on thermochemistry of dominant chemical reactions. Should I expect this kind of input from Pr. Fauvarque in July? A delay of three weeks is not a big deal, my draft will have to wait. I am convinced that a paper about excess heat must contain statements that what we measured was not due to well known chemical reactions, or to droplets. The manuscript would certainly be rejected without quantitative information about this.

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100) Ludwik --> Richar + Pierre (6/26/06):
Here is a paragraph I just composed for my anticipated draft. The ??? will be replaced by the number provided by Richard. I also believe that the number will be very small, but a paper "difficult to reject' needs numbers. Anything else I can add on this topic?

"Can excess heat measured be atributed to some kind of energy accumulation process? To answer this question we considered a possibility that our cell acted as a recheargeable electric battery. In a control test the cell, after being connected to the power supply for several hours, was suddenly disconnected and discharged through a resistor. The amount of stored energy turned out to be ??? J. That is a small fraction of one percent of the excess heat measured in a typical test. We concluded that the battery-like effect, if any, had a negligible effect on the reported value of COP. Another possibility considered was short-time accumulations and releases of thermal energy. That hypothesis was ruled out the basis of high reproducibility. The mean COP of 1.24, and the standard deviation of 0.13, from 41 tests, would not be possible if such mechanism played a significant role. Furthermore, significant random thermal accumulations and releases would be associated with significant temperature fluctuations during consecutive tests. The actually observed temperature fluctuations, about one degree or so, indicated some thermal instability. It probably contributed to the size of the standard deviation."

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101) Ludwik --> Richard + Pierre (6/26/06):
I suspect the resistor is too small. Your two parallel capacitors have 6200 microfarads. Discharging them through a 500-ohms resistor should take slightly less than 10 seconds (R*C=3 seconds).
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102) Scott --> Richard + Ludwik (6/26/06):
I got to the XRF machine today. As usual, the story is not as clear as we would like it to be. So that you can join in the fun, I have attached Excel spreadsheets that contain the raw spectra I obtained today along with some crude calculations (this XRF machine does not have fancy software). The two sheets are K.xls and W.xls for potassium and tungsten analysis, respectively. For K analysis, I used no filter in front of the x-ray tube (so that the softer x-rays would get to the sample) and 10 kV on the anode. Unfortunately, this spectrometer has an air path from the tube to sample to detector so Ar x-rays interfere with the K analysis. In fact, the Ar K-beta is right under the K K-alpha...the most intense K x-ray. So, in fact, I used the Ar K-alpha as a measure of the background under the K peak.

Important: the data show about half as much K in the stock solution than I expected from the 0.2M preparation. And this is based upon a known standard that I prepared today, gravimetrically. More about this later. Despite this setback, there is decent agreement on the ratio of K in the condensate samples to K in the stock solution and the corresponding ratio of % solids in the condensate and % solids in the stock soln.

And then there is the W analysis. As you can see, there are large W peaks in sample #4 which tells me that sample #4 is not virgin stock electrolyte but USED stock electrolyte. At the moment, I do not have any quantitative results for the W but I did calculate the ratio of W in the condensate samples to W in the stock soln. The values match the other two ratios mentioned above pretty well. All three sets of ratios can be found in colored fields in K.xls on the "calculations" worksheet.

I now suspect that the %solids result we got that matched the expected 0.2M so well was a coincidence. The solids I'm weighing must be about half K and half W. Hmmmm..that's a LOT of W dissolved in there. .. I wonder if that's

possible? Maybe there's another component to the solids I'm seeing when I evaporate these samples. And there's still the possibility that I've done something wrong with the K analysis... I'll continue to work on it. If I'm right, though, how did the stock solution lose so much K? Maybe it went out in the mist droplets. Did you guys use this solution for several runs, making up to the same volume each time with pure water? If so, that could explain the reduced K content I am seeing.

A small point that we need to straighten out: Is 70C hot enough to make the K2CO3 anhydrous? It's supposed to be KCO3+1.5H2O when just dried from solution but, of course, there is some point at which it loses all its water and becomes K2CO3. Richard, did you use anhydrous K2CO3 in your preparations? If not, did you calculate so that the solution came out 0.2M K2CO3 and not 0.2M K2CO3+1.5H2O?

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103) Ludwik --> Scott + Richard (6/26/06):
For the time being I will take Scott's numbers on their face values.

1) The mean and standard deviations from three samples (for K) are 0.073 and 0.015. That translates into 7.3% droplets based on K alone.

2) The mean and standard deviations from three samples (for W) are 0.070 and 0.014. That translates into 7.0% droplets based on W alone.

3) Assuming nothing else is present in the samples of electrolyte one would conclude, on the basis of these preliminary results, that the percentage of droplets was 14.3 %. In other words, this seems to be the lower limit. Right? Standard deviations are about 18% of the mean values. In other words the percentage of droplets would be reported as 14% +/- 2.5%.

104) Scott --> Ludwik + Richard (6/26/06):
No, I don't think these two 7's should be added. Instead they just support each other. Each one is an independent measure that says that about 7% of the condensate is due to droplets. And the same goes for my oven-drying tests. ..they also show around 7% (with considerable variation).

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105) Richard --> Scott + Ludwik (6/26/06):
I did not use anhydrous K2CO3 but K2C03*1.5H20. I did not understand why my bottle of K2C03 was different from Pierre until just now when you used the term anhydrous and it dawn on me..... So it is 26.7 grams of K2C03*1.5H20 in one liter of H20.

The solution was a fresh solution but had been about 10 run adding about 50 grams of fresh water for each run. It took this many runs to get positive COP's and enough condensate from a single run. [P.S.So sample #4 is the final electrolyte after all the test.]

I would estimate about 4-5 inchs of W 3/32 dia rod used during the tests to get the condensate.

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106) Scott --> Richard + Ludwik (6/26/06):
At 10:01 PM 6/26/2006, Richard Slaughter wrote:
[1) You wrote ”..... So it is 26.7 grams of K2C03*1.5H20 in one liter of H20]. OK, that's good to know....your K content was a bit low to start with, then. You should have used 0.2 * 165.2 = 33 grams of your hydrated salt to get 0.2M K2CO3.
[2) You wrote “ many runs to get positive COP's and enough condensate from a single run.”] Very interesting. so, with a steady loss of K via droplets, your final K concentration could easily be significantly lower than your initial K concentration.

[3) You wrote: “I would estimate about 4-5 inchs of W 3/32 dia rod.”] OK, I just weighed a new, 6" rod and got 14.46 grams. That's 2.41 grams/inch (excuse us, Ludwik, while we wallow miserably in this American morass of mixed unit systems...:). If 4.5" was used, that's 10.8 grams of W, most of which went into solution according to the seven year old measurements I referenced recently. That's at least comparable to the K content. sortof.

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107) Ludwik --> Richard + Scott (6/27/06):
My recollection is that you mentioned the COP's close to 1.10 for runs during which samples were collected. Is this correct? At the time I did not consider this to be important (10% of excess heat would also be spectacular if the contribution from droplets were below 1%). But now this fact might become essential. You probably did not use the same vessel and the same geometry as that used when we worked together.

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108) Ludwik --> Pierre + Richard + Scott (6/27/06):
Pierre:
1) Preliminary results from the XFR analysis seem to confirm what Scott reported last Friday. I expect his conclusions to be sent to us today or tomorrow.

2) Meanwhile I would like to share with you the very beginning of my draft. I no longer think that it will become a publishable manuscript very soon. But I do want to send it to the CMNS library. It will be preserved there for posterity. This will be done after the appendix is revised, if necessary, by JFF.

3) Please confirm that nobody who contributed to our work was forgotten. Naturally, I will show you the entire piece when it is finished. We have to wait for JFF to be available again.

4) I really think that what Scott did by simple mass analysis should be done independently in Paris and in Bolder. Too much is at stake, Scott's conclusion must be confirmed before we finalize its validity.

5) Are you planning to go to Italy, Pierre? I suggest that we inform the meeting about the status of our work, especially, if we are no longer as enthusiastic as we were a week or two ago. Would you be willing to do this in the name of all of us, Pierre?

Is the unexplained heat during high voltage electrolysis real?

Richard Slaughter, Pierre Paul Clauzon, Jean Francois Fauvarque, Ludwik Kowalski, Gerard Jean-Michel Lalleve, and Scott Little.

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109) Richard --> Scott + Ludwik (6/26/06):
The COPs were 1.10, 1.15 and 1.20. You are correct the vessel and geometry were different. I will work on confirming Scott’s weight measurements. I think I will let the steam saturate a piece of filter paper and then weight the paper after it dries. Scott if I sent a piece of filter paper saturated from the steam is that a large enough sample for the XRF machine? It would be sent wet. Could you soak and/or rinse it to collect the sample? I can also collect samples of electrolyte after each steam sample. These results are important that we should run a second test to confirm these results.

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110) Scott --> Richard + Ludwik (6/27/06):
Richard, I need to know the total volume of electrolyte in your cell. I am working on getting actual W concentration results from the XRF data and I need to see if they make sense with the quantity of W you eroded into solution.....[A paper filter] does not sound like an ideal sample, Richard. A thin layer like that can be analyzed via XRF but with overall reduced sensitivity for the analyte. I need about 15 mL of liquid to fill up the sample cups we use. .. so rinsing and collecting the sample from a filter paper would likely result in a significant dilution of the sample. That would not

be good as we are already working at low concentrations. I need bulk liquid samples, at least 15 mL of each.

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111) Richard --> Scott + Ludwik (6/27/06):
Ok, I was just thinking that I could hang the filter paper above the cell and let if absorb the liquid from the steam. This way I don't have to worry about contamination from the plastic bottles used to collect the electrolyte. I did clean the bottles and interesting I didn't see any signs of electrolyte on the bottles after they dried. There were lots of water droplets on bottles after collected the condensate. The amount of electrolyte I was using was very close to 1 liter. I always started with 1 liter and as the electrolyte evaporated would add more up to the 1.0 or 1.1 liter mark. I know I used only one W rod for all the test collecting the condensate. The 4 inches I mentioned would be the absolute max. The absolute min would be 2.5 inches.

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112) Ludwik --> CMNS list (6/27/06):

1) Due to unforeseen delays, the draft of the paper I was working on could not yet be posted. But what I can do, while waiting for additional input, is to show the beginning of my draft. Details and modified conclusions must wait; they will probably be different from what I anticipated when the introduction shown below was written. Here it is, followed by the list of references. . .

2) Nuances often escape our attention when we quickly read or listen to what others have to say. I was thinking about tiny droplets associated with boiling. My conclusion that such effect, or large systematic errors in measuring electric and thermal energies, cannot lead to COP>1.2 was based on the fact that the measured value of L turned out to be very close to 2260 J/g, in Paris1 experiments. But L was measured with an ohmic heater. Somehow I did not pay attention to this.

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113) Scott --> Richard + Ludwik (6/28/06):
Same spreadsheets attached but this time complete.

1. I used Richard's formula for the stock solution to calculate the original K conc in the stock solution. My XRF data shows that about half the K is now gone from the stock solution....! That is consistent with it leaving via the "steam".

2. I used Richard's estimate of 4" of 3/32 W rod consumed to determine that the stock solution should have been about 1% W if nothing had been lost. My XRF analysis shows the stock solution to be only about half that W concentration... again confirming that W was lost from the stock solution.

Both of these observations at least qualitatively support the XRF results that K and W are found in the condensate samples.

3. I added the COP values that Richard put on the sample jars to the spread sheet. There is not a good correlation between them and the K or W concentration in the condensate samples.

4. I computed the 1 sigma standard deviation in the XRF results. In the case of K, it is about 5% relative of the K conc in the condensate samples. In the case of W, it is larger, nearly 10% of the W concentrations we saw. That could be improved with longer count times but I think it is adequate as is.

Note, the non-zero W readings on the two water samples is a reflection of improper background subtraction, not of actual W contamination in the water. I could do a bit better on the W analysis but the results would be quite similar to what you already have.

In conclusion, it looks like approximately 7% of the condensate volume is due to liquid transferred out of the cell. Does that translate linearly to the COP bottom line?...i.e. that 0.07 must be subtracted from the apparent COP's. ..i.e. that a COP of 1.10 turns into 1.03 when this is taken into account?

I think that's the way it works but I could be wrong. Ludwik? Richard?

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114) Ludwik --> CMNS list (6/28/06):
I know that Scott, Richard and probably Pierre are busy to verify a tentative conclusion reached so far --> 7% of the lost liquid consisted of the electrolyte droplets. It was based on a simple test performed by Scott. About two weeks ago Richard performed three experiments during which the escaping liquid was condensed outside the cell. The COPs, measured turned out to be 1.2, 1.15 and 1.1. Condensed liquids, plus some amount of the original electrolyte, became samples #1, #2, #3 and #4, respectively. They were sent to Scott Little who has an XRF device able to perform elements analysis.

Note that this approach is very different from that described in JFF's appendix. Titration analysis (Paris2 experiments) was performed on the electrolyte remaining in the cell, not on the re-condensed wet steam. Before performing a sophisticated X-rays analysis, Scott did something much more simple. Here is a message to Richard that I received from Scott last Friday. " I have not found the time to go visit my XRF analyzer yet. However, I have a nice Mettler balance so I conducted a simple dissolved solids test on the samples you sent. First I weighed 4 Al weighing cups. Then I dispensed some of each sample into each cups. I used a new disposable pipette for each sample to prevent contamination. Then I weighed the wet cups. Then I gently dried the samples in a 70C oven for 4 hours This gentle drying should leave the salt as K2CO3+1.5H2O. I recorded the first set of dry weights. Then I returned the samples to the 70C oven for another 8 hours. I recorded a 2nd set of dry weights. Here are the results: [a table with numbers] . . . These results make it even more important to go to the XRF analyzer to confirm that this is actually K in the samples. I'll do that sometime this weekend."

Under ideal conditions sample #4 should show that the mass of the deposit is consistent with the molarity of the initial electrolyte. And samples 1, 2 and 3 should have zero mass, corresponding to 100% dry steam escaping from the cell. But that is not what was discovered by Scott. Masses in samples 1, 2 and 3 were not negligible. Comparing them with the mass in the sample 4 one can conclude that ~7% of the escaping wet steam consisted of pure electrolyte. In my opinion, everyone who is going to study excess heat from a GDPE cell (Glow Discharge Plasma Electrolysis) should first perform a similar dry mass test. I also think that those who reported on excess heat from GDPE cells, especially Mizuno, Naudin, Iorio, Richard, Pierre perform similar tests and report the results. It is prudent not to depend on what has been reported by a single researcher, even a very careful one, like Scott. In fact, I would not be surprised to find out that such tests were performed but were not reported.

Scott, can you please describe your X-rays analysis in the form of an appendix for our draft? I just read what you posted this morning and examined your spectra. They seem to confirm the results based on weights. But, as you indicated, absence of correlation between the percentages of droplets and the roughly determined values of COPs is disturbing. If droplets are responsible for the COPs>1 then samples from experiments with larger COPs must contain more electrolyte than samples from experiments in which the COPs were smaller. That was Richard's comment on what you wrote this morning. Only additional experiments can clarify this matter. My suggestion is to focus on this in Paris3 and Marseilles1 experiments. I am also disturbed by conclusions from Texas1 and Colorado1 experiments. In these experiments the COPs were very close to 1.00. Why were tiny droplets emitted in Colorado2 but not in Colorado1 and Texas1? Is answering this question not a good reason to start Texas2 experiments, Scott? I would very much like to assist you again.

115) Ludwik --> CMNS list (6/28/06):
I am looking at Scott's second table again: The mass of solid deposit from each cc of condensed wet steam was 1.68 mg. That was the mean for the samples #1, 2 and 3, after 12 hours in the oven. Individual results were 2.06, 1.15 and 1.84. This leads to the standard deviation of 0.47. For the sample #4 (the original electrolyte) the mass of the solid deposit was 27.4 mg per cc. What does this mean? Let me begin with the original electrolyte. The 27.4 mg/ml translates into 27.4 g/liter. This is in very good agreement with 26.5 grams of K2CO3 that was put into distilled water to make the 0.2 M solution.

Each cc of the solution is essentially 1 gram. That solution was supposed to be from pure steam. Finding 1.68 mg of

solid deposit, per one gram of condensed liquid, is significant because 1.68 is 6.13% of 27.4. In other word, our evaporative losses were exaggerated by 6.13 %. This 2.45 times larger than the upper limit in Fauvarque's appendix. And, according to last Thursday test in Paris3, the upper limit should be changed to a much lower number. What is the new upper limit?

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116) Richard --> CMNS list (6/28/06):
. . . If we POSITIVELY have droplets and/or mist [of electrolyte] then the method of computing excess heat is incorrect and the results of Colorado #2 and Paris #1 are wrong. We still need to validate the presence of electrolyte in the steam. One set of test is not enough and we need test with COP>1.30. . . .

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117) Richard --> Scott + Ludwik (6/28/06):
I don't think we can just subtract the mass of the electrolyte from our evaporated mass and recalculate the COP. It seems to me that the presence of the electrolye indicates the presence of droplets. The size and mass of those droplets are unknown. I could see the droplets having at least the mass of the electrolyte. If the presence of the electrolyte is confirmed and we decide that this means the presence of droplets then it seems the open cell GDE is not worth more effort. If we decide to abandoned the open cell I do think we should publish something showing the problems we encounter.

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118) Ludwik --> Richard + Scott (6/28/06):
In my mind the term "droplets" stands the "mass lost that is not pure steam." It can be droplets, molecules or tiny crystals. In our context this is not relevant; we just want to be sure that evaporative losses are calculated correctly. In other contexts such details might be of great importance. I like the idea of publishing negative results. In that way what we did will not be totally wasted. But, once again, we need more data. In particular, a correlation between individual COPs and individual "mass corrections" must exist if a large part of COP is an illusion due to the "droplets effect." Data demonstrating the correlation would be a clear demonstration that the droplets effect is real.

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119) Ludwik --> Pierre + Richard (7/1/06):
Pierre, I did not hear from you since Scott wrote that, according to his data, about ~6% of the lost liquid was not dry steam. Are you OK? If Scott is right then Colorado2 and Paris1 conclusions must be modified. Do you agree? But his conclusion should be independently verified. Are you and Richard going to perform "dry-deposits" tests on condensed steam samples? We need many tests to check the reproducibility of results, and to get the standard deviation. Then we would correct the initial COP accordingly. Do you both agree that this is a desirable plan? Can this be done in two weeks, before Fauvarque becomes available? I still hope positive COP, such as 1.10, will be worth publishing. But we need numerical data to succeed.

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120) Pierre --> Ludwik + Richard (7/2/06):
C'est la FRANCE, Ludwik! Pierre (one of the Colorado 2 team). [That was prompted by what Ludwik mentioned in a messsage posted yesterday at the CMNS list].
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121) Ludwik --> Pierre + Richard + Scott (7/2/06):
1) I am glad to hear from you Pierre. I was also happy that France won the game. I started worrying that absence of messages from you might mean you are sick, or something like this.

2) If Scott is right (about the ~6% of the escaping mass being something else than pure water) then Colorado2 and Paris1 conclusions must be modified. Do you agree?

3) But his conclusion should be independently verified. Are you and Richard (and Gerard, Jacques, Naudin, etc.) going to perform "dry-deposits" tests on condensed steam samples? We need many tests to check the reproducibility of

results, and to get standard deviations under different conditions. My hope is that ~6% will not change significantly with conditions, such voltage or geometry. If this is confirmed then we would correct the COP accordingly. Do you both agree that this is a desirable plan? Can this be done in two weeks, before Fauvarque becomes available? I still hope that positive COP, such as 1.18, will be defendable and worth publishing. But we need numerical data to succeed. Hope is good for motivation, not for justification.

4) And what if experimental data show that our claim of COP>1 is not defendable? I agree with Richard that in that case our paper should also be submitted to a mainstream journal. The experimental part, and theoretical appendix about chemical origin of excess heat , do not have to be changed. But the introduction and conclusions will be totally different.

5) How are you Scott? You were also rather silent recently. Are you OK?

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122) Scott Little --> Ludwik + Richard +Pierre (7/2/06):
Just fine, thank you. The only thing I have done since the K, W, and gravimetric analyses of Richard's 4 samples is to expose the dried residue from sample #4 to 100% relative humidity air by placing it in a sealed box along with an open container of water. The weight gain was about 2.5 times more than expected if K2CO3 was going to K2CO3+1.5H2O so it appears that there are other hydrate ratios involved here and it also appears that my 70C drying oven probably reduced the salt to the anhydrous form: K2CO3.

What this says to me is that we should focus on K concentrations and ignore the state of hydration....except that will give us some trouble when we try to do a real mass balance as Peter Gluck rightfully suggests.

The main problem with the samples that Richard sent me is the fact that the electrolyte was used over and over again and, according to my analyses, became more and more diluted as salty droplets escaped from the cell while Richard added only pure water to make up the lost volume. Thus the condensate samples I have came from runs whose electrolyte concentration was essentially unknown, except that it was somewhere between the original 0.2M and the final ~0.1M.

For a rigorous approach to this problem we need to perform a series of runs in which a sample of the condensate and a sample of the electrolyte are collected for each run.

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123) Pierre --> Ludwik + Jean Francois (7/2/06):

Je crois t'avoir dit que nous travaillons maintenant , Gérard et moi, dans le laboratoire de Jacques Dufour dont les moyens sont plus importants que ceux que nous avions.
Nous avons rencontré cependant les mêmes difficultés que celles rencontrées à Boulder pour reproduire le phénomène. Nous avons dû alors reprendre le même beaker et refaire nos expériences. Tout cela a pris du temps et de plus, nous avons dû regarder le phénomène de stockage-déstockage d'énergie au cours des variations de puissance. Nous étions très préoccupés par ce phénomène qui pouvait remettre en cause tous les résultats acquis sur le CFR....Mes premières conclusions sont que ce phénomène est bien réel, mais que si l'on prend des précautions, par exemple n'enregistrer qu'après au moins 5 minutes de stabilisation et vérifier que les variations de puissance restent modérées pendant l'expérience, il n'y a pas de problème. Le second problème , les "tiny droplets", va être mieux examiné par Jacques dans le cadre du futur Paris 3, mais cela va sans doute prendre du temps...
Pour en revenir à Colorado 2, tu sais que je ne suis pas chimiste et que le temps me manque pour en devenir un. Il faut donc que Jean-François F. te réponde et tu peux compter sur moi pour lui demander de le faire dès la mi-juillet....Ne m'en veux donc pas de ne pas participer aux discussions "chimiques" plus activement... mais je reste cependant persuadé intuitivement que ce problème n'est pas majeur.
Voir dans ton texte ci-dessous quelques réponses en MAJUSCULES.
Amitiés Pierre

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124) Ludwik --> Richard + Scott +Pierre (7/2/06):
Hi Richard and Scott:

Pierre replied to me in French. I will try to summarize what he wrote after showing what he wrote in French.

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Voir dans ton texte ci-dessous quelques réponses en MAJUSCULES.
Below Pierre is replying to my last message by inserting words in CAPITAL LETTERS.

1) I am glad to hear from you Pierre. I was also happy that France won the game. I started worrying that absence of messages from you might mean you are sick, or something like this.

2) If Scott is right (about the ~6% of the escaping mass being something else than pure water) then Colorado2 and Paris1 conclusions must be modified. Do you agree?
OUI, SI CONFIRME (*)

3) But his conclusion should be independently verified. Are you and Richard (and Gerard, Jacques, Naudin, etc.) going to perform "dry-deposits" tests on condensed steam samples? OUI, MAIS PAS DANS UN DELAI RAPIDE (**) We need many tests to check the reproducibility of results, and to get standard deviations under different conditions. My hope is that ~6% will not change significantly with conditions, such voltage or geometry. If this is confirmed then we would correct the COP accordingly. Do you both agree that this is a desirable plan? Can this be done in two weeks, before Fauvarque becomes available? I DO NOT KNOW. (***) I still hope that positive COP, such as 1.18, will be defendable and worth publishing. But we need numerical data to succeed. Hope is good for motivation, not for justification.

4) And what if experimental data show that our claim of COP>1 is not defendable? I agree with Richard AND ME (****) that in that case our paper should also be submitted to a mainstream journal. The experimental part, and theoretical appendix about chemical origin of excess heat , do not have to be changed. But the introduction and conclusions will be totally different.

5) How are you Scott? You were also rather silent recently. Are you OK?

(*) Pierre agrees with need to correct our COP=1.24 to account for Scott results, if confirmed.
(**) They will perform Little-type tests but not without a delay.
(***) He does not know if such tests will be performed during the next two weeks.
(****) The idea of publishing negative results, if it comes to this, was already discussed by us in Denver and Pierre reminds me that we all agreed on this.
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Ludwik,
Je crois t'avoir dit que nous travaillons maintenant , Gérard et moi, dans le laboratoire de Jacques Dufour dont les moyens sont plus importants que ceux que nous avions.
Nous avons rencontré cependant les mêmes difficultés que celles rencontrées à Boulder pour reproduire le phénomène. Nous avons dû alors reprendre le même beaker et refaire nos expériences. Tout cela a pris du temps et de plus, nous avons dû regarder le phénomène de stockage-déstockage d'énergie au cours des variations de puissance. Nous étions très préoccupés par ce phénomène qui pouvait remettre en cause tous les résultats acquis sur le CFR... Mes premières conclusions sont que ce phénomène est bien réel, mais que si l'on prend des précautions, par exemple n'enregistrer qu'après au moins 5 minutes de stabilisation et vérifier que les variations de puissance restent modérées pendant l'expérience, il n'y a pas de problème. Le second problème , les "tiny droplets", va être mieux examiné par Jacques dans le cadre du futur Paris 3, mais cela va sans doute prendre du temps...
Pour en revenir à Colorado 2, tu sais que je ne suis pas chimiste et que le temps me manque pour en devenir un. Il faut donc que Jean-François F. te réponde et tu peux compter sur moi pour lui demander de le faire dès la mi-juillet... Ne m'en veux donc pas de ne pas participer aux discussions "chimiques" plus activement... mais je reste cependant persuadé intuitivement que ce problème n'est pas majeur. Amitiés Pierre

(a) As you know, Gerard and myself are now working working in Dufour's lab, which is much better equipped.
(b) We also encountered difficulties in reproducing Boulder's results. We had to perform the experiments again in the same beaker. All this took time.
(c) Furthermore, we observed storage and release of energy when consecutive tests are performed at different powers. That was important because the phenomenon could negate our Paris1 conclusions. Fortunately this turned out not to be the case. One must avoid collecting COP data before the temperature of the electrolyte stabilizes. This usually takes more than 5 minutes. Significant electric power changes should be avoided.
(d) The tiny droplets tests will be performed by J. Dufour, in future Paris3 experiments. This will take time.
(e) As you know I am not a chemist. Fauvarque must reply to you and will remind him in two weeks. Now you know why I do not participate in discussing chemical things. Intuition tells me that this is not a major problem.

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125) Richard --> Ludwik + Scott + Pierre (7/4/06):
I'm not convinced that the ~6% is a good correction. We are making the assumption that the K2C03 is coming off as particles or a new compound. Is this a valid assumption? I see three other possibilities 1) It is attached to a droplet of water. 2) The K and/or C03 is being vaporized. 3) The K and/or C03 is part of a chemical reaction. All three of these possibilities have different outcomes. Possibility 1 means we have to account for the droplet size. Possibility 2 means we should be adding the (joules) required to vaporization K2 and/or CO3. Possibility 3 might be ok if the deposit are the same as those of the chemical reaction. Does this make sense?

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126) Pierre --> Richard + Ludwik +Scott (7/5/06):
We got the same results at the beginning of Paris 3, i.e. COP around 1.0. So, we came back to our usual beaker of Paris 1(small size and no insulation), but then we tried 0.1 M electrolyte in order to get higher voltage without too many amperes...(too high power). And that worked and we got again COP between 1.2 to 1.3. The difficulty is to have a very stabilized power level and then to be sure that we have no storage or release of energy during the change in power... We are planning tests to be sure of that... (we will try to get the same power curve with the ohmic heater and then to see the COP values obtained...)

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127) Richard --> Pierre + Ludwik +Scott (7/5/06):
The input power needs to be stable over the same time frame as the measurement of the excess heat. Once the beaker is above room temperature it has stored energy. If I just turn off the input power I will have infinite excess heat until the beaker reaches room temperature ie Power_out/zero. As long as the beaker temperature is substantially above room temperature the electrolyte will still evaporate even without input power. In Colorado #2 just watching the current and voltage the input power seemed to be stable over the 5 minute test period. It is possible that this input power was drifting down and we are measuring some of this stored energy as output power. . . .
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128) Ludwik --> Richar + Pierre + Scott (7/5/06):
Ideally one wants to operate under thermal equilibrium, that is when the dm/dt (rate of evaporation) remain constant. The way to accomplish this, in open beaker during the electrolysis, is to use two power supplies, one feeding the electrolysis and another feeding the ohmic heater, immersed in the electrolyte. I am assuming one can measure the electric energy delivered to the cell by each power supply. One would have to add them to calculate the COP.

Suppose the mean dm/dt is evaluated every 3 seconds and the values are plotted value versus time when an experiment is in progress. To keep the dm/dt constant one simply changes the voltage feeding the ohmic heater. If the dm/dt starts going up the voltage is decreased; if the dm/dt starts going down the voltage is increased. I think that keeping the dm/dt constant, within 2 or 3% would not be too difficult.

The temperature-versus-time plot could be useful below the boiling point. Stirring of the electrolyte is probably worth having in all cases. I thing that the last container in Colorado2 was too big for the power of 300-400 W, especially without stirring. One can probably work without stirring, at such wattages, when the beaker is much smaller, for example, one liter. Under such conditions boiling is sufficiently uniform to get a constant temperature within the cell.

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129) Ludwik --> Scott + Richard + Pierre (7/7/06):

On Jul 7, 2006, at 12:11 AM, Scott Little wrote:

>You, [Ludwik] asked:
>
>>Should I add Texas2 to the list of experiments that will be
>> designed to better address the issue of a possible illusion?
>
> Not just yet, Ludwik. We may eventually get around to Texas2
> but I cannot predict when. There are two problems. 1. The MOND
> experiment remains my highest priority right now and I am having
> a great deal of difficulty with it. 2. Earthtech is MOVING to a new,
> larger facility. It's the first time we have moved in almost 10 years.
> It will be hell. We have to be out of the present location by the end
> of this month!!!!

I hope this will not prevent you from performing the XRF tests on new samples that Richard said will be sent to you soon. These will be from experiments performed as in Colorado2. The initial composition of the electrolyte, before each COP was measured, will be better known this time, I suppose. We should not be surprised, the basis of what you did so far, if the data lead us to a less desirable conclusion. Being able to consistently justify negative results in three different ways (weights, XRF for K and XRF for W) would be very convincing.

Assume we come to a conclusion that the necessary correction of the Richard's COP's leads to the value close to one. We talked about such possibility and agreed that conclusive negative results should be published. A good appendix about the XFR would be needed. Would you also be willing insert a section about your old unsuccessful attempts to replicate Mizuno experiments, Scott? These experiments did not use evaporational calorimetery and this would be important, I think. Another possibility would be to publish two papers at the same time, preferably in the same mainstream journal. One would be about the old Scott's results and another about the Colorado2 results. It is probably not too early to start speculating about these things. But only among four of us, at this time. Right?

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130) Ludwik --> Scott _ Richard + Pierre (7/10/06):
I want you to be the first to see unit 302
http://blake.montclair.edu/~kowalskil/cf/302numbers.html
I will post it tomorrow. Comments, as always, will be appreciated.

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131) Scott --> Ludwik (7/10/06):
Good Ludwik, I like the honest way in which you discuss the situation. I only wish that Mizuno was an active part of our group.

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132) Ludwik --> Pierre + Richard (7/11/06):
I will respect your request for privacy, Pierre. Should I assume that this refers to the second sentence only, and that the problem of storage and release of energy can be openly discussed on the CMNS list? The issue of storage of thermal energy is important. I wanted to quote you on this in unit #302 but decided not to do so.

I hope that unit #302 answered your question about what should be done to make Colorado2 results publishable. Measuring of the COPs, when 5 to 10 samples of condensed liquid are collected would certainly be desirable. But this is not essential when the purpose is to learn about percentages of the electrolyte in the escaping liquid. Just make sure conditions (geometry and voltage) are as close as possible to what we had. Can this be done in your present laboratory

situation? . . .

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133) Pierre --> Ludwik + Richard + Gerard (7/11/06):
Of course, the problem of storage and release of energy is important and we have to discuss this point openly in CMNS list. But, what I sent to you was preliminary results and reflects only our point of view, Gérard and myself . That's all. Jacques Dufour was absent from the lab these days.....Now, I am wondering about the wet steam. When we make calibrations curves with the ohmic heater, are we concerned with the tiny droplets of electrolyte? Are your corrections, Ludwik, not already taken care of by the calibration curve made with the electrolyte ? I will try to check that point as soon as possible.

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134) Ludwik -->Pierre + Richard + Gerard (7/11/06):
That is a great..... let us hope for the best. Ludwik
P.S. I do not think that the ohmic heater correction automatically corrects for the mass of tiny droplets. To convince myself I am considering an experiment in a thermos. In that case non-evaporative losses tend to become negligible. But droplets escape in the same way as from a beaker of the same size. How can a nearly negligible correction compensate for a non-negligible effect due to tiny droplets? I am thinking about a case in which the "droplets effect" lowers evaporative losses by nearly 10% while the non-evaporative losses are close to 1% of the supplied energy. Do you agree that this would not be possible, Pierre?

You were probably thinking about a situation in which the percentage of droplets in the wet steam during the electrolysis is the same as during the use of the ohmic heater. This remains to be verified experimentally. Let us suppose that an experiment shows that in both cases wet steam contains 5% of the electrolyte. How would you argue that our non-evaporative correction (such as 50 W determined by the use of the ohmic heater) also corrects for the presence of droplets in the wet steam?

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135) Richard --> Pierre + Ludwik + Gerard (7/12/06):
In one of my early attempts to collect the steam samples I use the ohmic heater as a test and got very clear water sample. When I switch to the plasma the new sample was obviously contaminated (milky white). Just an observation. Is there an easy test to measure the opaqueness of a sample.

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136) Ludwik -->Pierre + Richard + Gerard (7/13/06):

Here is one possibility: Food and drinks are often sold in transparent glass jars or bottles whose walls are flat (not round). Suppose the liquid to be tested for opaqueness is in such container. A beam of light enters the container from the left side and exits from the right side. It falls on a photodiode (from Radio Shack ?). That photodiode, like a thermistor, produces voltage that can be measured; it is proportional to the light intensity. For our purpose opaqueness can be operationally defined as V1/V2, where V1 is the photodiode voltage when the tested liquid is in the jar and V2 is the voltage when the jar is empty.

The experiment should be performed in a darkened room so that most light received by the diode is from the direct beam (not from scattered light, or from other sources in the room). I would place the photodiode into a ~15 cm pipe. A similar pipe would be used on the other side, to collimate the incoming light. A white-light-emitting diode would probably be a sufficient source. If not use a small light bulb, from an old car or a flashlight. The light intensity should match the sensitivity of the photodiode; you might need a dimer.
P.S.
Actually, V1/V2 should be called transparency, not opaqueness. That ratio would change from 0 (not transparent) to 1 (100 % transparent). The V2/V1 could be called opaqueness, if one wanted to use this parameter. Then 1 would stand for the "not opaque at all" and infinity for the 'totally opaque."

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137) Pierre --> Richard + Ludwik + Gerard (7/13/06):
I think you are right, Pierre... I looked yesterday to the curves we plotted with the ohmic heater calibrations. I found that the slope of this curves, which must give 2260 j / g , give values very close to 2260 j / g (at + or - 2%). With tiny droplets, these values would have been lower than 2260j/g, I suppose...Your comments, please...

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138) Richard --> Pierre + Ludwik + Gerard (7/13/06):
I think it would be very difficult to correct the COP value for the misting. At least in a convincing way to skeptics. The original Mizuno experiment was closed cell very similar to Colorado #3 and Paris #3. I think that is the best approach now.
P.S. Pierre I meant to ask any comments from JL Naudin about the misting. Is he going to try and confirm?
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139) Pierre --> Ludwik + Richard (7/15/06):
From mid-July to the end of August, many labs are in holidays in France... I hope that our contributions will not be too delayed... But any way, we will look at yours with interest and pleasure. Cheers

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140) Ludwik --> Pierre + Richard (7/15/06):
Should I still be expecting to hear from JFF in July?

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141) Pierre --> Ludwik + Richard (7/16/06):

I hope so, Ludwik, and I will tell him to do so. But, anyway, do not hesitate to remind him directly of the answers you are waiting for... Next week, with my family, we plan to go to Russia for a cruise on the Volga between Moscow and St-Petersbourg...(12 d)

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142) Ludwik --> Pierre + Richard (7/16/06):
This cruise is on our agenda, perhaps next year. Meanwhile I would like you to take some pictures for me, at a specific location. As you travel toward Volga river, along the famous canal, about 40 miles north of Moscow, there is a village where I lived during the war. It is called "posiolok Dedenievo." There is a railroad station called "Turist" there. Your guide will know the place. Take pictures on both sides of the canal; both before the dock and after the dock. After Dedenievo there was a village "Medviedki," where a railroad bridge goes over the canal. I remember when this bridge was blown up by the Red Army, in late October 1941. The next railroad station is in a place called "Yachroma." And then a town called "Dimitrovo." Germans were in Yachroma for several days when we were hiding in a cellar, under an officially-closed big church. Then German army was pushed away. That was the beginning of their end. Take a picture of the tower of this church from the boat, if you can. Also the picture of the high hill on the right side of the canal, between the dock and the bridge.

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143) Pierre --> Richard + Ludwik +Gerard + Jean Francois (8/ ? /06):
Back from vacation in Russia (beautiful cruise from St-Petersburg to Moscow -1800 kms- 4 pictures joined), I went yesterday to our common CNAM laboratory with only Jacques Dufour and his son there. (Gerard, Denis on vacation). Jacques has his own program of experiments using an ice calorimeter, very precise, and his aim is to work on his own theoritical approach to explain cold fusion. Of course, I let him expose his theory in future cold fusion meetings. Jacques has a very long experience on cold fusion experiments. He convinced me to establish the reality of our experimental results by eliminating all the possible artifacts definitively in an irrefutable manner. This is the only way to convince our physicists colleagues, he told me. I think that Ludwik will approve... The tree points we are examining are the following:

1- the measurements of the electric input energy : we protected all the electric wires with external cladding (armoured? I dont' know the american word) and also used of a special circuit with resistance and capacitors to avoid large electrical impulses. I will ask Jacques to give you more details on that, but I can say that we are now very

confident on our measurements.
2- tiny droplets: we have two different approaches with Jacques:
- Jacques will favor the condensation outside of the beaker of all the steam in order to measure both the water produced and the amount of electrolyte eventually carried on (and then the thermal energy produced) .
- Personally, I would prefer to condensate the steam inside the beaker (as Richard has begun with Colorado 3) and measure the cooling flow and his delta T.
We will choose by September the experimental set-up , but of course, this heavy change will take time to fix...
3- Storage and release of energy, with power changes:
I am not as anxious as Jacques is on this item. We made experiments showing that this could be masterized quite easily. But, the way to avoid this problem is to have longer experiments and to try for that to have cathodes moving very slowly in order to maintain constant their active length and then constant the power. Still there, we have to fix the experimental apparatus...

I am confident that we will achieve these goals by the end of this year. Yesterday, we got a COP of 1.3 with our traditional apparatus (analysis by hand to be confirmed). This experiment was to confirm the interest for "armoured" electrical wires... (answer is yes). Richard, where are you on Colorado 3 ?

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144) Ludwik --> Pierre (8/5/06):
1) I did not hear from Richard since before your trip to Russia.
2) The Paris3 agenda looks promising. I will probably have some comments later.

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145) Ludwik --> Richard + Pierre (8/7/06):
. . . I haven't heard from you since the above question was asked. Is everything OK? By the way, I am also puzzled by Naudin's silence on the CMNS list.

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146) Richard --> Ludwik + Pierre (8/7/06):
I've been very busy at work lately. Haven't had time to work on the experiment much. We had a family reunion last weekend it was a lot of fun. Got to see all 11 of the siblings and a lot of the cousins. Attached is a picture of my brothers and sisters. I enjoy your pictures also. I do plan on working on the experiment again soon.

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147) Ludwik -->Pierre + Richard +Gerard + Jean Francis (8/8/06)
Pierre: Thanks for information about the upcoming Paris3 experiments. To refresh our memories, let me make some comments and suggestions. I hope they will be useful in planing.

1) Shielded-grounded wires should prevent occasional interference effects observed in the U390 meter. My suggestion is to add a kWh meter on the other side of the power supply. That is what was done in Texas1 and Colorado2. At that level the v(t) and i(t) are sinusoidal and a simple instrument can be used to measure electric energy. I thinking about the instrument Richard used for the ohmic heater. The second kWh meter readings will probably be consistently higher than U390. That should not be a surprise; some electric energy is constantly converted into heat inside the power supply. The rate is probable less than 40 W. An ohmic heater of variable R can be used to learn how this number changes (probably not much) with the wattage readings of the U390.

Suppose that the the difference between two kWh meters is the same fore ohmic heater operating at ~300 W and for the plasma electrolysis cell operating at ~300W. That would be a very convincing argument that no significant errors are made in measuring electric energy supplied to the GDPE cell. Sampling
the v(t) and i(t), for example, 100 times per second, and showing that the sum of v(t)*i(t) agrees with the U390, would be another strong indication that no error was made.

2) Another suggestion is to use fresh electrolyte for each long experiment. Also to measure the amount of tungsten lost by the cathode (to calculate eV of excess heat per removed atom). Large eV/atom, for example, 1000 or more, are

convincing arguments against chemical origin of excess heat. Chemical reactions never produce more than 10 eV/atom (usually less than 5). Note that tungsten that remains metallic does not react chemically with anything. For that reason only a fraction of the W lost by the cathode should be counted when the eV/atom are calculated. It would be essential to measure that fraction. Mizuno wrote that nearly 100% of the W lost by the cathode was in the form of metallic particles at the bottom of the cell. One way to separate metallic tungsten from the less dense solid components would be to agitate the electrolyte and collect what is deposited quickly at the bottom, like in gold panning. By doing this several times one should be able to separate most of the metallic tungsten. A chemist might suggest a better method.

3) Performing the Little-type test (comparing the mass of residuals per cubic cm in the fresh electrolyte with the mass of residuals per cubic cm in the condensed steam) is highly desirable. That seems to be a good method of determining the percentage of tiny liquid droplets, or colloidal particles, in the escaping steam. Scott found that the mg/cm^3 in two samples were not very different. That is alarming. His conclusion, however, was tentative because fresh electrolyte was not used in each experiment. In my opinion Little-type tests should be performed at the very beginning. Hopefully, they will show that the COP>1 is not an illusion due to tiny droplets or particles. Only then should further investigations be made. I suspect that results, such as COP=1.24, would have to be reduced to account tiny particles or droplets in the escaping steam.

4) I agree that establishing "the reality of our experimental results by eliminating all the possible artifacts definitively in an irrefutable manner" is essential at this stage. We already know that results are reproducible. Now we must convince ourselves, and others, that excess heat is not due to a well known effect. Three to five well done experiments, with the maximum amount of information, would probably be more valuable than a much longer sequence of experiments, with the minimum amount of information (only what is needed to calculate the COP).

5) If the purpose of the experiment is to validate conclusions made in Paris1 and Colorado2 then an open cell (favored by Jacques) is preferable. Pierre's preference -- condensing steam inside the cell -- would be more desirable if one wanted to perform better experiments. That is what Richard is planing to do in Colorado3, I suppose. The most desirable outcome would be to show that Paris3 and Colorado3 (and Texas2 ?) results are not very different.

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148) Richard --> Pierre + Ludwik + Gerard + Jean Francois + Scott. (8/9/06)

Pierre thanks for sharing what is happening in Paris. I've been busy at work and haven't done much. I finally got the Pinnacle power supply working correctly using 220V and single phase instead of the 3 phases it usually takes. It is doing a nice job of regulating the input power.

I'm also seeing the arc suppression that we never saw during Colorado1. It's very interesting I'm able to slowly increase the power from about 100 Watts to about 1500 W. The plasma starts at about 300 Watts it is a bright red if I keep increasing the power the plasm goes from red to bright white. The two attached pictures show input power at about 500 Watts and 1500 Watts. There is little difference in the beaker between the two power ranges.

I'm also seeing the current drop if I find the operating point, just like we saw for Colorado 2. My biggest problem right now is EMI. I just can't collect the data with computer. The USB data acquisition system keeps going crazy. I'm going to work on that this weekend.

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148) Scott -- >Pierre + Ludwik + Richard + Jean Francois + Gerard (8/9/06):
Thanks for the update, Pierre. You are going in the right direction with your rigorous checking of every part of the experiment. One point I would like to make: Seven years ago we studied the fate of the W lost by the cathode and learned that only a small fraction (~5%) of it ends up as swarf at the bottom of the cell. The rest goes into solution, presumably as H2WO4. Our measurements and rationale for this conclusion are presented in this report:

http://www.earthtech.org/experiments/Inc-W/2ndtry/run6.html

This result is also qualitatively confirmed by my recent XRF analysis of Richard's samples. Each of them contained

significant levels of W.

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149) Pierre --> Ludwik + Richard + Gerard + Jean Francios (8/26/06):
[Sending us a diagram of a setup for their Paris3 experiments]

150) Ludwik --> Pierre + Richard * Gerard + Jean-Francois (8/26/06):
Thanks for updating, Pierre. I suppose you will have a small pomp forcing water to circulate through the cooling loop. The mass of the water, passing through the cooling loop during a stable run, must be measured accurately. How accurate is your flow-meter? With the new arrangement the input energy, E, is as before but the output energy, Q, is different. The COP is still defined as Q/E but now Q=Q1 + Q2 + Q3. The first two terms are as before (Q1=evaporative loss, Q2=thermal loss) but Q3 = c*m3*dT, where m3 is the mass of water passing through the loop during a run, dT is the constant temperature difference between your two thermocouples, and c is specific heat of water. Am I interpreting your plans correctly?

I suggest you place at least one thermistor (or traditional thermometer) inside the cell, preferably near the wall. The rate of cooling should allow you to control temperatures below the boiling point. Suppose the COP is significantly larger than 1.0 at 80 C. At that temperature Q1 will be negligible and, as you write, the issue of tiny droplets will disappear. My guess is that the COP will not change suddenly between 95 C and 100 C. The temperature of the electrolyte near the wall is not a measure of local temperatures near the cathode.

As you know, optical thermometers (pyrometers) are routinely used by metallurgist to measure temperatures of glowing objects. Eventually one might try to measure the temperature of plasma with an optical thermometer. How does the COP depend on that temperature? But that is a big project. For the time being the main task should be, as for the Colorado2 results, to convince honest referees that COP>1 is not due to well known chemical reactions. I hope these observations will be useful to you.

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308) Expected things

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1) About a year ago I asked Steven Jones about progress of his work, especially about high energy alpha particles that he asked me not to write about because it was work in progress (item 124). The reply surprised me; instead of finishing this work Steven simply switched to something that has nothing to do with cold fusion. That is how I learned that he was investigating the conspiracy idea about the collapse of the World Trade Center towers on September 11, 2001. He wrote that the towers were too strong to collapse from the airplane hits alone. He was waiting for samples to be analyzed for the presence of explosives presumably pre-located in critical locations of each building.

In the last ten days two people sent me an article about that investigation of Jones'. It was recently published in Utah's newspaper Desert Morning News. The university seems to punish Steven for this work; they did not allow him to teach (payed leave). The full text of the article can be found at

http://newenergytimes.com/Inthenews/2006Q2-4/Q3/DESERET-BYUPlacesJones.htm

How will this affect reputation of CMNS? Jones and Fleischmann were the most important contributors to that field; the term 'cold fusion" was invented by Jones three years before the 1989 announcement. The effect could not possibly be positive, unless the so-called "conspiracy theory" is confirmed. I do not want to probe into this subject but this unit will have an appendix about possible consequences.

2) Another thing worth mentioning is a new book about CMNS. The author, Edmund Storms, is still working on this book. On August 28 he posted the following message on the restricted Internet list for the CMNS researchers. "Dear Fellow Confusionites, In the process of writing a book, I have come to some conclusion which some people in the CF field might find heretical. In addition, new published information, when viewed in its totality, leads to some conclusions that are in conflict with some major theories. I would like to have these assertions debated in semiprivate before I publish what might be incorrect ideas. So that the debate can be kept focused, I will make each assertion a new link. Of course, ideas in one link will spill over into others, but hopefully the idea will only be applied to the subject dujour. Some people might want to continue a more detailed discussion in private, which is ok, but I do not have time to handle many private discussions. So there goes. (Assertion #1) . . . "

I am not going to quote numerous replies generated by the Assertion #1, and by two subsequent assertions -- #2 and #3. They shows that, contrary to what many think, the CMNS community is far from being a society of mutual admiration. Numerous disagreements, and traces of past disagreements, emerged from the debate. After examining a list of chapters I realized that something very important was missing. This prompted me post the following message. " I suggest that a chapter about promising commercial initiatives that failed is worth adding, for example, between chapters 6 and 7. The most spectacular, as far as I know, was at iESiUSA. Or it can be an appendix. A book without this would not be a total description of what was going in the CMNS field during the last 17 years."

Here is the reply from Ed: "Thanks for the suggestion, Ludwik. I'm afraid I'm not the person to evaluate the commercial aspects. Steve Krivit is doing this. My job is to make sense of the science. Too many of the commercial attempts were not based on science and failed because of human limitations. Such failures will not happen in the future

if and when the science is understood." Another old-timer wrote: "Regarding 'promising commercial initiatives that failed", I'm reminded of Ceti's claim, made in their 1995 Corporate Video that: -- 'Ceti is scaling up the size and efficiency of the cell and expects to produce 1 kW of power as part of a 250 kW array. At ICCF-6 Ceti were claiming they had 40 firm orders for their Rifex demonstration kit. In the event I think only 4 were sold."

I was not aware of this fact. Would it be appropriate to say that the above mentioned corporate video was an axample of a fraudulant manipulation? The same question can be asked about the iesiusa company, as described in units #216, #229, #236, #237, #239, #263 and #279. Assuming the answers are positive one can ask another question. Who is responsible for the fraud, scientists or business people? That question, addressed by S. Krivit (at ICCF12), was asked by Krivit. He said that iesiusa scientists were victims, rather than perpetuators, of fraudulant business activities. I would like to know what Peter Hagelstain thinks about this now. He was deeply involved in the iesiusa situation last fall.

I think that commercially motivated projects are worth describing in a book about CMNS. They were taken seriously in the CMNS community (for example Case presentation at the ICCF10 that I attended) because each project was based on ideas discussed at gatherings of non-commercial researchers. In that sense commercial projects were not different from other projects -- nothing is "based on science" in our area, everything is based on proto-scientific speculations, both theoretical and intuitive. By the way, Case presentation does not appear in the ICFF10 conference proceedings. Why it so? Probably because he failed to submit it before the deadline. I do remember him speaking at the confernce, and showing an impressive metallic setup.

CMNS will remain proto-science until at least one reproducible on demand experiment becomes available. That is why I think that all efforts should be focused on finding such experiment. How many millions of dollars were spent on the CMNS research since 1989 worldwide (including support from Toyota, Mitsibushi and other private benefactors)? Would we still be asking for reproducible on demand demos if all that money was used more rationally? I do not know how to answer these questions. But they should be addressed in a book on cold fusion. Storm and Dash decided to reinvestigate the experiment demonstrated by Dash's students at ICCF10, but not before the book is published. Will this impressive 2002 demo be described in the book? I hope so. This relatively inexpensive setup was enthusiastically described by Eugene Mallove. And, as far as I know, it was not criticized by those who saw the demo. Was it seen by Fleischmann himsef? He was attending the conference but I do not recall seeing him the public demonsration of the experiment. I suppose it was shown to him privately. It would be useful to know his comments.

3) Below is a piece about peer reviewing. It is from Photonics Spectra (April, 2006, page 10). The author, Wendy A. Laurin, is a group publisher of that journal (devoted to applications of optics, lasers, imaging, fiber optics, electro-optics, etc.)

Journals: Advancing Science

As do other qualIty publications, Photonics Spectra uses peer-reviewed journals for news ideas and information on scientific research. Science, Nature, Optics Letters and Applied Physics Letters are a few of the journals we use regularly. They provide not only details on the results of an experiment, but also the technical details of how the experiment was conducted.

Peer-reviewed journals depend on individuals who are willing to commit their time to judge whether submitted papers are suitable for publication and to ask questions if the research is not clear. Most of these people are experts in their field, making them the best ones to perform the initial review.

The accuracy of these journals is now being questioned following the retraction of some articles, including the well-publicized research on cloned human cells and a cancer study from Norway. Some in the general press are asking for more policing of the research that peer-reviewed journals publish.

The reality is that journals do not have the money or time to scrutinize every experiment. Additionally, they do not claim to Investigate each paper. They do, however, ensure that enough information is presented for the experiment to be replicated. Although they have been taking a more proactive approach to the problem, there is no denying that the most powerful means of review are the readers.

Journals are doing their job. They disseminate scientific information to a large group of scientists who can redo the experiments and examine the results in detail. Most highly rated publications will acknowledge that there are some problems with the peer-review process, as evidenced by Science's retraction of a paper. But if the journal had not published the paper, fellow researchers would have had no other way to effectively review the experiment and point out the fallacies.

This is a system that has been productive because peer-reviewed journals rely on their advisers and their readers for feedback. Although there may be individuals who falsify research, in most cases the very act of publication keeps most researchers honest because they know that the work can be thoroughly examined. There is no doubt that the system can be improved, and It has been only in the past few years that some journals have required the authors to report financial interest. But peer-reviewed journals remaIn a powerful way for researchers to communicate with each other.

The power of publication can be shown in other ways as well. A recent editorial in Science magazine states that there are times when an organization or individuals have asked that research by a particular group not be published. Because Science does not believe In censorship, it instead suggests that the would-be suppressors put their thoughts In a technical comment. And therein lies the true job of peer-reviewed journals - putting research in the open for everyone to see.

The upcoming 2007 ACS conference (appended on 10/14/06):
Several days ago someone made a suggestion that papers devoted to CMNS topics should be presented at the next American Chemical Society (ACS) meeting. Such presentayions have been made at recent APS (American Physical Society). But presenting CMNS papers at ACS was not easy, as described below by Mel Miles. Instead of addressing the issues scientifically (during the conference) the organizers tried to block presentations administratvely. What a pity. Is such discrimination justifiable? When will it end? Here is how one 1999 discrimination episode was described, on the CMNS list, by Dr. Miles:

In 1999, my China Lake co-workers were tasked with organizing the ACS Ontario, California Western Regional Meeting, October 6-8. They asked me to organize a battery and cold fusion session which I did. This caused all hell to break loose. The special plenary symposium speaker, Dr. Jerome Karle of NRL, threatened to withdraw from the conference if cold fusion papers were allowed. Others such as Dr. Cotton of Texas A&M threatened to resign from ACS. At one point, ACS proposed a compromise solution where the cold fusion session would be allowed but would not be sponsored by ACS and abstracts would not appear in the ACS Abstract Book. At this point, my China Lake co-workers (Peter Zarras and Erick Erickson) threatened to resign from ACS if the cold fusion session were treated this way. In the end, ACS sponsored our session and allowed our abstracts (For program, see http://www.vidrine.com/paccon/techprogram.html). However, the cold fusion session was separated from the battery presentations and moved to Friday, we were told to keep a low profile, and not to invite any more speakers on cold fusion. Speakers at this cold fusion ACS meeting included Martin Fleischmann, Stan Szpak, Mike McKubre, Talbot Chubb, John Dash, Ed Storms, Robert T. Bush, Ben Bush, Russ George, and myself. I believe that this was the last time that cold fusion papers wee allowed at an ACS meeting except for maybe a few isolated papers that escaped detection. In contrast, APS (Physics) has been allowing cold fusion sessions at their March meetings for years - thanks to Scott Chubb.

I hope my experience with ACS will give Dr. Marwan and others some insight on what to expect when word gets out about these submitted cold fusion papers." In giving me permission to post the above message Dr. Miles added: "I

consulted my notebook before I wrote this, thus it is accurate." Will the 1999 opposition episode be repeated in the spring of 2007? That remains to be seen.

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309) What is NAE?

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1) Yesterday I posted a message (shown in green below) at a restricted list for CMNS researchers. It was about NAE (Nuclear Active Environment), the concept often used by Ed Storms. I hope my message will generate some discussion. Those who are not familiar with the concept of NAE should refer to two Ed's papers:

a) "A Student's Guide to Cold Fusion" at:

http://www.lenr-canr.org/

b) and "The Nature of the Nuclear Active Environment Required for Nuclear Energy Reactions" at:

http://www.lenr-canr.org/acrobat/StormsEthenatureo.pdf

Here is the concluding part of the Storms'second paper:

"Once the NAE is accepted as being of a small size and made from a complex alloy of various elements, perhaps including palladium, methods required for replicating the effect can be suggested. The most obvious method is to use techniques developed by the microelectronic industry, which can deposit controlled amounts of material onto very small regions. Such methods would allow the particle size to be controlled as well as having the ability to apply known amounts of various elements. Once the range of variables affecting the NAE is understood, a large amount of active material could be easily made. In addition, a correct model for explaining the unique nuclear reactions could be developed based on such knowledge. In the absence of this approach, present efforts are handicapped by having to create the NAE using random processes, which is a very inefficient method. The Iwamura et al. [54] study shows what can be accomplished when suitable tools are applied to the problem."

Here NAE is described in terms of physical parameters: small size, made from a complex alloy, etc. But in other messages NAE is described in terms of what is expected to do. Which one is the definition and which one is the consequence of that definition?

2) Ludwik wrote:
"I am again thinking about Ed's concept of NAE (nuclear active environment). We do not know what NAE is; it is something to be identified in the future. Therefore, I think, that the concept of NAE cannot be used to validate known experimental facts or situations. In science we proceed from what is already known to what is not known. The opposite approach seems to be unscientific; perhaps it should be categorized as philosophical or mathematical. Why mathematical? Because mathematicians begin with propositions and proceed to examine consequences resulting from them. Scientists, on the other hand, begin with experimental facts and proceed toward explanations.

Ed assumes that CMNS phenomena will become demonstrable on demand when NAE is identified in terms of its physical parameters. For the time being he identifies it in terms of desirable effects. But how do we know that such effects will take place? He postulates that. A mathematician should have no trouble with postulating. He would say

that a starting point of a derivation is an axiom (or an already proven theorem); it does not have to be validated (or justified again). A physical scientist, on the other hand, is expected to begin with reproducible experimental facts and to proceed toward deeper understandings.

Yes, science and mathematics benefited greatly from clever thinkers who were frequently changing hats -- from the pointed hat of a mathematician to the pipe hat of a scientist, and vice versa. But that should not prevent us from distinguishing two kinds of thinkers. It should also not prevent us from asking questions about NAE. Why do we need that concept at this stage? For the time being, as far as I can see, NAE is used only as an explanation of failures to produce a convincing reproducible-on-demand demo. Does it validate claims based on not-always-reproducible experiments? Does it help us to make CMNS more acceptable to mainstream scientists? I do not think so."

3) Replying to the above Ed Storms wrote:
"Frankly, I'm at a loss to understand why the concept of NAE is so hard to accept. The concept only acknowledges that the LENR reactions do not occur in ordinary materials, but only in special regions. We need to call these regions something if we wish to communicate about the differences between these regions. The NAE is a description used to distinguish one region from another. We say one region of a material is smooth and another is rough as a means to make a distinction. The idea of NAE is no different. One region can initiate a nuclear reaction and another region can not. What is so difficult to understand about this a pproach? In addition, it is obvious if we were able to make more of this special environment, we would be able to initiate more nuclear activity."

This generated an interesting discussion. The message posted by X reminded us about theoretical physicists who sometimes wear one hat and sometimes the other. A specific illustration was given. But not too many theoreticians are like Fermi who was equally productive under each hat. Most theoretical physicists rely on reported experimental results. Likewise, most experimental physicists rely on conclusions reached through complex theoretical derivations performed by mathematically inclined colleagues.

Ludwik's reply:
a) Regions should be distinguished in terms of physical parameters, not in terms of desirable effects. That was my main point.

b) The last sentence is an example of circular reasoning. First we define NAE in terms of desirable effects and then we say, "it is obvious" that such effects will take place in NAE.

c) Will the concept of NAE help us to make CMNS more acceptable to mainstream scientists? I do not think so.

4) Ed's reply:
a) "[You wrote:] Regions should be distinguished in terms of physical parameters, not in terms of desirable effects. That was my main point." Since when? A region can be beautiful, soft, or bright. A person can be desirable, intelligent, handsome. All are terms used to distinguish one region or person from another. No physical parameters are involved. In order to discuss anything, we need words to distinguish one idea from another. NAE distinguishes those regions that do not allow nuclear reactions to be initiated from regions where such reactions are possible. Only later will the characteristics be defined. It is like the word "tree" which distinguished an item from a flower, for example, without say what kind of tree or what kind of flower. You are making this much too complicated.

b) "[You also wrote:] The last sentence is an example of circular reasoning. First we define NAE in terms of desirable effects and then we say, "it is obvious" that such effects will take place in NAE." I don't think you will understand what I'm saying if you don't read what I wrote. You are not quoting my statement accurately. The analogy to what I said about NAE is, if you have more money, you can buy more things. This is a very simple statement requiring no additional interpretation

c) "[You also wrote:] Will the concept of NAE help us to make CMNS more acceptable to mainstream scientists? I do not think so." If the concept helps those of us who understand the concept make progress in defining the NAE, then the field will advance and it will be accepted."

5) Ludwik's reply:
Circular reasoning is the practice of assuming something, in order to prove the very thing that you assumed. Why do I think that your message about NAE (see below) is an example of circular reasoning? You wrote:

> The concept [NAE] only acknowledges that the LENR reactions
> do not occur in ordinary materials, but only in special regions.

In other words, you are **defining** NAE as an environment in which LENR reactions occur. Then you wrote:

> . . . it is obvious if we were able to make more of this special
> environment, we would be able to initiate more nuclear activity.

Yes, it is obvious because NAE was **defined** as an environment supporting nuclear reactions. To remove circularity of reasoning one would have to define NAE differently. One possibility, for example, would be to say that NAE consists of regions in which average distances between atoms are shorter that 100 F. Or it can be defined in terms of local temperature higher than 10^6 K, etc. That what I had in mind by "physical parameters."

It is possible that you already defined NAE in terms of physical parameters but I am not aware of this. In that case ability to generate CMNS reactions would no longer be 'by definition," it would be a logical consequence of a definition. And your statement above would no longer be an example of circular reasoning. In that case, however, one has to explain why such environment should produce such and such consequences. That is what theoretical physicists do.

6) A comment from Y:
NAE is defined by Y as a region of space in which a chain reaction of some kind can propagate, from atom to atom. He argues that a region containing BEC (Bose Einstein Condensate) should have the ability to sustain a chain reaction. (By the way, I am not quoting what X and Y wrote because I do not want to waste time on asking for permissions. Ed, on the other hand, and several other researchers, gave me blank permissions to be quote what they post on CMNS list. I really appreciate this.)

7) Ed's reply to Y:
"Well, Y, I see you understand the need to accept the concept of NAE. Once a NAE is acknowledged, we can talk about what it is made of and what mechanisms operate in its unique structure, mechanisms not possible anywhere else. This restriction immediately forces theory to take a different approach. For example, some theories assume the basic requirement of the NAE is a large D/Pd ratio. Unfortunately, nuclear activity is observed when the D/Pd ratio is low. Some assume high voltage gradients are necessary. Unfortunately, nuclear activity is seen in the absence of applied voltage. Quantum effects are proposed to cause charge shielding when the lattice is perfect. Unfortunately, the lattice is never perfect anywhere. Each assumed ideal condition is shot down by nature. I suggest theoreticians must start thinking in new directions. I also suggest, hydrino formation is the only mechanism not in conflict with observation. We have the irony of one outlawed idea being an explanation for another outlawed idea."

8) Ludwik's comment:
In reading Ed's reply to Y I see that he also believes that NAE must be defined by models (to which I referred to as material parameters) and not by desirable effects. This opens the path toward noncircular arguments about validity of each model. Desirable effects of NAE must now be explained; they are no longer "obvious" by definition. I hope that readers of Ed's anticipated book will have no reason to think that circular reasoning is allowed by CMNS researchers.

9) Storms' comment:
Several additional researchers contributed to this thread. One of them also thinks that defining NAE in terms of desirable effects amounts to "tautology." In a subsequent message Storms wrote: "For a long time, people assumed the nuclear reactions were occurring throughout the sample. Then attention was focused on the surface of a cathode because this is where the D/Pd ratio is greatest. Theories were based on the properties of PdD as if nothing unique was required. The NAE concept focuses attention on the need to explore some very unique property of matter." In his first reply (see above) Ed wrote: that the concept NAE "only acknowledges that the LENR reactions do not occur in

ordinary materials, but only in special regions." In another message a comparison was made with bacterial colonies which grow in favorable spots, and not uniformly.

10) Ludwik's comment:
Discussion between Z and Ed was interesting. How do we know about bacterial colonies? By observing them through microscopes. Such observations are highly reproducible. And how does Ed know that CMNS reactions cluster in small regions? Is it an experimental fact or is it a theoretical postulate?

a) Suppose it is an experimental fact. In that case the validation criterion is replicability. One must refer to specific reactions and to conditions under which they are clustered. Unfortunately, we still do not have reproducible demonstrations of CMNS reactions.

b) Suppose we say that the existence of NAE is a theoretical statement (which is not a guess). Then the statement must be validated theoretically in terms of what is already known. In that case NAE becomes a model-dependent concept.

Is it not true that scientific concepts are either experimental or theoretical? If so then how should NAE be characterized? I think that Ed would agree that existence of NAE is only a useful educated guess, at this time. Such guesses often contribute to progress in science and technology.

11) Another Storm's comment:
The next day, replying to Z, Storms wrote: "The idea of NAE is very simple - apparently too simple for it to be easily understood. You are reading too much into the idea and making it too complicated. The NAE describes a solid material in which conditions permit a CF reaction to take place. The idea acknowledges the obvious, that the CF reactions are very selective about where they occur. The idea allows people to discuss the difference between ordinary dead material and the special active material. It also encourages focus on finding this unique material rather than trying to explain the effect by QM actions in ordinary material. I do not know what the NAE looks like or what elements it contains, No one knows this yet. The challenge is to determine just what is required of this environment. I propose hydrinos are involved, but so far, there is no proof for this assumption. You are free to propose anything your imagination can support. The whole point is getting people to think about a special condition."

12) Next day Ludwik wrote:
Saying "it describes" is very different from saying "it is." If we do not know what "it is" then how can we know what "it describes?" That is what bothers me when I think about NAE. The concept seems to be very unusual, unless it is recognized as an educated guess.

P.S. (**not posted**):
Ed wrote "the NAE describes a solid material In that sense NAE can be said to be a theory. Theories describe things; things do not describe other things. Why am I focusing on philosophy? Because I am looking for clarity about NAE.

We already have too many unusual things in CMNS; why do we need this one? To improve the status of CMNS in the eyes of mainstream scientists we should emphasize that our methods of validation do not differ from those they use in difficult situations. What we need is at least one reproducible-on-demand demo of a new nuclear effect. That is why I am happy that Storms and Dash plan to produce a convincing demo based on what students of John Dash did at ICCF10. I hope that collaboration will go further than what we did in Colorado. Replicating Paris1 was relatively easy, convincing ourselves that excess heat was not due to a chemical, or other non-nuclear, effect seems to be much harder. Why do we need the concept of NAE to guide us? What we need is a theory.

13) Ludwik wrote (also not posted):
In my opinion educated guesses are likely to be much more productive than random jumping in different directions. In that sense the NAE concept might be very useful. But what is really needed, as often emphasized by Storms, is a theory that has been recognized as successful. A theory becomes successful when it is able to predict a set of conditions under which at least one CMNS reaction, for example, emission of unexplained alpha particles, is experimentally reproducible. Theoreticians create and manipulate conceptual models, experimentalists deal with

physical reality. What will be available first, a successful CMNS model or a truly reproducible CMNS experiment? Both might happen at about the same time. I am thinking about Fisher's model based on polyneutrons and about Oriani's and Lipson's experiments with CR-39 detectors. I do not remember what motivated Lipson but Oriani told me that his CR-39 experiments were undertaken to test Fisher's theory (see item #191 at this website).

14) Z was William Collis; today he gave permission to quote this message:

a) Edmund Storms <storms2@ix.netcom.com> wrote: "We know that the nuclear reactions are difficult to initiate and when they occur, the nuclear products are found in only isolated locations." Agreed.

b) [He also wrote] "We know that clusters of deuterons are involved in the transmutation reactions and probably in heat production as well." Not sure. This is an aspect of some models but perhaps it is not an observation.

c) [He also wrote] "We know that significant gamma radiation is not produced." Agreed. Few gammas, neutrons nor apparent radioactivity.

d) [He also wrote] I propose that for energy to be lost after a nuclear reaction, at least two products must be formed, one of which may be a gamma. The only way this requirement can be avoided is for the energy to be communicated directly to the lattice." Agreed. We also know that Cold Fusion can take place in liquid, gas or solid surface phases which would rule out any lattice bulk mediated effect. Unfortunately there has been little demonstration that the lattice is capable of suppressing gamma radiation. Until such a demonstration occurs we should consider any model that relies on it as unverified conjecture.

e) [He also wrote] "We know energetic particles are emitted, which is in conflict with energy being coupled to the lattice." Energetic particles are quite rare, so I would not class them as a primary observation. If a near surface excited nucleus could transfer energy towards the lattice bulk, we would expect momentum conservation to create an energetic particle directed away from the bulk.

f) [He also wrote] . "I propose that nature resists the concentration of energy. Therefore, for clusters to form or for sufficient energy to be localized to initiate a nuclear reaction, the process must be exothermic." Probably.

g) [He also wrote] "Formation of hydrino's is exothermic. A fully collapsed atom would appear to have very little nuclear charge. Therefore, it might fuse with another element. This idea was suggested by Mills years ago. In addition, hydrinos can form clusters, which is an exothermic process. When these clusters fuse with another nucleus, a logical assumption results. This assumption is that the electrons are spit out of the reaction with high energy, which is the process whereby energy is communicated to the lattice. The faux beta would not be detected outside of the apparatus. However, the weak X-rays have been detected."

Agreed as far as the synthesis of hypothetical hydrinos and their clusters are concerned - no easily detectable X-rays. However I suspect that the rate of fusion in mini molecules will be such that copious hard radiation would be emitted. Using Koonin's formula (See NATURE VOL 339 NO. 6227 PP 690-691, 29 JUNE 1989), a mini PD or D2 molecule a tenth of normal size will have a lifetime of only 10**11 and 10**9 Seconds respectively. As we don't see gammas or neutrons corresponding to these expected fusion rates, that's evidence they don't exist.

I'll concede the above rates are similar to measured excess powers. But I would expect excess power to continue for a time scale appropriate for the lifetime of the mini-molecules - which might be a long time after the experiment is switched off. Heat after death does not last decades!

h) [He also wrote] "So, by simply assuming hydrinos exist, a model for formation of clusters, fusion of clusters, and the transfer of energy is provided." Agreed, apart from the above.

i) [He also wrote] "In addition the difficult and localized process occurs because hydrinos can be produced only where certain catalyst atoms are present in the sample. The obvious test, which most other models lack, is to discover which atoms act as catalysts under CF conditions and put them in a sample. I suggest the CaO used in the Iwamura method is

such a material. If so, the process occurring in the Iwamura sandwich becomes obvious." It doesn't seem obvious to me. Would you agree that any deuterons fuse with Iwamura's 133Cs one at a time! If so, the expected first reactions could be:-

2H +133Cs(100.%)-> 1n (beta-)+134Ba(2.42%) +5.942 MeV
2H +133Cs(100.%)-> 1H (100.%)+134Cs(beta-) +4.667 MeV
2H +133Cs(100.%)-> 4He(100.%)+131Xe(21.2%) +11.050 MeV

Once again, I ask, "Why don't we see neutrons?". I ask the same question also if you assume that multiple deuterons all fuse simultaneously (Takahashi's model). The trouble with all these models is that the deuteron is a very blunt instrument for creating non-radioactive transmutations. Most deuterium reactions are just too energetic. In theory, a deuteron can fuse exothermically with every naturally existing isotope! But it doesn't. This might mean that the basic mechanism is NOT deuterium / protium fusion. Maybe in a further message, we can enumerate other key observations which need explaining.

15) Ed wrote (about the last point above, at the end of a much longer message)**:**
. . . This is a good point that has troubled me as well. We don't know the rules that control such reactions. Apparently, the reactions only favor stable products, with a few exceptions. Nevertheless, I expect once this idea is accepted, someone will find a way to explain how only stable isotopes are produced. It seem intuitive obvious to me that the reaction would want to go to the lowest energy state, hence only a stable product would be favored.

16) Ludwik wrote:
In rereading Storms' paper:

<http://www.lenr-canr.org/acrobat/StormsEthenatureo.pdf>

I see a very important statement; it appears in the introduction. "We will start by making the assumption that most claims for low energy fusion and transmutation are correct. Support for this assumption is readily available in the literature[3-5]." Yes, we can make this assumption, and point to corresponding references. But that does not automatically validate the assumption. At this stage each experimental claim is still questionable, as far as I know. That is why I think that NAE is an educated guess. It amounts to the following:

a) Highly qualified researchers reported experimental facts which seem to conflict with generally accepted theories. However, for unknown reason, experiments are not always reproducible.

b) The reason for irreproducibility is absence of understanding. How can one argue with this? Essential factors determining output of experiments remain unknown.

c) Ed seems to believe that new nuclear reactions will become reproducible on demand in a special kind of environment -- NAE. Here is how he writes about this in the same introduction. "The challenge of this paper is to learn about the environment in which these nuclear reactions occur by assuming that the environment has universal characteristics common to all methods found to produce the effect."

d) The educated guess about universal set of preconditions might be confirmed in the future. Another possibility is that different nuclear reactions will require different sets of preconditions. For example, an environment that makes production of He possible might be very different from an environment in which alpha particles, or neutrons, are emitted.

17) Peter Gluck wrote:
Very good synthesis of the situation, I think. However, the global history of the experimental results show that the problem of reproducibility is or can be an essential information. Why CMNS does NOT work is as important as why it works, when it works. Very few (0.1%) of the experiments give excellent results, some tens of percents of them give very mediocre results and a lot of them give immeasurably low results. (As heat excess). . . .

18) Ed Storms wrote:
. . . I agree, neutron production results from a different environment than heat-helium production. Nevertheless, I do not believe nature has many mechanisms at its disposal. Otherwise, these effects would have been seen more often and in many different types of environments.

19) Ludwik wrote:
Earlier in this thread I wrote: (a) Regions should be distinguished in terms of physical parameters, not in terms of desirable effects. That was my main point. (b) The last sentence is an example of circular reasoning. First we define NAE in terms of desirable effects and then we say, "it is obvious" that such effects will take place in NAE.

I was responding to Ed who wrote: "Frankly, I'm at a loss to understand why the concept of NAE is so hard to accept. The concept only acknowledges that the LENR reactions do not occur in ordinary materials, but only in special regions. We need to call these regions something if we wish to communicate about the differences between these regions. The NAE is a description used to distinguish one region from another. We say one region of a material is smooth and another is rough as a means to make a distinction. The idea of NAE is no different. One region can initiate a nuclear reaction and another region can not. What is so difficult to understand about this approach? In addition, it is obvious if we were able to make more of this special environment, we would be able to initiate more nuclear activity."

What triggered my message was the word "obvious" in the last sentence. The concept is not at all difficult to accept. Ed's educated guess about a special region, named NAE, is reasonable. But the word "obvious" makes sense only if NAE is defined as the cause of CMNS phenomena. Suppose, as Ed wrote many times, that NAE is defined as a region containing X, Y and Z ingredients, or conditions. Then things become much less obvious. First, the ingredients and conditions must be identified, then one must show, either theoretically or experimentally, that CMNS phenomena become reproducible when X, Y, Z are present. Why should this be obvious to us at this stage? It was only the word "obvious" that turned a reasonable guess into tautology. I am sorry for not making this sufficiently clear. Let us hope that things will become obvious in not too distant future.

20) Ed Storms wrote:
What is obvious is that CF needs a special environment. I do not need to define what that special environment is to make this statement. For example, I can say it is obvious that an embryo needs a special environment to grow into a functioning human baby. I do not need to define what that environment needs to be. Of course, if I intend to grow a baby from an embryo, I need to know the details. Even without this knowledge, I can call the special environment the uterus without any argument. Why can't I call the special environment needed for CF a NAE without argument?

21) Ludwik (not posted)**:**
Yes, processes we know need support. Saying this is like saying that every real effect has a cause. But scientific community has not yet recognized reality of a single CMNS process. In other words, it does not agree that the embrio exists. We are still trying to built a reproducibal-on-demand demo. That is why many might think that speculationg about NAE is premature. They have right to ask for at least one reliably reproducible demo before accepting speculations about CMNS phenomena. They are likely to say that what is obvious for an embrio is not necessary obvious for CMNS phenomena. Offering a single reproducible demo seems to be a better step toward recognition than offering educated guesses. But, as expected by Ed, educated guesses might help us to select a path toward a potential success.

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310) Alchemy versus CMNS?

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This unit is an update on what has been described in unit #186.
In what follows I am using blue to quote myself and green to quote others.

Introduction:
On Friday October 13 of 2006, Roberto Monti, an independent alchemy researcher, posted a message on a restricted Internet list. He thinks that some ideas, usually attributed to John Fisher, should be attributed to him. A dispute about priority is not worth discussing unless the idea becomes part of generally accepted science. This message was prompted by Roberto's website that was given as a reference.

http://www.lowenergytransmutations.org/papers.htm

A real alchemist among us
In a subsequent message he wrote "my website has had more than 9000 visitors and more than 22000 pages have been downloaded (since 2004, when we decided to put there a counter)." I went to the website and downloaded his second file. After reading it I posted the following comment on the restricted Internet list for CMNS researchers.

. . . Referring to the method described in (17) Roberto wrote:

We *washed*, following Geber's instructions, 500g of Mercury in *very good Vinegar*, obtained mixing 1/2 liter of Vinegar with 1/2 liter of Acetic Acid for 2 - 3 months. Then we took 100g of the *washed Mercury* from the 500g. Once dissolved in Nitric Acid 1 to 5, the 100g showed 55mg of Gold crystals. After 2 more months 100g of the same lot of 500g, dissolved in Nitric, gave 88mg of Gold crystals. Analyzed by SEM and ICP the Gold crystals showed to be from 100% to 90% Gold. The most beautiful we have ever seen (see photographs) (18).

17) I. Filalete, *Open Entrance to the Closed Palace of the King*. Phoenix, Genova 1987, p.11.
18) R. A. Monti, G. A. Cesarano-Monti, Metallic Transmutations induced by Acetic Acid. J. N. E.

What is not clear to me is who are "we" in the first sentence above. Is it Roberto et al. or is it Filalete et al.? The J.N.E. probably stands for the Journal of New Energy. When was the (18) published? Was it discussed at earlier ICCF gatherings? What did other CF researchers say about it? Roberto also wrote:

On April 1992 John Bockris invited R. A. Monti to join and witness experimental tests suggested by Joe Champion (The Philadelphia Project). The first test showed immediately what was going on: it was clearly an attempt to replicate the *Twelfth Key* of the *dry way to the Philosopher stone* of Basil Valentine (19).

Bockris and Champion knew very little about Alchemy. Fortunately April is *the right season* for Transmutations. Consequently the experiments showed definitely - during April and May - the production of Gold and other Noble

Elements from the ignition of a mixture of Metallic Salts and other Elements (20). In September 1992 the Philadelphia Project finished.

J. Bockris is often described as a leading electrochemist of the past century. Ed, I think that his alchemy experiments, and those described by Roberto, should not be ignored in your upcoming book about cold fusion. These people, like Piantelli, Patterson and Case (mentioned by Peter Gluck this morning), were part of cold fusion history. Let me again suggest that you write a special chapter devoted to their ideas (even if you disagree with them). Likewise, the commercial applications project described at ICCF11 by E. Anderson, or that described at ICCF12 by S. Krivit, should not be ignored. Let me end by quoting Roberto's paper again, for those who found it difficult to download the 52 Mb file.

After ICCF-5 (Monte Carlo, 1995) it was necessary to have similar experiments repeated in independent laboratories for the *validation* of the process . In 1996 an industrial reactor was built in Canada and sent to Italy for a new series of independent tests at ENEA, Saluggia (Italian National Laboratories). In these tests the production of Silver from Lead was used as a driver of the transmutation of Thorium and Uranium. Thorium was reduced by 88% . Uranium by 30% (23).

To make a further demonstration of the reality of these experimental results a new series of tests was carried out on May 21 and May 25 1998. The first experiment (May 21) showed the transmutation of 1.32g (30% of the total) of Uranium (23). The uncertainty declared by the laboratory was between 5 and 10% . To avoid any possibility of error we decided to show the possibility to increase this result through a slight change in the proprietary formula used, suggested by the Alpha Extended Model of the Atom (1), (5). Consequently a second test was carried out on May 25, 1998, with the only addition of 50g of SiO2 (powder) in the same composition used for the test of May 21. The result was the transmutation of 2.07g of Uranium (45% of the total). An increase of 15% (+ 50% compared to the test of May 21). . . .

In 2002 (October 9) we made another demonstration at the Royal Institute of Technology (Stockholm, Sweden). It was the first time we tried a test, with thorium, in the second window of the year. Three ignitions were made (see the videotape and History section). The samples obtained from the first ignition were taken, for analysis, by G. Godowski . The results never returned (disappeared). The samples from the second and the third ignition were taken by J. Coleman and analyzed by R. N. Barnes (26). A very interesting result came out: using the mixture with 50g of SiO2 also the thorium in the slag was destroyed (about 100% of the total). We need, obviously, more tests made in the second window of the year, to verify this experimental result. . . .

A comment:
If highly radioactive wastes from nuclear reactors could be turned into non-radioactive substances then the main objection to nuclear energy from fission reactors would disappear. That would be a totally unexpected, and highly appreciated, gift to mankind from CMNS community. I do not think that this is possible without using highly intense sources of neutrons, as suggested, for example, by Carlo Rubia at CERN. But I would be happy to change my mind. Godowski, who you mentioned, was (is?) deeply committed to Rubia's project.

Replying to the above Dr. Roberto Monti wrote:
Filalete published his book in 1645. Or you are kidding or you show deep ignorance ot the history of alchemy. My website is enough. I do not need any further publicity. There is even too much Information. I know that many of the experiments that I suggested, have already been repeated successfully by many people. The French, in particular, have a peculiar attitude: whenever one of my tests is successful, they put on it: "Classified." My reply: “Yes, I had no idea who Filalete was. What confused me was the date at your reference #17”

Is alchemy part of CMNS?:
Then, addressing Ed Storms, who is writing a book about cold fusion, I wrote: ”I think that his [Bockris'] alchemy experiments, and those described by Roberto, should not be ignored in your upcoming book about cold fusion. These people, like Piantelli, Patterson and Case (mentioned by Peter Gluck this morning), were part of cold fusion history. Let me again suggest that you write a special chapter devoted to their ideas (even if you disagree with them). Likewise, the commercial applications project described at ICCF11 by E. Anderson, or that described at ICCF12 by S.

Krivit, should not be ignored. Why should (or should not) a book about CF inform readers about all aspects of CMNS? Why to focus (or not focus) only on those ideas which, the author beliefs, have a good chance of being accepted? Perhaps someone will answer these questions.

In the immediate reply Ed Storms wrote: " . . . My criteria is to discuss that which is done with sufficient skill and described with sufficient clarity to allow an educated person to make sense of the concept and accept that reality is being described, even when the reality is not understood." Does this apply only to Monti or to Piantelli, Patterson and Case as well? In one of his messages Roberto mentioned that his papers, presented at ICCF11 and ICCF12, were not included in the proceedings. But, as I just verified, Case and Pattreson are also not mentioned in the index of authors of ICCF11 papers. I did hear Case's presentations at ICCF11. I suppose that the issue of publishing or not publishing certain papers was discussed among conference organizers. They probably decided that certain topics had nothing to do with CMNS. Or perhaps they were afraid that publishing alchemy papers would give ammunition to those who claim that all cold fusion researchers are practitioners of voodoo science.

Another voice:
Referring to an experiment performed by a friend, X -- he prefers to remain anonymous -- wrote: "Effectively in 2000 a test of transmutation, according to the protocol of our friend R. Monti has been performed by a friend of mine; he was the chief of a big R&D lab completely devoted in the field of explosives and ammunitions. ..... Two tests have been successful in transmuting Hg into Au. The total of the mixture was in the range of 50 grams. In the best test 0.6 grams of gold were recovered by the classical way of Aqua Regalis. The quality of Au has been tested by a mass spectrometer." Then X described bizarre circumstances under which the report was classified. The alchemically produced gold is now in two wedding rings.

Another comment:
Dr. Monti is apparently not the only one who takes ancient alchemy seriously. In my last contribution to this thread I wrote: "I suspect that most people on this list, like most mainstream scientists, have the same attitude to medieval alchemy as I have. We were educated to think that transmutations of elements by chemical means are not possible. And we all believe in nuclear alchemy. What would I do if I had a simple reproducible-on-demand demo of a chemical transmutation? Taking negative attitude toward old alchemy for granted, I would focus on the demo itself and not on the fact that it is based on what was described long time ago. Any chemist should be able to replicate a procedure that was used in 1645, provided it was properly described. I would say "I do not know why it happens; but it does happen. Please confirm or refute my claim before jumping to a conclusion." That would be more productive than anything else, at least for the time being.

Roberto, I would be happy to make your simple protocol known to science teachers. Please describe it clearly and I will post it on my website. But avoid the case of turning mercury into gold, many people would simply smile instead of performing experiments. You wrote: '. ....A very interesting result came out: using the mixture with 50g of SiO2 also the Thorium in the slag was destroyed (about 100% of the total). We need, obviously, more tests made in the second window of the year, to verify this experimental result.' Please describe the protocol for destroying thorium by a chemical procedure. And do not mention 'the second window of the year.' Most people expect reproducibility at any time.

No matter how strongly one disagrees with Monti it is important to keep in mind that "the proof is in the pudding. " Suppose that a procedure to transform one element into another, by using chemical means, is confirmed by many experimentalists. That would be a proof and we should be able to accept it. The burden of offering the procedure, as always, should be on those who make claims, not on those who are asked to accept claims. Reality, however, is often more complicated. It has to do with prejudice, with money, with conservatism, with fanaticism, etc. etc.. Will I live long enough to witness rehabilitation of old alchemy? Most likely not. For the time being I will accept general belief that non-nuclear alchemy, like astrology, is pseudo-science.

Appended on 10/19/06:
1) This is about alleged discrimination (not publishing Monti's ICCF11 report in the proceedings). Prompted by Roberto, the chairman of the conference, Jean Paul Biberian, wrote: "The reason your paper was not published in the proceedings of ICCF11 is simply that it was not a scientific paper. I was very open to publish papers at the border line

of CMNS: I accepted a paper on alchemy by Pérez Pariente. . . " Yes, many controversial presentations (but not as controversial as making gold from mercury) were published in the proceedings.

2) Gold from mercury or gold in mercury? I know that everything is present in everything, in a traceable amount. Is it possible that tens of milligrams of gold, shown on the photos from Roberto web site, were initially present in mercury, or in the vinegar, used to produce tiny crystals? Why was the question not addressed in his paper? What is obvious to the author is not always obvious to readers.

3) As mentioned in the unit #186, Monti's process of destroying radioactive waste by chemically induced transmutations is at the base of an anticipated commercial project. We know nothing about that project, except that progress is being made. The company wants to keep low profile at this time. This is understandable, they do not want to reveal technological secrets to potential competitors. But there is also a negative aspect in such attitude; discussing scientific aspects with scientists on the CMNS list can be very helpful. That is probably a well known dilemma. Is it possible to discuss purely scientific aspects of a pending application without revealing the "how-to-do-it" details? Assuming that the answer to this question is positive, I want to ask some questions about the proposed transmutations of NORM (Naturally Occurring Radioactive Material). Presumably such materials will be mixed with industrial byproducts (tons of waste?).

(a) Is Roberto's transmutation process selective or does it have about the same efficiency for all NORMs: U, Th, Ra, Po, Rn, etc.? (*) Roberto describes efficiency in terms of the "decrease per ignition." For example, referring to 1998 experiments, he wrote: "The first experiment (May 21) showed the transmutation of 1.32g (30% of the total) of Uranium."

(b) What instruments are used to measure transmutation efficiencies?

(c) How many tests were performed in the last two years? How reproducible are the results?

(d) How certain is Roberto that the decrease in the activity of NORMs is due to transmutations of atomic nuclei rather than to their redistribution (changes in counting geometry)? Or how does he know that the decrease is not due to dispersion of radioactive materials into the environment?

One often hears the old English proverb about proof being in the pudding. But what kind of commercial pudding will convince us that Monti's claims are valid? It has to be a set of reproducible-on-demand experimental data and the discription of the "reactor's" anatomy that most us would consider to be free of artifacts. What else can it be, Roberto?

To make clear what artifacts I would look for, let me describe an ongoing experiment. For nearly 7 weeks I have been measuring radioactivity of a hyperthyroidism patient (my wife) who received 8 microcuries of the I-131. The half-life of that isotope is eight days. But a Geiger counter, situated in the same position with respect to the throat, was showing that the counting rate was decreasing much faster, especially at the beginning. The first measurement was taken about three days after the pill was taken. A naive person might conclude that Linda's body transmuted radioactive iodine into something non-radioactive. But that would be wrong. A more credible explanation is that a significant amount is iodine was biologically removed from the body (dispersion) or that the distribution of the iodine within the body was changing (counting geometry becoming less favorable). The first hypothesis was confirmed by the fact that Linda's saliva, for example, was found to be radioactive. How does Roberto know that dispersion and redistribution were not responsible for what he attributed to pyrolitically induced nuclear transmutations of uranium and thorium?

Scientific papers posted on out web sites should meet standards imposed on papers published in referred journals. Reasonable objections should be anticipated by the authors and addressed in what is published. The same should be true for papers presented at our conferences, as indicated by Jean Paul.

Appended on 10/21/06:
Here is what one reader of the above wrote to me in a private message last night:

". . . Regarding mercury transmuting to gold: Read pages 540-542 in Strong's Procedures in Experimental Physics.

This gives good reasons to be suspicious of claims of "transmutation", especially if one knows nothing about the history of the mercury used in the experiment. Mercury almagamates with many metals (as with silver, used for tooth fillings). The more likely explanation of the "transmutation" is contamination. If the experimenter does not know what is in his mercury before the experiment, then the results of the experiment must obviously be in question. Certainly the mercury of 1645 would be suspect. ....." Yes, this observation is valid. The burden of proof is on Roberto. Perhaps he will share the results of chemical analysis of mercury used in transmutation experiments with other CMNS researchers. Mizuno, who also performed transmutation experiments, was very specific about purity of various materials, including water.

But contamination can no longer play a significant role in experiments during which nearly 50% of the total is transmuted, as in the case of Roberto's May 25, 1998 experiment with uranium (see above). In that case the suspected artifacts are dispersion and changes in the counting geometry. Note that if 45% of uranium (about 2 grams) was transmuted then about 2 grams of something else should have been created. What was it? I suspect that Roberto has no answer to this question. But he is certainly aware that the question is very important -- it is likely to be asked by critical readers. Did you try to demonstrate appearance of about two grams of a new substance, Roberto? That would make your transmutation claim much more credible than it is.

What did John Bockris have to say about contamination in his alchemy experiments? He is a worldwide authority on chemistry and electrochemistry -- a teacher of teachers, as someone wrote. I saw his textbook and I read that Fleischmann was his student. Is it conceivable that Bockris also ignored to address the issue of possible contamination? I do not think so.

Appended on October 24, 2006:
Let me mention that George Washington bridge, admired each day from the balcony of our new apartment, was inaugurated exactly 75 years ago. Was it simply a coincidence that I was born on the same day? An astrologist might think so -- and probably explain the coincidence in terms a stellar configuration .....In a message posted on our restricted list, Bill Collis wrote:

"Your quite right in raising the question of dispersion and also asking about the instrumentation used to measure any transmutation. The two issues are connected. Both Th232 and U238 decay by alpha emmission which penetrate Geiger counters with difficulty or not at all. Some gamma radiation is produced (see PCNUDAT on the ISCMNS CD-ROM/DVD-R) but at low energy and intensity. Put a Geiger counter near thorium or uranium and it buzzes frantically. This is due to gamma radiation from the beta decays of the daughters. You can demonstrate this easily by placing an absorber between the source and the Geiger tube. Elimination of radioactivity by "ignition" probably involves temperatures greater than 1500 degrees. Many of the radioactive daughters of uranium and thorium, such as Radium, Francium, Radon, Astatine, Polonium all boil below these temperatures. Of course we should also realize that the boiling point of their chemical compounds may be different. Nevertheless, the prospects for dispersion into the gas phase look rather high."

That is why a very hot piece of uranium might appear to be less radioactive than the same piece before heating. Was a possibility of such scenario discussed by Roberto? If so then I am not aware of it. Responding to Bill, I wrote: "I would like to know what Roberto thinks about the role of dispersion. Ideally, it should be prevented; less ideally (in a preliminary experiment), it should be accounted for. That is not a simple task. On his website Roberto wrote that nearly 50% of uranium -- about 2 grams -- was transmuted in May of 1998. Such outcome, if possible, would be very desirable. Roberto, what allowed you to conclude that transmutations were real?"

Will he reply to our questions? A well known nuclear scientists once wrote, many years ago, that "researchers have the responsibility to publish their own experimental data. They should be in a position to explain and defend their results to other qualified experts. This is especially true when the reported data are controversial and directly contradict well-established scientific results in the literature. The description of experiments and results should be published in sufficient detail to give the expert reader the possibility of evaluating the significance of the claimed result." How can one disagree with this? Personally, I do not think that Roberto paper contains good arguments for reality of chemically induced nuclear transmutation. And, for some reason, he is not willing to reply. That is not a good sign. Something is not right somewhere.

This birthday card came today, 10/24/2006.

Appended on October 27, 2006:
A long awaited reply from Roberto was posted this morning. He addressed concerns of several CMNS researcher. But my questions (about dispersion and the counting method) were not addressed. Why is it so? In an earlier message Monti asked Bill Collis to translate his Italian papers. Here is the reply from Bill: ". . . As I wrote before, I'm quite happy to discuss your theoretical works privately. However I suspect they will not pass peer review and consequently any translation work will be in vain. I would suggest instead you write an experimental paper, describing transmutation. It should include proper blanks or controls. It should use nuclear instrumentation to verify claimed nuclear effects. Such a scientific paper, if you can write it, should certainly pass peer review. I will be happy to consider translating it, or correcting it, free of charge. But I'm not prepared to discuss translation of any kind, whether free of charge or for a fee, unless I first see the manuscript. I think that's fair isn't it?" I did not know that Roberto is also a theoretician. His paper about Einstein, shown at:

http://www.lowenergytransmutations.org/papers.htm

is just as strange as his experimental report about fabricating gold from mercury. I cannot accept Roberto's conclusion that Einstein goofed on special relativity. We know that equations of that theory have been shown valid many times. High energy accelerators, used in numerous laboratories, were designed with that theory. These complex devices work because the equations are correct. Perhaps someone will post a message evaluating Roberto's theories of transmutations. Let end with the following dead-end speculation about making gold.

It is a well known fact that 10% of natural mercury is the isotope ^{198}Hg. A stable ^{197}Au (common gold) would be created if one proton was emitted from the ^{198}Hg nucleus. What could be more simple than this? Unfortunately, the atomic masses involved in such emission are unfavorable:

^{198}Hg 197.96677 amu
^{197}Au 196.96655 amu
proton 1.00782 amu

The sum of the last two masses, 197.97437 amu, exceeds the mass of the ^{198}Hg by 0.00760 amu. In other words, spontaneous emission of protons from ^{198}Hg is energetically forbidden; the minimum energy needed to emit a proton is $m*c^2 = 0.00760*931.48=7.08$ MeV. Yes, I am using Einstein's famous $E=m*c^2$ equation here. Is it possible that a chemical process can supply a ^{198}Hg nucleus with more than 7.MeV of energy? I do not think so. By the way, the binding energy of a proton in ^{198}Hg, exceeding 7 MeV, is not a wall-like barrier, it is a step-like energy threshold. That is why speculations about catalysts are likely to be fruitless. But what about billions of atoms, each contributing a tiny amount of thermal energy to a single ^{198}Hg nucleus? Something like that would help; if we knew how to concentrate thermal energy of many atoms in a nucleus of one atom.

P.S.
A message posted two days ago (on our restricted Internet list for CMNS researchers) referred to a paper published in the most recent issue of New Scientist. The title of that paper was: "The Cryogenic Model of Nuclear Fusion." Referring to this paper, and to a message about a relevant French patent, Roberto wrote about his much earlier paper on that model. His message contained the following prediction:

". . . They shall have a bigger surprise when they will come to
know that any radioactivity can be easily destroyed in 3 days."

Here is my reply:
1) Are you referring to transmutations induced by neutrons? That can probably be done, but not "easily."
2) Yes, easy destruction of "any radioactivity" will be a big surprise. How soon will we experience this surprise?
3) Will the issues of possible artifacts be addressed?

Efficient destruction of highly radioactive isotopes, in spent fuel from nuclear reactors, would open a new era of nuclear electricity. By the way, rapid turning of radioactive isotopes into stable isotopes will rapidly release usable energy. The process my be able to pay for itself. Let us hope for this.

Roberto, may I have permission to quote the above prediction on my website?

Permission to quote was given but, as before, my questions were not answered. Why is it so? Something is not right somewhere.

Appended on 11/6/06:
Apparently, Roberto decided not to answer my question about dispersing. But messages he did post on the CMNS list indicate that his "Cryogenic Model of Nuclear Fusion" (developed in 1987, two years before the discovery of CF was announced by Fleischmann and Pons) played an important role in subsequent investigations. The word "cryogenic" usually refers to temperatures near absolute zero. Roberto's paper is in Italian but it will be translated into English and posted on his personal website, in a month or so. If I understood posted messages correctly, the model claims that the temperature at the center of the sun is lower than at the surface. The surface temperature, only about 6000K, is certainly too low to allow for generation of hot fusion energy, and for conversion of hydrogen into helium. What will astrophysicists say about this model? Will they conclude, incorrectly, that all CMNS claims are as unrealistic as Roberto's? I hope not.

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311) A contribution from A. Alberts

Ludwik Kowalski; 11/xx/2006
Department of Mathematical Sciences
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About two months ago Albert Alberts, from Netherlands, mentioned some observations made by Otto Reifenschweiler. This was on the restricted Internet list for CMNS researchers. Asked for a clarification, Alberts wrote:

"The 'Reifenschweiler effect' is the observation that the beta-decay of tritium half-life 12.5 years is delayed reversibly by about 25-30% when the isotope is absorbed in 15 nm titanium-clusters in a temperature window in between 160-275 C. Remarkably at 360 C the original radioactivity reappears. The effect is absent in bulk metal. Discovered around 1960/1962 at Philips Research Eindhoven, The Netherlands Reifenschweiler extensively discussed his observation with o.a Casimir (the director of research at the time), Kistemaker (ultracentrifuge expert), and although no satisfactory explanation was found, R. was allowed to publish it. At the time a unique example as to how an electronic environment might affect nuclear phenomena."

In a private e-mail message Alberts, who used to be a physics teacher, wrote: "I don't know if that work was replicated, he was invited to, I think, Sandia and later to Los Alamos to demonstrate the effect, instigated by a fellow named Cox. What I do know is that it was replicated in the Philips labs many times, supervised by the eminent Casimir at the time. The beta-rays were detected by Geiger-Mueller counters, at the time Philips had the most advanced evaporation/evacuation techniques possible. Even electron microscopy. The work was criticized by a German fellow, but Reifenschweiler retaliated. That stuff should be in his reprints." He also wrote that he is in contact with Dr. Reifenschweiler (who is quite old) and that he has some old manuscripts. Subsequently I asked Alberts to compose an essay on Reifenschweiler effect. He agreed. His essay will be shown below as soon as it arrives. I think Reifenschweiler himself will assist in writing a good summary. Meanwhile let me mention that a French researcher, Fabrice David, also summarized Reifenschweiler effect in 2004. That was the topic of his presentation at ICCF11 (11th International Conference on Cold Fusion) in Marseilles.

ALBERTS' ESSAY WILL BE PLACED HERE

Click to see the 1976 patent (4 pages).

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312) Another kind of beta decay?

Ludwik Kowalski; 10/28/2006
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What follows is a message I posted on a restricted Internet list for CMNS researchers. It was prompted by a speculation, posted by another researcher, that half lives of radioactive nuclei can be reduced by keeping them at extremely low temperatures. My piece, ironically, is about speeding up the decay of some nuclei at extremely high temperatures.

Here is an idea that preoccupied me more than 25 years ago. It is recorded in a note from an old binder. I expected the half life, in beta-minus decay, to be reduced when atoms are ionized. Some bare radioactive nuclei (atoms without electrons) in very hot plasma, I speculated, would decay faster than nuclei in neutral atoms. This was based on what is known about beta-plus decay, where two decay paths are available (two ways of changing a proton into a neutron): (a) positrons are emitted or (b) orbital K electrons are absorbed by nuclei. The second path is known as EC (electron capture). The half life of a beta-plus decay would become longer if the second path could be prevented, somehow. By how much longer? It would depend on the fraction of nuclei decaying via the EC path. That fraction, by the way, is called branching ratio.

In the beta-minus decay (changing a neutron into a proton) we have only one path -- emission of an electron. The second conceivable path -- placing an electron into an atomic orbit of neutral atom -- is forbidden by Pauli principle. It is not possible to add an electron into an already-occupied orbit. The central idea was that a new path for the beta-minus decay is automatically open when bare nuclei are formed. By opening another decay path one would speed up the decay of some beta-minus radioactive nuclei.

Unfortunately, total ionization of atoms calls for temperatures that are probably too high for practical applications. Why did I not think about this when I first heard Roberto at ICCF11 ? He was talking about a "pyrolytic method" of destroying radioactivity. I do not know what kind of process it is and how high were his temperatures. My old binder shows that I was anticipating two hot plasma experiments. This was discussed with two plasma physicists in Princeton. The experiments never materialized. Here is a note composed on 1/5/1980. ". . . The second experiment in hot plasma has to do with "speeding up" the decay of beta-minus radioactive nuclei by a process that can be called orbit population, OP. In this process the transformation of a neutron into proton is associated with the emission of an antineutrino and with the appearance of an electron on an empty atomic orbital." Branching ratio formulas for the beta-plus decay can be found in many textbooks. Assuming the formulas are applicable not only to EC but also to OP (in fully ionized atoms), I was able to show that changes in half-lives can be very significant. I wrote that according to preliminary calculations, "the half-life of bare ^{106}Ru should be reduced by a factor of 23, as compared with neutral atoms. [That means about two weeks instead one year.] In most cases, however, the reduction factors are not that large." . . .

P.S.

(a) Temperatures T needed to produce nearly total ionization of atoms are high. The values of kT, where k is Boltzmann's constant, are usually expressed in keV (several keV, for light elements and up to about 100 keV for the elements at the end of the periodic chart).

(b) The OP process probably does take place in many stars. I would not be surprised to learn that it is taken under

consideration by nuclear astrophysicists.

Appended on 12/10/06: For some reason, in speculating about the OP process nearly three decades ago, I was not aware that it was discussed by others. The paper of Gareev, presented in 2004 at ICCF11, discusses this topic much better. The title of the article was “Enhancement mechanisms of low-energy nuclear reactions” and the authors were: F.A. Gareev, I.E. Zhidkova and Yu.L. Ratis.

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313) Nonscientific aspects of CMNS

Ludwik Kowalski; 11/17/2006
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This morning Peter Gluck started a thread “we are not alone.” That was on the restricted list for CMNS researchers. Referring to two recent publications in Science, he wrote that high temperature superconductivity, like CMNS, is also struggling with explanations of experimental discoveries made many years ago. Several hours later, John Coviello posted the following reply.

“It is interesting how relatively uncontroversial high-temperature superconductivity is compared to cold fusion, considering they are both in the same quandary of lacking an explanation that satisfies mainstream science. What are the differences? Perhaps the fact that cold fusion could destroy a multi-trillion dollar fossil/nuclear fuels industry, while high-temperature superconductivity barely even threatens multibillion dollar communications and power transmission industries is a factors? I say barely because high-temperature superconductivity would not doom the industries, in fact, it would cause a new wave of spending on infrastructure and equipment upgrades and create a lot of business activity, as firms rush to install the latest technology, so perhaps they have nothing to fear at all. Cold fusion would mostly end the financial prospects of the bloody oil trade, dirty coal industry and wasteful nuclear fission industries. Is it any wonder cold fusion enjoys few friends in high places given the implications of its commercialization?”

Three people responded. They pointed out that, unlike in CMNS, experimental observations in the area of high temperature superconductivity turned out to be reasonably reproducible. Here one of the responses; it was posted by Scott Little. He wrote:

“My son and I made some YCBO blocks for his junior high-school science fair project in 1991. around 4 years after their discovery. Following a published recipe, the first batch we made worked well enough to demonstrate the Meissner effect (magnet levitation). I still have these superconductors. They still work. Somehow that doesn't remind me of cold fusion at all. ....)”

“What I would like to know, is whether or not factors mentioned by John played any role in creating the discriminatory attitude toward CF, in early 1990s. The young lady, CF, Ed Storm's mistress, is going to be 18 next spring. That is about time to be treated as adult science. Will Ed's book help? Will confirmations of San Diego results help? Will conformation of Oriani's results help? We will see.” That was my own comment on the list. No one responded so far. John was not the first to point the accusing finger at oil companies. What evidence is available to support such accusations? Perhaps other factors, such as personal animosities, jalousie, and, above all, competition for grant money, should also be blamed for excommunication, if I can use this term, of cold fusion in early 1990s. I hope that others will help us to understand the mechanism of the process of social discrimination of cold fusion. The "nonscientific" aspects of the history of CMNS are interesting; I hope at least some of them will be addressed in the book about cold fusion that Ed Storms is writing. Let me mention that I first saw the word “excommunication” in one of his essays, written several years ago.

Personally I am not convinced that oil companies are behind those who use administrational means (like rejecting papers, not accepting patents, etc.) to discriminate against cold fusion. Big companies would probably be among the

first to invest heavily in CMNS technology, if convinced that it is promising. But blaming of “rich and powerful” is not new. What evidence do we have that oil companies conspired against cold fusion in 1990s?

Appended on 11/23/06

The only reply, so far, was that suppressing an emerging new energy technology (such as electric cars) would be in the interest of oil companies. That is not a proof of conspiratorial manipulations. When Enron executives were accused of such manipulations they were confronted with convincing proofs, I believe. The argument “that would be in their interest” is not sufficient. Being prudent and not investing in the not-yet-demonstrated technology is a virtue. Automatic blaming of rich and powerful for everything bad is not new. Sometimes these “bloodsuckers” are responsible for bad things but sometimes bad things have other causes. I am thinking about hurricanes, epidemics and famines, for example.

The more I think about this the more am I convinced that accusing oil company executives of conspiracy against cold fusion makes no sense. Why would they be less concerned about consequences of pollution and global warming that most of us? Most of them also love their children and grand children. And they always look for opportunities to make money. Replacing declining oil resources by CMNS sources of energy would offer a chance to make money. Therefore, it is only logical, to expect them to sit and wait for opportunities to make money. Some of them are probably watching carefully what is going on in the CMNS fields. Why should they think that CMNS is “ready to deliver” when we still do not have a reproducible-on-demand demo?

Absence of additional comments is a good indication that most CMNS researchers do not blame oil companies for discrimination against cold fusion. This can be contrasted with what one often reads at open websites at which cold fusion is discussed by non-practitioners. At those websites accusations of oil companies are much more frequent.

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314) CMNS Research in the US Navy Lab

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Introduction

1) This unit is prompted by what has just been published by Steven Krivit. He is a journalist who follows CMNS developments and writes about them in the online magazine called New Energy <http://newenergytimes.com >. He also wrote a book devoted to the controversial CMNS field. That book, by the way, can be ordered from the above website. Before reading beyond this introduction, please go to

http://newenergytimes.com/news/2006/NET19.htm

and click the link to item #7, entitled "Extraordinary Evidence." Read the item carefully and then read my comments below. I do not want to repeat what has already been described on the above webpage; I only want to share some observations.

2) Yesterday, in a personal message to me, Steven wrote: "The field of low energy nuclear reactions, historically known as cold fusion, has never had simple physical evidence of the claimed nuclear processes to physically place in the hands of doubters. Until now. Scientists at the U.S. Navy's San Diego SPAWAR Systems Center have produced something unique in the 17-year history of the scientific drama historically known as cold fusion: simple, portable, highly repeatable, unambiguous, and permanent physical evidence of nuclear events using detectors that have a long track record of reliability and acceptance among nuclear physicists. Using a unique experimental method called co-deposition, combined with the application of external electric and magnetic fields, and recording the results with standard nuclear-industry detectors, researchers have produced what may be the most convincing evidence yet in the pursuit of proof of low energy nuclear reactions. "

3) Naturally, I was not the only one to read about Steve's journalistic description of the work in San Diego. The publication was announced on the selected list for CMNS researchers. A scientific paper describing the results was submitted to a refereed journal in September; I hope it will not be rejected. On the same day the following message ws posted by another CMNS researcher, Scott Little. He wrote: "Ludwik, you have considerable experience with CR-39. What is the possibility that these results, despite the controls that have been run, are due to some sort of chemical/electrical/physical interaction and not energetic particles?" Responding to this I wrote:

"Scott, I am glad you asked. As it turns out, in the last several days I was trying to answer the very same question about the new results of Richard Oriani. Last summer he sent me the unpublished paper describing the results. I am considering a possibility of working with Richard again, in the near future. With his permission I am happy to summarize the results. The bottom line is that, according to Oriani, nickel cathodes, after being used in a simple electrolytic cell, emit nuclear particles. His average result for used cathodes, from 32 experiments, was 89 tracks per cm^2 while his average result for virgin cathodes, from 24 experiments, was 20 tracks/cm^2. The electrolysis times and CR-39 exposure times (after cathodes were removed the cell) fluctuated between 2 and 4 days. For the purpose of statistical analysis I assumed that the average electrolysis time was 3 days while the average CR-39 exposure time was 2.5 days.

Richard, please confirm that this is correct, or give me correct times. Actually, the number of post-electrolysis experiments was larger than 32. But I eliminated the Pd cathodes results and one result for the Ni (because the electrolysis time was only 1 day). These eliminated cases were not significantly different from the 32 cases I analyzed statistically. But why should a statistical analysis be done on a mixture of very different cases? I also eliminated one extraordinary case in which the track density, from the used Ni cathode, was about 300 per cm^2. Yes, I know that this might turn out to be the most important case. But textbooks tell us to examine distributions before analyzing them, and to extract outliers. That is why the most favorable case was removed. I wanted to follow a standard statistical protocol. The so-called "null hypothesis" was that two slightly overlapping distributions (32 results 24 controls) were from the same population. The alternate hypothesis, that two distributions are really different, prevailed. My conclusion agrees with what Richard wrote in the draft -- the probability is vanishingly small that the difference 89 and 20 is coincidental. Yes, standard deviations are large, but they were calculated from Richard's data. In other words, random errors were realistically accounted for, to reach the conclusion.

I will provide more details about what I did in another message. And I hope Richard will tell us what the current status of his manuscript is. We are anticipating an experiment in which the CR-39 results will be compared with results obtained by using a silicon detector. The major technical difficulty will the very low counting rate, perhaps only two or three particles per hour. This is slightly above what is said to be possible with a commercially available unit from Erotic. Being an optimist I expect the background to be below 0.5 per hour, when the detection threshold is increased, for example to 4 MeV. Is this realistic? The advantage of using CR-39 is that it allows to work at much lower average counting rates, provided pre-counted CR-39 chips are used, as done by Oriani. . . .

The advantage of an electronic detector is that it will give energies of particles responsible for CR-39 tracks. Just imagine what one would be able to say if the energies turned out to be higher than 8 MeV. That would immediately rule out a possibility that what is being detected is due natural alpha radioactivity of something in our environment. On the other extreme, one may find that the energy histogram is consistent with energies for Rn etc. That would cause us to focus on a possibility of contamination. Another great advantage of electronic detection would be an opportunity to study the time dependance of counting rates. Do they decrease significantly from day to day or do they remain constant? . .

Here are actual Oriani's results;

1) Controls, CR-39 chips, exposure 2 to 3 days

(tracks/cm^2):
18,15,39,23,45,21,27,0,10 0,22 26,36,22,34,14,10,17,10,25,16,21,24,12
24 results, mean value=20.3; standard deviation=11.1

2) Used Ni cathodes, CR-39 chips, exposure 2 to 3 days

(tracks/cm^2):
145,128,116,149,133, 145,63,45,66,118,69,96,180,127,84,115,57,79,65, 71,74,71,41,66,43,81,71,53,56,69,99,80
32 results, mean value=89.2; standard deviation=35.8

= =

This is the end of my introduction.
Please go to the "Extraordinary Evidence" item now, and read it before
reading what I have to say.

http://newenergytimes.com/news/2006/NET19.htm

= =

Appended on 11/15/06:

The ongoing Internet discussion of SPAWAR results, on our restricted list, is going on. I was reading numerous messages and thinking about the best way to summarize them here. Meanwhile, Pamela Boss, who leads the SPAWAR project, wrote to me in private: ". . . Once we've got this Q&A to a mutually satisfying place, we'll review the verbiage and get you a cleaned up version that we feel comfortable with you posting very soon." That is great; what could be better than this? Please read her input below before jumping to my closing comments.

Appended on 11/19/06:

I sort of expected to receive pieces from Pamela and from Steve during this weekend. But nothing was sent to me. I suppose Pamela is very busy with the ongoing experiments. So I should wait. Meanwhile I started speculating about the Frapa -Stanila effect discovered in San Diego. The suggested name seems to be appropriate; it is made from first names of four SPAWAR researchers: Frank, Pamela, Stanislaw and Larry, in the order in which they were introduced in Kriven's report. This temporary name has some melody; perhaps it is appropriate for a folk dance to be created to celebrate the 2006 discovery. But what if Frapa-Stanila refers to something bad or impollite in another language? In that case another name would have to be composed. Why don't I say SPAWAR effect ? Because when I tried to guess what the acronym stands for, the first words that came to my mind were "spaying" and "war." I prefer something more pleasant, for example, an Italian dance, Tarantella. But I will be glad to use the name chosen by those who discovered the effect.

Appended on 11/20/06:

Something interesting was posted this morning on the CMNS list by Krivit. He wants the replications of the Frapa-Stanila effect to be organized and coordinated. The first step, labeled alpha; is to have the effect replicated by selected electrochemists. The second step, labeled beta, will be based by the detailed protocole written by the SPAWAR team and refined by alpha group. Anyone willing to be part of the beta project will receive the protocole. This was described in:

http://www.thegalileoproject.org

I would not mind to participate in the beta phase; but not before confirmations are announced in the alpha phase. The idea of coordinating efforts of several teams, working on the same project at the same time, is worth supporting. Will researchers outside the CMNS list, for example, teachers and students, by invited to participate in the beta phase? I hope so.

Appended on 11/22/06:

What follow is a set of answers to Frequently Asked Questions (FAQ) about the Frapa-Stanila effect. It has just be posted by the New Energy Institute,

http://newenergytimes.com/contact/contact.htm

I expect the FAQ list, grabbed from

http://www.thegalileoproject.org/FAQ.htm

to become longer. Keep in mind that the author, Steven Krivit, is a journalist, who has learned a lot about science since he started investigating cold fusion (the new name is CMNS) several years ago. It would not surprise me to learn that the piece pasted below was actually approved by scientists who are investigating the new effect. Steven assumes that readers are already familiar with his article, "Extraordinary Evidence," published in New Energy Times on Nov. 10, 2006 It is item #7 in:

Q1. Did the researchers consider possible sources of contamination?

A1. The SPAWAR team performed a number of controls that indicated that their observations were not the result of radioactive atoms in the solution.

(a) For example, they immersed a CR-39 detector in the co-deposition solution without performing electrolysis. The detector was immersed for the same amount of time as for their electrolysis experiments. The result was that no tracks were observed on the detector.

(b) They performed electrolysis experiments without the $PdCl_2$ in the solution. Result: No tracks were observed.

(c) They performed co-deposition experiments, in the absence of external electric or magnetic fields. Result: No tracks were observed.

With regard to possible radiological contamination, although CR-39 background studies continue at SPAWAR, the reported CR-39 charged particle density is more than 10,000 times the background track count.

If the tracks are caused by alpha or proton emission, as proposed by the researchers, then this indicates energy levels 1 million times larger than any known chemical source. The absence of tracks, as shown in the control studies performed without the required parameters, rules out internal chemical, and both internal and external radiological, cell contamination as the track source.

Q2. Are the researchers claiming that this is proof of cold fusion?

A2. No. They make no claims of any form of fusion reaction with regard to this experiment. Instead, the evidence reported is suggested as a novel, anomalous low energy nuclear reaction.

Q3. Did the researchers measure heat?

A3. No attempt was made to measure heat in this experiment, it was not a concern of the researchers at the time.

Q4. Di-alkyl carbonate polymers will decompose in the presence of a acid, and especially acid + heat to form lower molecular weight species + CO_2. The acid-degraded lower molecular weight chains become carboxylic acid terminated, and hence are soluble in aqueous base, and can be extracted from the polymer matrix. This could give the appearance of pits (as seen in this article). **Could the CR-39 plastic have degraded due to heat,** and thus become soluble in the aqueous base development step, and thus be the result of a chemical, not a nuclear reaction?

A4. SPAWAR researchers reply: "Each of the wires Au, Ag, and Pt were at the same potential of approximately 2 V or less, and the current was approximately 100 mA or less. It was observed that the number of charged particles varied among the wires, but there was a distinct "shadow" corresponding with the back side of the wires closest to, and in contact with, the CR-39. This “region,” without tracks, directly behind the wire constitutes an insitu “control.” If the proposed localized heating is a function of joule heating, the heat conductivity of the various metals is sufficient to prevent local hot spots against the CR-39. If the proposed localized heating is a function of simply D2O dissociation and Pd loading during, or subsequent to, co-deposition and even if anisotrophic, again, the heat conductivity of the various metals is sufficient to prevent local hot spots against the CR-39. Therefore, if the purported catalytic effect causing depolymerization of the CR-39 was occurring, then it should have occurred directly behind the wire in closest contact with the CR-39. It did not.

In addition, we observed tracks at distances exceeding the diameter of the wire and extending over 1 mm, where there was no contact between the wire and the CR-39. Thus, there was not opportunity for exothermic, Pd-loading generated heat or joule heating to provide the catalytic heating required to depolymerize the CR-39, whether or not a localized

low low pH or insitu acid contamination exists.

I know that the authors are still deeply involved in studying the Frapa-Stanila effect, and in revising the initial manuscript. Their paper will probably be published in two or three months. That would be the best time to ask for additional questions. Their argument (c), against a possibility that tracks might be due to alpha-radioactive substances accumulating on the cathode, is very convincing. But if I were a referee of their paper, I would insist on measuring and reporting activities of alpha-radioactive substances in the used materials. Such activities, or upper limits, can easily be measured by using CR-39 detectors.

The most likely source, in my mind, would be water. i know that alpha radioactive substances, such as uranium, radium, radon, etc., are always present in water. Typical concentrations are 1 to 10 pCi/L, but sometimes much more. Suppose that only ^{222}Rn is present and that its initial concentration is 10 pCi/L. How many alpha particles is emitted each hour? (Hint: One Ci emits 37 billion particles per second.) The answer is 133 particles. How many atoms of ^{222}Rn are initially present in the 0.1 l of water? (Hint: The half-life of ^{222}Rn is 3.8 days.) The answer is 17700 atoms. Suppose all these atoms are rapidly electroplated on the cathode and decay there during a 10-days long experiment. Also suppose that 10% of alpha particles are creating tracks in CR-39 detectors. How many tracks will be recorded? The answer is trivial; it amounts to 1770 tracks.

If the same activity of 10 pCi/L was due to ^{226}Ra, (half-life is 1620 years) then the number of atoms would be much larger. Only a very small fraction of them would produce tracks. The number of tracks, however, would be larger, but not astronomically larger, under identical conditions. More specifically, 133 particles per hour translates into 31920 particles in ten days. Ten percent of this is 3192, or about two times more than for ^{222}Rn . The same result would be obtained for any alpha-radioactive isotopes whose half-life is significantly longer than the duration of the experiment. This includes ^{238}U, ^{235}U, ^{232}Th, etc. etc. Naturally, all this would become irrelevant if the initial activity in the electrolyte was shown to be less than 0.001 pCi/L; I do not know what to expect from distilled, or twice-distilled water. Naturally, all this would become irrelevant if the initial activity in the electrolyte was shown to be less than 0.001 pCi/L; I do not know what to expect from distilled, or twice distilled water.

The reader of a paper should be informed about the initial concentration of alpha radioactivity (pCi/L) in order to be even more convinced that tracks are not due to alpha radioactive substances originally present in the electrolyte. The claimed effect -- a nuclear process triggered by a chemical process -- is so unexpected, according what is known, that a reader (and a referee whose role is to help potential readers) is likely to think about various artifacts. What if electric or magnetic field, imposed from the outside the cell, simply deflect the ions that would otherwise be deposited on the cathode? It is not hard, for a knowledgeable person, to invert scenarios in which observed effects are trivialized, in one way or another. The authors of claims must try to anticipate such attempts and address the issues.

This is very difficult when one has no theory to lean on. Nothing makes sense without an acceptable theory. And experimental results that make no sense are very difficult to promote. I am convinced that experimental facts will be explained, one way or another, in less than one year, after experiments become reproducible on demand. This will be accomplished in small steps, by changing one parameter after another and by discussing the results openly. That is the essence of scientific methodology. Secrecy does not help to promote science.

Appended on 11/24/06:

The New Energy report contains references to papers already published by San Diego team. The authors have investigated several indicators of nuclear activities before focusing on the use of CR-39. One of these papers (3), by Stanislaw Szpk, Pamela A Mosier Boss, Charles Young and Frank Gordon, describes morphological changes in the cathode and production of new elements in presence of an external electrostatic field. After a brief historical introduction the authors mention co-deposited Pd/D electrodes and external electrostatic field. These are essential components of the methodology they used to discover massive tracks in CR-39. I suppose that present setup does not differ drastically from the one used before.

The rectangular cell (2 by 2 by 8 cm) was made from acrylic -- 8 cm refers to the height. The cathode was gold while the anode was platinum. The electrolyte was 0.03M PdCl2 + 0.3M LiCl in D2. Each experiment was perform in three

stages, as specified below:

(a) Preparation: Current 1 mA for 24 hours. Then raising it to 3 mA and waiting till the electrolyte becomes transparent.

(b) Stabilization: Raising the current to about 40 mA for 2-3 hours.

(c) Exposure: Applying the constant difference of potential, 6000 V, to two parallel copper plates glued to outside walls. The anode and the cathode plates were parallel to the cell bottom. The current was raised to 100 mA and above. To explain this the authors wanted the system to be “in far-from-equilibrium condition.” After 48 hours the current was turned off and the cathode was examined. Exposure to external electric field was reported to be responsible for dramatic morphological and chemical changes on cathode surfaces. It is now easy to understand motivation for placing detectors of nuclear particles near the cathode in recent experiments.

It would be interesting to known if similar experiments were performed with the electrolyte based on ordinary water, rather than based on much more expensive heavy water. Note the Oriani effects were discovery with the electrolyte made from ordinary water. I am thinking for experiments to be performed by students and teachers.

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315) A new effect or contamination?

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This unit is based what was described in unit #314. Critics will suspect that what is attributed to a new nuclear effect might be due to presence of alpha radioactive substances in the electrolyte. San Diego team is fully aware of this and the issue has been addressed by them, as reported by Steven Krivit. The strongest argument they presented was the disappearance of emission when an external electric or magnetic field is applied to the cell. This topic is worth thinking and speculating about. It will certainly become an issue after the San Diego paper is published. How easy would it be to show, by using CR-39 chips, that the initial concentration of alpha radioactivity (pCi/L) was indeed too low to produce as many tracks as actually observed? I just realized that the method I was recommending (on the CMNS list) is impractical. In fact, I recommended two methods, as shown below:

"I hope that those who read unit #314 noticed that my numerical examples about alpha activities in the electrolyte were based on the exaggerated initial concentration (10 pCi/L) in the electrolyte. I hope the electrolyte will be shown to be practically non-radioactive. But this might call for keeping a CR-39 suspended in the liquid for two or three months. A faster way to eliminate suspicions (that codeposited radioactivity is responsible for the tracks) would be to run the electrolysis, without an external field, for several hours. Then to remove the cathode and expose it to CR-39. If the initial pCi/L is very small then the number of tracks on the chip should be essentially the same as on a chip, from the same stack, that was kept in a vacuum during the same time. Oriani would probably use a pre-counted CR-39 chip; in that way the answer would be more precise (smaller random error)."

The principle behind the first method is simple. Suspend the CR-39 chip in the liquid whose pCi/L is known and count the number of tracks per cm^2. That would establish a relation between the track density, D1, and the pCi/L. Then suspend another chip in the liquid X and measure the track density D2. If D2=D1, when the exposure times are identical, then both pCi/L are also the same. If D2=0.1*D1, for the same exposure time, then the pCi/L of X is ten times smaller than the known concentration in the standard liquid. The same conclusion would be reached if, in order to obtain D2=D1 the exposure time of the second chip had to be ten times longer. For a much smaller D2, the D2=D1 would be possible only after very long exposures. The numerical illustration below, also posted on the CMNS list, shows why this simple method becomes impractical when concentrations of alpha radioactivity become less than 10 pCi/L.

Let us see if 2 or 3 months was a good guess. I will assume that the CR-39 (area=1cm^2) has already been etched and pre-counted. One side showed 6 preexisting tracks and another showed 4 (due to exposure to cosmic rays during the long storage, etc.). I select the side which has only 4 tracks and expose it to water for 100 days. I will ignore the other side. Suppose that after 100 days in water I find 40 new tracks. What was the radioactivity concentration in pCi/L?

To answer this question approximately I will make the following assumptions:

a) Alpha radioactivity was due to an isotope whose half-life is much longer than 100 days. In other words, the activity is constant.
b) The range of alpha particles in water is 0.05 mm.
c) The layer of water from which alpha particles can reach the detector is 0.05 mm thick. (The rule of thumb is "as

many mg/cm^2 as MeV")
d) From all particles emitted in my layer only 10% produce tracks.

The assumption (d) can be justified as follows. 50% of particles are emitted away for CR-39. About 80 % of the remaining particles will be either absorbed in the 0.05 mm layer or reach the detector at too large angle of incidence. If these assumptions are justified then the following reasoning applies:

e) Since 40 tracks were found then 400 alpha particles were emitted.
f) What was the volume of my layer? It was 1(cm^2)*0.005 (cm)=0.005 cm^3 = 5*10^-6 liters.
g) Activity was 400 decays in 5*10^-6 L. This translates into 8*10^7 decays per liter in 100 days (or 8640000 s) or 9.2 decays per liter per second.
h) By definition, 1 Ci is 3.7*10^10 decays per second. Thus 9.2 translates into 250 pCi.

Conclusion: To produce 40 tracks in 100 days the activity would have to be 250 pCi/L. To detect 2.5 pCi/L, and to get 40 tracks, the CR-39 would have to stay in water for 100*100=10000 days, or about 27 years. . . .

It has already been shown (see above) that the concentration D2=10 pCi/L (for 222Ra, 238U, etc.) could cover the cathode surface with the amount of radioactivity emitting 32000 alpha particles in ten days. The result would be 8000 if D2 were 2.5 pCi/L. Would this be considered negligible in comparison with the number of alpha particles attributed to the San Diego effect? I cannot answer this question because I do not know how to estimate the total number of alpha particles attributed to that effect. It is desirable to have D2<0.25 pCi/L but that would call for exposures longer then 270 years.

And what about my “faster method” suggested above? Only an electrochemist can tell if the basic assumption of that method is realistic? The assumption is that atoms, such as Ra, U, in water are ionized. I think they are. That is why I expected them to migrate to the cathode surface rapidly. Several hours is reasonably short in comparison with ten days. This suggests a way to make the first method practical, even when initial concentrations of alpha-radioactivity are very low. I mention this in
a reply to a message posted on our restricted list this morning.

On Nov 23, 2006, at 5:06 AM, Michel Jullian wrote:

1) "SPAWAR effect" sounds fine, I can't understand why Ludwik dislikes it.

I will stop using the Frapa-Stanila label, at least for a while and see how others refer to the new effect.

2) Ludwik don't you agree any more that Pam's observation of "no external field --> no tracks" rules out the codeposited radioactivity artifact?

I think that the absence of tracks, when the external EM field is removed, is a very powerful argument against alpha radioactive contaminants. But I would very much like to see another, equally powerful argument.

3) It occurred to me that all imaginable artifacts associated with direct contact between the cathode wires and the CR39 material (thermal effects discussed in Steve's Galileo project FAQ, my mechanical puncturing hypothesis..) could be ruled out by interposing a thin layer of waterproof and ultra low density material such as expanded polystyrene: this would be transparent to alphas wouldn't it? (low g/cm3)

I think it is a good idea for arguing that mechanical effect are not responsible for observed tracks. But I saw too many CR-39 chips to worry about this. On the other hand, reporting that such check was actually performed, and showing real numbers, would help to establish credibility.

4) I convinced myself last night, by playing with numbers, that measuring specific alpha-activity in the electrolyte, at the level above 100 pCi/L, is feasible, provided one is willing to wait at least a month or two. Specific alpha-activities of 1 pCi/L, or less, cannot be measured in the same way. But then I realized that there is a way around. Suppose we

place an open 10 L container with water to be tested, into an oven where the temperature is 97 C. How long would it take to reduce the volume of water to 0.1 cm^3? (I am trying to avoid the "escaping droplets" ambiguity we discussed in a different context.) This would increase the initial concentration, for example, from 0.1 pCi/L (that we hope for) to 1000 pCi/L. Measuring 1000 pCi/L is not at all difficult.

Again, I would like to know what chemists think about such evaporative leveraging. I already asked this question here, several days ago, but no one replied. Is it reasonable to assume that most radioactive atoms will remain in water? I believe so, especially when there is no boiling. In any case, reporting that the initial specific alpha-activity was found to be less than 0.1 pCi/L would also add credibility to the discovery of a new nuclear effect. What else can be done, to promote credibility?

5) Suppose we wait till all water is evaporated. Then nearly all (?) radioactive atoms (responsible for 10*1000=10000 pCi) will be in a very thin layer. The 10000 pCi refers to 370 alpha particles per second (or 1330000 per hour). Suppose the area of the source is 100 cm^2. Put a 1 cm^2 chip (or, more preferably, a Si detector) on top of the remaining layer. How many tracks will be formed in each hour? The source activity would be 13300 alphas per hour and the counting rate would be ~13300/10=1330 per hour. The factor of 10 is probably exaggerated, to account for particles that are not intercepted by a 1 cm^2 detector. But it is prudent to be pessimistic in rough estimations.

The situation can be improved considerably. It is silly to throw away 99% of the source area (by using only 1 cm^2). Radioactive atoms, initially distributed over the 100 cm^2 bottom, can probably be forced to move to a much smaller area, for example, 1 cm^2. I am thinking about dissolving the deposits first and then evaporating water in a tiny beaker. That would increase the counting rate to more than 1330*100=133000 per hour. Yes, I know that it is much easier to speculate than to perform experiments. But I do have some experience with such things; and I am trying to be useful.

I hope the reader of this unit would not object if I make a little digression. I want to tell a real story; it should explain why I am so much preoccupied with the issue of contamination. I was working on my doctoral project in Orsay, France. The topic was “Fission induced by protons of 156 MeV.” One day, it was probably in 1961, I received a phone call from our librarian, Mme Vergne. She said she had an American visitor who keeps telling her things she does not understand. It is something about fission. She had other things to do and she wanted me to take Bob Walker, a researcher from General Electric Laboratory, from her. That is how I met the coauthor of the famous book about solid state track detectors. He wanted to show us how tracks, due to fission fragments, could be detected in mica.

Mica was used in the same way in which CR-39 chips are used today, except that it had to be etched in hydrofluoric acid. The CR-39 chips are very useful when one wants to count rare alpha particles in presence of abundant beta and gamma rays. In the same way, mica is an ideal detector when one wants to eliminate the effect of alpha particles and protons. I quickly learned the technique and it was later put to good use. Those who might be interested in details can refer to "Fission and Complete-fusion Probabilities as a Function of Angular Momentum ...", in Physical Review C, 1974, 10, 200. Examining pieces of natural mica, found in different places, I was fascinated by tracks they always display. Tracks in mica are due to spontaneous fission of isotopes of uranium, thorium, etc. Sometimes rivers of tracks could be seen under the microscope, showing how water, containing fissionable isotopes was sipping through the tiny cracks in the crystalline mineral. It was fascinating to think that some of latent tracks were formed hundreds of million years ago, waiting to be etched and observed. I know that water always carry elements like U and Th, in trace concentrations. And I would not be surprized to learn that Pu, and other man-made elements, are also present. OK, that is the end of my personal story.

P.S.

Replying to my request for permission to quote, Michel wrote: “my suggestion was meant to rule out ‘all imaginable artifacts associated with direct contact’, not just mechanical, it could be thermal or electrical or whatever one could come up with.” Hmm, all imaginable? OK, I know what he had in mind. Yes, as many reasonable objections as possible, should be anticipated and addressed. Contamination with alpha-radioactive substances is likely to be one of them. Convincing critical thinkers that chemical processes can occasionally trigger nuclear processes will not be easy, even after the San Diego effect is recognized as universally replicable. Theoretical considerations, based on reproducible data, will become essential ammunition at the next stage.

Appended on 11/27/06

In a message posted on 11/27/06 one CMNS researcher suggested a cell whose bottom would be a very thin metallic foil -- sufficienntly thin to pass alpha particles. That would be a cathode. A detector of particles would be placed below the cathode, rather than in the electrolyte. I think that a foil transparent to alpha particles would not be strong enough to support a column of the electrolyte. That was the essence of my reply. But then I added: “About one year ago, in Oriani's lab, we were performing experiments with a glass cell that had no glass at the bottom, only a Ni foil. That was our cathode; the Pt anode was suspended above the cathode. But the Ni foil was much thicker than the range of alpha particles. The CR-39 chips were placed below the cathode, during the electrolysis. The most surprising was that the number of tracks, accumulated in two two or three days, were larger that the background. Contamination of electrolyte with alpha radioactivity, if any, could not be blamed for the effect. I do not know what was the final verdict; the results have not been published by Oriani.

I tried to replicate his results at home but no definite conclusion was reached. I did observe the effect on several occasions but results were not reproducible. I started to suspect that static charges on CR-39 surfaces (that could possibly be created by removing the protecting plastic) attracted positive alpha-radioactive ions. Such ions are always present in air. I already mentioned that, in most basements, dust removed from a TV screen is alpha radioactive. This can be checked by using a Geiger counter with a sufficiently thin window. Leave the TV on for a week or two and then remove as much dust as you can with a small piece of wet paper towel. This of course does not imply that my results were affected by static charges as large as those on TV screens. But it is better to be prudent; it easy to remove cahrges by squeezing the unwrapped CR-39 chip beween wet fingers.

I shared my suspicion with Oriani and he said that simple precautions will be undertaken to be sure that CR-39 surfaces do not remain charged. Did this eliminate the effect? I do not know. Most recently Oriani was studying cathodes after the electrolysis. CR-39 chips were applied to surfaces that were wet during the electrolysis, as described here in one of his recent messages. Oriani’s results are shown in unit #314 at my CMNS website.”

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316) New nuclear processes

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This unit is inspired by a set of three messages, from the restricted Internet list for CMNS researchers. I think that they are important. Referring to the second FAQ (see above), Brian Josephson, our only Nobel Laureate (so far? ;-) wrote:

"I wonder if that is the most appropriate answer. The usual objection to 'CF' is that LNER is impossible because of the Coulomb barrier. A2 is literally correct, but I would have thought that some mention should be made of the connection. In fact, A2 is puzzling. However, on doing my own puzzling I see that there is a way in which the results could happen without fusion, which is that in some strange way energy from the fields gets focussed on individual particles and accelerates them sufficiently to make tracks in the plastic. That would be interesting in itself. In any event, Q2 deserves a longer answer than A2. In fact, my suggested mechanism (which would seem to be the only way to get tracks without fusion) would *not* be a nuclear reaction as normally understood. Or am I missing something?"

My reply was: "The only change that can help, at this stage, would be to replace the last word "reaction" by "process." in my opinion it was wise not to say more. We want the discussion to be focused on experimental details assuring replications, not on possible interpretations. This will come naturally, as soon as reproducibility on-demand is recognized. ..... I think that the strategy for keeping low profile (in their first publication and in PR activities) is good. They should say something like this: 'we do not claim anything but the fact that something extraordinary was discovered. Please check the reproducibility and help us to make sense of experimental facts.' We do not want to see the repeat of mistakes as those that resulted in the 1989 tragedy."

In the next message Brian added: "Perhaps the thing to do is to get rid of the first two sentences of A2,so that there is no explicit denial of a connection, and then to go on with "all that the experimenters claim is.....[whatever]" and perhaps, as Ludwik suggests, replace reaction by process."

Mike McKubre replied:
"Brian makes a good point. First I am uneasy about any general statement about the claims of ALL cf experimenters ('they'). In 18 years I have heard the most astounding claims: mini black holes, trapped thermal neutrons, electrons clusters at solid densities, polyneutrons, etc. ALL of which would be capable of initiating or catalyzing fusion. NONE of which I can reject from first principles or experimental observation.

Second I (on behalf of my co-workers) do in fact claim evidence of fusion events and fusion products (4He, 3He, tritium, lattice heat at 24 MeV/4He). For years I refused to call the field "cold fusion" (all the while attending ICCF's). It was not until we had evidence well in hand of fusion products that I began openly to use the term cold fusion (without inverted coma's), and using the abbreviation cf.

What we don't claim, what nobody ever has claimed as far as I am aware (but I am sensitive to my first point above) is that the reaction is the same pairwise d-d or d-Li reactions that power the H bomb. It is precisely the relaxing of the 2 body boundary condition that makes what we do possible*. But it is still fusion at one limit of nuclear reaction. Other things seem to be happening as well. *Actually I would add relaxing the particle mindset as well - but I throw that out just to stir up the billiard ball boys."

How can a retired teacher miss an opportunity to elaborate and comment? Brian refers to “some strange way in which energy from the fields gets focussed on individual particles.” And Mike suggests relaxing "the particle mindset." To elaborate on this let me explain why I prefer to use the phrase "nuclear process" instead of "nuclear reaction." A nuclear reactions is always imagined as a collision of two atomic nuclei. The initial stage, when two particles approach each other, is called input channel. It nearly always involves two nuclei (particles); the probability of a multiple collision is extremely small. The number of reaction products (competing output channels) can be larger than two; it is no longer a matter of probability of being in the same place at the same time; it is a matter of available energy (and other conservation laws).

At low energies (less than ~30 MeV) the following model is in good agreement with a very large set of experimental data. The two atomic nuclei either bounce away from each other (scattering) or fuse, forming a compound nucleus. That system is highly unstable and it brakes into two pieces, usually after a very small fraction of a picosecond. One of these pieces might again break into two pieces, etc., till excitation available energy is exhausted (converted into kinetic energies and excitation energies of products, and subsequently into heat.) Suggesting that we should reject the "particle mindset," Mike was probably saying that the entrance channel might be very different from what is commonly associated with the phrase "nuclear reaction." Why should a different kind of entrance channel, perhaps involving billions of nuclei in a crystal, be excluded. Several people speculated about collective behavior of many nuclei in the exit channel (to explain quasi-absence of neutrons and tritium), but I do not recall seeing similar considerations for the input channel.

I do not have any specific model for the "collective input channel." A very general idea is that a chemical process of some kind, under favorable conditions, leads to occasional nuclear events. Each atom might contribute only a small fraction of its kinetic energy (0.025 eV at room temperature), to the emitted particle. All existing models will be discussed after truly reproducible-on-demand demos become available. The main point is that, unlike the phrase "nuclear reaction," the phrase "nuclear process" will free our minds from the traditional interpretation of the input channel. For that reason, I would prefer labels LENP instead of LENR, and CANP instead of CANR, where P would be for "process or processes."
P.S.
Replying to my request for permission to quote, Mike added; “I am happy to see your elaboration. I believe that we are cursed by the apparent simplicity and success of two body, point interaction, scattering / tunneling physics. It will not work for us. We need to expand our discussion and our vision.” The comment Brian made was “It is nice having it all together.” If collective behavior makes sense for the output of a process then it should also make sense for the input. But nothing significant will happen till experiments are recognized as truly reproducible.

Appended on 11/27/06

Here is one crazy model. Consider a parallel plates capacitor with a costant difference of potential 10 volts. The medium is vacuum. Suppose that a H+ ion is released near the anode. It travels toward the cathode gaining 10 eV of kinetic energy. The negative cathode gives it an electron and the ion turns into a neutral atom. That atom bounces elastically from the cathode and arrives to the anode. Its kinetic energy is still 10 eV. Here its electron is given to the anode and the atom turns into a positive ions again. It means it is again accelerated and its kinetic energy increases to 20 eV. Suppose the process is repeated one million times. Then the kinetic energy will become 10 MeV. That is more than enough to produce a nuclear reaction.

I have no idea how to turn this idea into a practical device. But I do not think that conservation laws could be used to show that such process is impossible. A cyclotron is an example of a device in which kilovolts are used to accelerate particles 10 MeV and above through a large number of small steps. The idea occurred to me as I was learning about electrochemical double-layers. Each layer is modeled as a capacitor acharge to make several volts. But the distance between the positive and negative sheets is very small, something like 10^-8 cm. Perhaps the secret of NAE is is double-layer acceleration in a double-layer. Yes, I know that this is not even “physics for poets.” One has to learn more about double layers to produce something serious.

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317) About CR-39 detectors

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Introduction
Unit #314 was devoted to exciting developments in San Diego. The name I invented for the new effect, based on first names of SPAWAR researchers did not catch on; most researchers, on the CMNS list continue using the SPAWAR identifier, as Steven Krivit did in reporting the discovery on 11/10/06. In issue #19 of New Energy Times, at

http://newenergytimes.com/news/2006/NET19.htm

, referring to a recent meeting in Washington D.C., he reported: "The chips that the SPAWAR Systems Center scientists had brought to Washington were slices of CR-39 plastic, a common, transparent polymer that resists fogging and abrasions and is used to make eyeglass lenses, among other things. The researchers had placed the small pieces of plastic inside several of their electrochemical LENR test cells to capture and preserve any fleeting evidence of nuclear events.

"We heard about the use of CR-39 detectors from other LENR researchers at the 11th International Conference on Condensed Matter Nuclear Science in Marseilles, , in 2004," Mosier-Boss said.

She and her colleagues later learned that these same simple detectors have long been used by researchers in inertial confinement fusion (a form of hot fusion) and other areas of nuclear science to record the passage of neutrons, protons, and alpha particles (the two-proton nuclei of helium atoms stripped of their electrons). The traveling particles' charges shatter the bonds linking the plastic's polymers, leaving pits or "tracks" in the plastic.

After a CR-39 detector is exposed to a source of nuclear emissions, the detector is bathed in a sodium hydroxide solution, typically for six or seven hours, at a temperature between 65 and 73 degrees C. .....The bath scours away the collision's debris, and the resulting tracks are visible with a microscope or, if they're present in sufficient densities, with the unaided eye."

That is a good general introduction. Structural damage is created along the paths of nuclear a particle in certain solids. Called latent track, it is like a tunnel that is too narrow to be seen through an ordinary microscope, even at the highest magnification. Keeping the chips in a hot sodium hydroxide solution is called etching. The etching solution penetrates the tunnel and enlarges it. After that a track can be seen under the common microscope. Quoting Frank Gordon, Steven writes: " 'CR-39 detectors are ideal for detecting particles in LENR experiments because we can put them right inside the cell where the placement of electronics would otherwise be highly impractical, ... You don't need complicated instrumentation like you do with calorimetry or tritium analysis, ...It's an easy detection tool that's very straightforward.' "

That is certainly true. How were CR-39 detectors used in SPAWAR experiments? This question is answered in two pictures; they were drawn by P. Boss and S. Krivit. The cathode is a gold wire, presumably coated with codeposited metals (Pd and Li) and loaded with deuterium. The wire is simply wrapped around the CR-39 chip. This method of recording, if I understand it correctly, has one serious limitation. If alpha particles are emitted from the cathode then

only a very small fraction of them would be seen; most of them would be stopped in the electrolyte before reaching the detector. If particles were observed on the entire surface of the CR-39 detector (Krivit's report does not say that this was the case) then the unavoidable conclusion would be that they are emitted from the electrolyte.

Let me assume that tracks are observed only where the wire and the detector are in contact. In a situation like this I would use a different approach. I would evaporate a thin layer of gold on the CR-29; it would still be transparent to track-forming particles. The layer of gold would be my cathode. After the experiment I would remove the layer, etched the CR-39 chip and looked for tracks. That would be a detector inside the cathode. About 25% of all particles would be detected as tracks. An ideal detector would record 50% of emitted particles, i.e. all particles emitted into its direction. A real CR-39 detector, on the other hand, does not display tracks of particle that are intercepted at grazing angles.

In future experiments, if the SPAWAR claim is confirmed by using CR-39 detectors, people will probably use the idea of the "detector inside the cathode" for electronic detectors. I am thinning about a box with a thin wall containing a silicon detector. The advantages of the silicon detector are obvious, they should allow us to measure the energy spectra of particle, and to determine their identity. Protons, for example, could be distinguished from alpha particles by using foils of appropriate thickness. Furthermore, a silicon detector could provide information on the rate of emission, and on its dependence on various parameters, such as time, electric field, magnetic field, etc. But all this will be possible if results are reproducible, as in other areas of science. The mechanism of the process (or processes) by which a nuclear activity results from a chemical activity will probably be identified in less than a year, once the importance of that topic is recognized agencies supporting research.

Below I will show messages (or extracts from messages) about CR-39 detection that I posted on the restricted Internet list for CMNS researchers since the SPAWAR discovery was first presented at the Navy Science and Technology conference in Washington, D.C. on August 2. I really believe that something very significant is going to happen in the controversial CMNS field very soon; may be as soon as two or three months.

Message 1 (posted on 11/14/06)

Last year i had a chance of examining the CR-39 that had a very low background, typically, 2 to 5 tracks per cm^2, The thickness was 2 mm, which is very convenient for microscopic examination. You focus on the side facing you and what id on the other side is not visible at all. People with more experience, especially Lipson, Roussetski and Orin, will probably have something to say about this. I do not recall the Japanese vender; ask Jiro Kasagi <kasagi@lns.tohoku.ac.jp>. Perhaps he will post this information, and the current price on the list.

To count tracks I recommend the magnification 40 (x4 objective and x10 eyepiece). To examine individual tracks magnification 100 will probably be sufficient.
(x10 objective with the same eyepiece). In principle, any traditional microscope should be OK. But I prefer a microscope equipped with a digital camera connected to a computer. That is how Orin works. This offers several obvious advantages. One of them is that counting tracks on a printed picture s more accurate, especially when track density is high (you circle tracks after they are counted, to avoid double-counting). For extremely low counting rates magnification 20 would probably be more convenient -- each picture covers four times larger area.

Message 2 (posted on 11/14/06)
Here is how I just replied to a private message. The reply might be useful to other beginners; that is why I am posting it here. Diameters 10 to 15 microns are good enough for easy counting of tracks. It is often frustrating to wait much longer. Typical track diameters depend on the etching time, as shown below

1) Etching at 65 C
==========
3 hrs 5 microns
5 hrs 10 microns
10 hrs 22 microns
15 hrs 35 microns The CR-39 chip become translucent (foggy), but that does not interfere with counting large tracks

2) Etching at 70 C
==========
3 hrs15 microns
5 hrs 25 microns
10 hrs 37 microns The CR-39 chip become translucent (foggy), but that does not interfere with counting large tracks
15 hrs 45 microns Even more foggy, but tracks are clearly identifiable. Did someone looked at them through a good magnifying glass? I didn't.

I suspect that some tracks might be etched away when etching is too long. Keep in mind that my measurements of diameters were not very accurate. But that was good enough for my purpose.

Message 3 (posted on 11/14/06)
In a private message someone pointed to a paper of Frenje et al. in the Review of Scientific Instruments -- July 2002 -- Volume 73, Issue 7, pp. 2597-2605. To get the article go to Google and type this into the search box:

CR-39 Frenje "Absolute measurements of neutron yields from DD"

A short description of the track formation process in CR-39 (in section IIA) is probably sufficient for the intelligent use of detectors. For a much more detailed description see the long paper of Nikezic and Yu, to which George Miley referred two days ago. The main purpose of the paper, by Frenje et al., is to describe how CR-39 are used to detect neutrons. As mentioned by Krivit, CR-39 are widely used to detect neutrons, when yields are large, in the range of 10^6 to 10^13. What is the difference between detection of alpha particles (or protons) and detection of neutrons? Any alpha particle intercepted by the CR-39, will create a track, provided the incidence angle is not too large. For neutrons, on the other hand, the probability of forming a track is usually much smaller than unity. To get the general 2002 review of track detectors by Nikozic and Yu, go to Google and type this into the search box:

Nikozic "Formation and growth of tracks"

Message 4 (posted on 11/15/06)
. . . Consider a 222Rn atom in air; it emits an alpha particle and the recoiling nucleus, 218Po, becomes a positive ion. Yes, outer electrons are often lost when an atom is pushed suddenly. The 218Po ion, or an ion of 214Pb, or an ion of 214Bi, etc. slowly drifts toward the electrically-charged screen. That is why dust collected from a TV screen (on a tissue) contains radioactive atoms. This can easily be checked with a simple Geiger counter. About ten years ago I convinced myself that the measured decrease in activity was consistent with the above explanation. The motivation for the experiment was a message posted on a discussion list for physics teachers. The author advised us to use TV screens as extremely inexpensive radioactive sources for classroom demonstrations.

But this is not a list for teachers. Why do I think that users of CR-39 should be aware of what I write here? Because it is very easy to charge a CR-39 chip electrostatically, most likely when the protective plastic is peeled off. An electrically charged CR-39 chip, if left in air for a long time, will have more tracks than a similar chip left in distilled water, for the same time. I checked this last year because I was aware of the TV screen effect. Fortunately static charges can easily be removed, for example by touching CR-39 surfaces with wet finger. Keep this in mind while looking for CMNS particles.

Message 5 (posted on 11/16/06)
In New Energy Times Steven also reported: "During the plating process, the cathode is in contact with a CR-39 detector in the cell to which the scientists had applied an external electric or magnetic field. After the experiments had completed their runs of eight to 11 days, Mosier-Boss and Szpak saw dense, cloudy areas on the portions of the detector near the cathode.

'The fact that the cloudy areas are observed where the detector was in close proximity to the cathode suggests that the cathode caused the cloudiness,' Mosier-Boss said. As a control, Mosier-Boss also exposed CR-39 detectors to electrolysis in a lithium solution without palladium in it. The result: only a sprinkling of tracks, randomly distributed

and so few in number that they could be accounted for by background radiation. She also immersed the detectors in the usual solution of palladium chloride and lithium chloride in deuterium but without applying the external electric current. The outcome was the same: no unusual shower of tracks from high-energy particles."

An easy way to produce a cloudy area on a CR-39 is to expose it to a source of alpha particles. A small 241Am source I used was form an old radiation-type smock detector (a new one costs about $10, for example, in Home Depot or Radio Shack). Exposing it to CR-39 -- with about 5 mm of air between the surfaces -- produced many well separated tracks. A ten times longer exposure produced proportionally more tracks, with frequent overlapping. Much longer exposures produced cloudy surfaces (after etching) visible to naked eye. I do not recall seeing cloudiness before etching. But it would not surprise me to see it after much longer exposures.

My suggestion is to expose a CR-39 chip to an alpha source for an hour or two. Would it become cloudy? Would it resemble the chip removed from the co-deposition cell after 11 days? Perhaps cloudiness, after 11 days of exposure, was an indication of many "giant showers" mentioned by John Fisher. But such test should be performed far away from the room in which the electrolysis experiments are performed. (One advantage of a smock detector source, over a piece of depleted uranium, [or over a welding electrode containing thorium], is that Americium is most likely coated by a very thin layer of something, to prevent contamination of air etc.) By the way, 241Am is also a source of 59 keV X-rays.

Message 6 (posted on 11/19/06)
. . . My experience was that only rarely (up to several % of cases -- when 241Am tracks were examined) did mechanical defects look like nuclear tracks. Most cases can be definitely resolved by refocusing in depth. When I worked with Orin, one year ago, he suggested uncertain tracks be ignored. In particular we ignored cases in which more than four (often more that ten) adjacent pits were on straight segments. We believed that these track-looking defects were due to mechanical scratches. I do not know how discrimination between tracks and non-tracks is handled by counting software. But visual examination is highly reliable, after some experience, when one sees more that ten randomly distributed tracks, (or clusters of tracks) per view.

By the way, Oriani's CR-39 chips were often precounted. Such time-consuming approach is justified only when background track are nearly as frequent as tracks attributed to CMNS reactions. That seems not be the case in the PamStan effect. (Precounting involves two etchings, First a chip is etched before being used in an experiment and background tracks are counted. Then it is etched again, after the experiment. Suppose the difference is 25-7=18. Then no statistical error is associated with 7. Measuring background with another chip is not as reliable as measuring it with the same chip. One can go one step further -- old tracks can be recognized by their relative locations, and by larger diameters. Fortunately, all this is not necessary when background corrections are small.)

Message 7 (posted on 11/27/06)
. . . I started to suspect that static charges on CR-39 surfaces (that could possibly be created by removing the protecting plastic) attracted positive alpha-radioactive ions. Such ions are always present in air. I already mentioned that, in most basements, dust removed from a TV screen is alpha radioactive. This can be checked by using a Geiger counter with a sufficiently thin window. Leave the TV on for a week or two and then remove as much dust as you can with a small piece of wet paper towel. I shared my suspicion with Orin and he said he will make sure charges are removed from CR-39 surfaces. Did this eliminate the effect? I do not know. Most recently Orin was studying cathodes after the electrolysis. CR-39 chips were applied to surfaces that were wet during the electrolysis, as described here in one of his recent messages. His results are shown in unit #314 at my CMNS website.

Message 8 (posted on 11/30/06)

On Nov 30, 2006, at 12:58 PM, Scott Little wrote:
I"n the recent "Extraordinary Evidence" article in New Energy Times

http://www.newenergytimes.com/news/2006/NET19.htm#ee

There is a photo entitled, "Ag wire/Pd/D in Magnetic Field" with a caption below the photo that reads, "Dr. Gary Phillips: "They show a number of double tracks which you would see from a reaction that emits two particles of similar mass and energy."

The photo does show an abnormal incidence of double tracks...i.e. more than you'd expect from simple coincident landings of individual particles. But my limited understanding of nuclear physics suggests that two-particle reactions would normally send the two particles off in opposite directions. ..to conserve momentum.

Is there such a thing as a nuclear reaction that emits two particles in the
same direction?"

That could be a troublesome signature. I am thinking about tiny grains of uranium or radium codeposited on the electrode. The best way to check for this would be run the electrolysis for much longer than ten days. Suppose the percentage of doubles increases significantly, or triples start to appear. In that case one would have a proof of contamination. It did not occur for me that this could be the second independent test showing that contamination is not responsible for observed tracks. Steven already reported the first convincing test -- track formation stops when the external field is turned off. But, having a second test would be desirable, in the present situation.
P.S.
On the other hand, one can say doubles and triples do not necessarily prove that contamination is present; they prove that NAE is not uniformly distributed over the surface

Message 9 (posted on 11/30/06)
On Nov 30, 2006, at 6:22 PM, Scott Little wrote:

I'm wondering what to expect from CR-39 as an alpha detector. Will every alpha that hits normal* to the surface of the detector make a track?

Yes, and this track will be round. There is a critical incidence angle above which tracks would be so shallow that etching would destroy them. Roussetski and Lipson reported it. Perhaps one of them will reply to this message. My rule of thumb is that a CR-39 chip, located on top of cathode, detects about 25% of all particles as tracks. But that is only a quick estimate.

If not, what is the mechanism for an alpha striking normal to the surface and not making a track? Backscattering from near-surface nuclei seems possible in principle
but wouldn't the cross-section for that be very small?

I expect it to be extremely small. Rutherford scattering formula is highly reliable at at low energies. My guess that no more than one out billion alpha particles will be scattered into the backward hemisphere. Perhaps someone will theoretically confirm this intuitive guess

Is there any other mechanism besides backscattering that will cause an alpha particle NOT to make a track in CR-39?
*Low angle incidence alphas will certainly make nascent tracks but the track will be completely etched off during the etching process so you see nothing.

P.S.
Assuming that NAE is distributed uniformly over the 1 cm^2 of the cathode (below a CR-39 chip, also 1 cm^2), and assuming that N tracks are recorded, what is the probability of finding two tracks very close to each other? The answer depends on two factors: the total number of tracks, N, and the definition of the "very close." I expect the issue raised by Scott to be noticed by referees. That is why it should be
addressed. Here is my rather trivial contribution. It is based on a Monte Carlo program I wrote today. I am assuming that a given number of

tracks, for example, N=10000, are recorded at randomly selected points on the 1 cm^2 of the CR-39. That fixes the track density. By changing N, I can simulate any density I want. The program compares location of each point with locations of all other points. If the distance between two points turns out to be smaller than d then my counter of coincidences is incremented by one. Any value of d can be imposed. As expected, for a chosen track density, the percentage of coincidences increases with d.

Diameters of tracks are probably 10 microns. With this in mind, my first choice was d=20 microns, for N=1000. Results collected during the debugging are shown below. I will run the program for N=1000, for d=20, several more times. This will give me the mean percentage and the standard deviation.

N=1000 tracks, d=20 microns; Ncoinc=5 (this is only 0.5%)
N=1000 tracks, d=50 microns; Ncoinc=35
N=1000 tracks, d=80 microns; Ncoinc=104
N=2000 tracks, d=80 microns; Ncoinc=446

For very large N every track will have a close neighbor.
P.S.
In running the first case 30 times, the mean turned out to be 6.5 coincidences; standard deviation 2.0 coincidences. Each run, for N=1000, takes only about 30 seconds on my old Mac. I would be happy to run the program with other N and d, if needed. the listing of the program is shown below; the code is in True Basic, but it should easily be translated into another computer language. Exclamation sign are beginnings of comments (up to the end of the line). I am dividing by 2, in the last print statement because each coincidence was counted twice in my primitive code.

= =
```
program TRACKS
! 11/30/06 dim xx(100000), yy(100000) ! random locations (maximum 100000)
randomize
let N=1000
let dmin=0.002 ! in cm (10 microns is 0.001 cm)
let dmin2=dmin^2 ! to avoid slow sqr() calculations
for i=1 to N
let xx(i)=rnd
let yy(i)=rnd
next i
let cnt=0 ! counter of coincidences
for i=1 to N ! compare with all previous points
for k=1 to N
let d2=(xx(i)-xx(k))^2 + (yy(i)-yy(k))^2 ! avoiding the sqr
if d2<dmin2 and (k<i or k>i) then let cnt=cnt+1
end  if
next k
next i
print "coinc=";cnt/2;" out of N=";N;". dmin=";dmin; "cm"
END
```
= =

Message 10 (posted on 12/1/06)
Scott Little wrote asked: “I'm wondering what to expect from CR-39 as an alpha detector. Will every alpha that hits normal* to the surface of the detector make a track?” I can now be more specific about the CR-39 efficiency.

According to a long review paper of Nikezic and Yu [Materials Science and Engineering R 46 (2004) 51–123] the critical angle depends on the energy of alpha particles. It decreases from about 60 degrees for the 6 MeV alpha particles to 45 degrees for the 10 MeV particles. This refers to etching for 6 hrs. Also note that the angles are between the particle's trajectory and the CR-39 surface.

How does the 60 degrees compare with my 25% rule of thumb? Their 60 degrees translates into the incidence angle of 30 degrees (with respect to the normal, as in optics). The solid angle corresponding to an incidence angle A is 2*Pi*(1-cosA). It is always a fraction of the total solid angle, 4*Pi. For A=45 the fraction is close to 0.15, or 15%. I expect this fraction to double when etching time is reduced to 3 hours. Note that all tracks would disappear for really very long etching. That would be 0% efficiency (a lot of nuclear particles but no tracks at all). The efficiency for protons should be much better than for alpha particles. Why? Because their latent tracks are longer than those of alpha particles of the same energy. My guess is that the efficiency, for 10 MeV protons, will be nearly ideal. i.e. 50%. Why am I guessing? Because I trust my intuition based on knowledge of ranges of charged particles in matter. The actual answer, after spending many hours, would nearly certainly be higher than 40%. To be more accurate I would have to perform calculations based on rates of etching, etc. Or I would have to look for references in published papers. This is also a lot of work, in such cases.

I recommend etching for 3 hrs. It has three advantages: (a) efficiency is higher, (b) tracks are smaller (but still clearly visible and recognizable), and (c) time is saved. Note that two nearby tracks might overlap after 6 hrs of etching but not after 3 hrs of etching. Orin drills tiny holes in CR-39 chips and suspends the chips into the etching solution, with nickel wires. My approach was different; I just dropped the chip into the etching solution which was constantly stirred with a rotating magnet. Stirring is important when the beaker is sitting on a thermostat-controlled hot plate. Small chips are never at the bottom. Without stirring chips would stay at the bottom and their temperature could be unacceptably high (overetching).

Low magnification, such as 4*10 (objective 4 and the rest 10) is desirable when tracks are rare. In fact, I would prefer 2*10 when densities are below ~100 tracks per square centimeter. But low magnifications make visual counting impossible when track densities are very high, which, I suspect, was the case in SPAWAR experiments. At the extreme, when tracks are too numerous, with frequent overlapping (for example, when track densities exceed ~100000/cm^2, instead of ~10/cm^2 background) one might use traditional optical densitometry. It would be simply a matter of comparing chips exposed to NAE with chips exposed to known numbers of aloha particles from a calibrated source. Let us hope that future experimental data will force us to abandon counting of tracks in favor of densitometry. ;-)
P.S.
The 2004 review of solid track detectors, by Nikezic and Yu, that was mentioned above can be downloaded from my website as:

http://blake.montclair.edu/~kowalskil/cf/cr39nikezic.pdf

Message 11 (posted on 12/2/06)
On Dec 1, 2006, at 7:04 PM, Michel Jullian wrote:

. . . Thinking back to my proposal of letting the alphas escape through the cell bottom so they can be detected outside, I recall you doubted that a sufficiently alpha-transparent material could stand the ~5 cm water column. And then I remembered your own proposal of coating the CR-39 detector with a few microns of Ni (~9 g/cm3), wouldn't a 10 microns "plastic wrap" for household use (LDPE, 0.9g/cm3 i.e. 10 times less dense than Ni), which definitely could stand the water column, be just as alpha-transparent as a 1 micron Ni layer? Or isn't density the only factor to take into account?

That seems to be a good solution. I am assuming you are suggesting a thin metallic coating on the plastic wrap (on the wet side) to be the cathode. That is better than wires you first suggested. The path seems to be open to use a Si detector after SPAWAR results are confirmed by people involved in the Galileo project initiated by Krivit. When will we know about the results? It is a matter of following a protocol already tested by SPAWAR people. I do not expect any problems with CR-39 detectors. But everything should be tested and this takes time.

Let me show range-energy relations in several materials. That information would probably be useful to those who are planing to implement the idea. R is the range in mg/cm^2. Illustrations on how to use range-energy information will be shown below.

10 MeV alphas in gold: R=45.5
8 MeV alphas in gold: R=33.4
6 MeV alphas in gold: R=22.8
5 MeV alphas in gold: R=18.0
4 MeV alphas in gold: R=13.6
3 MeV alphas in gold: R=9.6
2 MeV alphas in gold: R=6.1
1 MeV alphas in gold: R=3.2

10 MeV alpha in nickel: R=22.6
8 MeV alphas in nickel: R=16.3
6 MeV alphas in nickel: R=10.9
5 MeV alphas in nickel: R=8.5
4 MeV alphas in nickel: R=6.4
3 MeV alphas in nickel: R=4.5
2 MeV alphas in nickel: R=2.9
1 MeV alphas in nickel: R=1.6

10 MeV alphas in aluminum: R=16.5
8 MeV alphas in aluminum: R=11.6
6 MeV alphas in aluminum: R=7.5
5 MeV alphas in aluminum: R=5.8
4 MeV alphas in aluminum: R=4.2
3 MeV alphas in aluminum: R=2.9
2 MeV alphas in aluminum: R=1.8
1 MeV alphas in aluminum: R=1.0

10 MeV alphas in carbon: R=12.4
8 MeV alphas in carbon: R=5.6
6 MeV alphas in carbon: R=5.4
5 MeV alphas in carbon: R=4.1
4 MeV alphas in carbon: R=2.9
3 MeV alphas in carbon: R=1.9
2 MeV alphas in carbon: R=1.2
1 MeV alphas in carbon: R=0.6

10 MeV alphas in hydrogen: R=4.19
8 MeV alphas in hydrogen: R=2.81
6 MeV alphas in hydrogen: R=1.70
5 MeV alphas in hydrogen: R=1.24
4 MeV alphas in hydrogen: R=0.85
3 MeV alphas in hydrogen: R=0.53
2 MeV alphas in hydrogen: R=0.28
1 MeV alphas in hydrogen: R=0.11

In planning for an experiment it would be sufficient to use carbon as a representative for a plastic material (unless very accurate calculations are needed)..

Plot the R=f(E) curve and use the smooth curve to solve a problem. Here are two illustrations. How much energy will an alpha particle of 6 MeV have after traversing the Al foil of 2.5 mg/cm^2. The range of 6 MeV alpha is 6.5 mg/cm^2; thus it will pass through the foil. Its energy will be reduced. Subtract 2.5 from 6.5. The result is 4 mg/cm^2. What energy does it correspond to on your plotted curve for aluminum? My curve shows it is close to 3.9 MeV. That is the is the energy of the the alpha particle after traversing the foil perpendicularly.

Another typical problem is to chose the foil thickness for a desired output energy. For example, I want the 6 MeV alpha particle to emerge with 3.5 MeV. How much nickel would I need? Use the R=f(E) curve for In. It shows that 3.5 MeV corresponds to R=5.3 mg/cm^2. What is the range of the 6 MeV alphas in In? It is 10.9 mg/cm^2. Thus the foil you need is 10.9-5.3 = 5.6 mg/cm^2. How to convert this mass per unit area into the thickness in cm? The density of In is 7.9 g/cm^3. Therefore 5.6 mg/cm^2 translates into 0.00071 cm; this is 7.1 microns.

Message 12 (posted on 12/2/06)
Responding to my request for permission to be quoted (that is a rule on the private CMNS list) and to what I added, Michel I wrote: "Indeed coating the cathode on the transparent window (by CVD?) might be better than loose wires, but keeping it in the shape of wires would be better than a uniform coating as it allows correlating track density with wire proximity as Pam noted"

Message 13 (posted on 12/2/06)
Changing from uniform coating (uniform wet cathode) from nonuniform coating (strips simulating wires) would be only one of many parameters to explore. For the time being I am assuming that the purpose of gold wires (inside the electrolyte) was to become NAE. In that case concentration of tracks near wires is natural. If I recall correctly, they also had a picture with silver wire. Let us wait for results that Steve Krivit will report at the termination of Galileo project. In that project the task is to replicate the SPAWAR protocol and to describe the results. That might lead to many interesting experiments, deviating from the working protocol, one parameter at a time or in different directions, according to expectations. Our theoreticians will finally have reliable experimental data for validating theories. It would not take too long before the mystery of NAE is solved. Will this end the the dark age of discrimination? Will CMNS will become normal science? Let us hope so.

I would appreciate if someone could send me ranges for alphas in one or two plastic materials, and for water. An electrolyte is mostly water.

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318) Theories, Meta-theories, etc.

Ludwik Kowalski; 12/4/2006
Department of Mathematical Sciences
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An interesting paper was attached to a message posted this morning on the restricted Internet list for CMNS researchers. The author believes that deuterium ions inside metals create surface cracks, typically 10 microns wide. That is not difficult to accept; photographs of cathodes that were used in CMNS experiments often show surface irregularities. What is difficult to accept is a claim that electric fields between the walls of cracks can be as high as 100 million volts per centimeter. The rationale for this claim was not explained. Was it a postulate -- something like "that would be sufficient to explain nuclear particles and X-rays "-- or was it a conclusion based on something that is known to be true? Acceleration of ions in strong local electric fields could indeed be responsible for emission of nuclear particles. If real, that would be a true "electrochemically induced nuclear effect."

If one million volts per centimeter of air (or H_2 or O_2 or water vapor) in a crack, is an ad hoc assumption then the idea belongs to the category of ideas mentioned recently by Mike McKubre. He referred to polyneutrons, whose existence was postulated by John Fisher, and to stable neutrons inside solids, postulated by Hideo Kozima. Such theories are worth developing, in anticipation of an era of reasonable reproducibility. Such logical structures should not be confused with structures that begin with what is known to be true. A theory that begins with an ad hoc assumption can be wrong on two accounts (assumption and logic); a theory that begins with what is already known can be wrong on one account only (logic).

The concept of NAE, introduced by Ed Storms, is not a theory, it is only a name given to what makes nuclear activities induced by chemical activities possible. The same is true for the idea of "poison" that was introduced by Peter Gluck this morning. He wrote: "The problem of reproducibility, more precisely the painful and disturbing lack of it, deserves a thorough discussion. And, it has to be the central and most urgent task of our thinking and experimental studies. . . . True, we have tens of theories that cannot be translated in practical instructions, lines of trying for the experimenters. . . . [My] explanation is that all the cathodes are inactivated by some poison or some other unknown factor, and the task is to discover or to annihilate this something, protecting the working surfaces, sites. This has happened naturally, by chance, in very few cases during CMNS's complex history.I am ready to discuss an experimental strategy based on this meta-theory. .....Therefore I consider unknown poisoning more logical than overwhelming complexity."

Peter was replying to a message in which failures to replicate were attributed to the overwhelming complexity of natural phenomena at the atomic level. Rather than dealing with many factor contributing to complexity he prefers to deal with one factor, an unknown poison. Like NAE, that poison is a mystery. I have a name for it; let us call it NIE. This stands for the Nuclear Inactive Environment. That is funny. NIE NAE covers the entire universe, by definition. And what does it mean in Polish? It means "NOT NAE." The term meta-theory probably means something to philosophers. Is it a theory of theories or is it a theory outside all other theories (like metaphysics is outside physics)? Go to Google and type -- what is metatheory -- into the search box: I got 225,000 hits.

After reading the above, Peter posted this message: "(a) I have been speaking and writing re poison as reproducibility killer in CMNS- for years. And I have told that the "usual suspects" are the well known impurities due to air pollution i.e. nitrogen oxides, H2S, SO2, CO, COS and VOC (volatile organic compounds)- plenty of them everywhere. Poisoning can be acute i.e fast as for the Pd cathodes or chronic - the most spectacular storyy being that of the Patterson system - the beads have lost their ability to generate excess heat- rather slowly. But once they were dead,

they remained so. Poisoning is, at least in part, reversible- laser irradiation can reactivate some sites on the cathode. Bacteria are are also plentiful; should they also be included in the list of poisons? Then Peter added: "(b) The best definition for a meta-theory is at < http://www.answers.com/topic/metatheory > " I clicked this link and the following definition popped up: metatheory is "a theory devised to analyze theoretical systems." That definition puts metatheories into the domain of philosophy. They are theories of theories, making sense of theories. They are not tools for making sense of experimental data.

Replying to Peter, Michel Jullian wrote: "Which makes me think, sorry if this has been discussed before, that the SPAWAR codeposition method could be used to bypass the cathode poisoning problem by regenerating the top cathode layer when needed: just pour some palladium salt into the electrolyte every time the effect seems to die, this will build a fresh palladium hydride layer on top of the dead one." That is an interesting observation. Peter's ad hok assumption about poisons will become testable, like other theories, after reproducible protocols are published. I do not think that he is offering a meta-theory. In replying to Michel he elaborated:

"The idea is good in principle, however unfortunately the liquid phase contains solved air, including the poisonous impurities. So the newly formed surfaces are poisoned at their birth, more or less (actually less, this is one of secrets of the superior results via codeposition. The same impurities are doing harm to chemical catalysts and it seems that our CMNS active sites are smaller and even more sensitive than the catalytic ones. In my 1992 paper -see lenr-canr.org I concluded that CF/CMNS is an extreme form of catalysis. Some polymerization processes are also very sensitive to impurities, e.g. 20 ppb (parts per billion sulfur) destroys a kind of acrylic polymer- you don't obtain a latex (milk like emulsion) but rather great beads. The solution of this problem is NOT to try to measure with great care and effort the impurity- a very difficult task; you add 0.5 grams copper salt to a 40 cubic meter vessel and this inactivates completely, kills the impurity. Probably in the case of CMNS a similar strategy could work:

1) the surface of the cathode has to be covered with a very thin, monomolecular layer of a substance permeable to hydrogen/deuterium but not to the impurities;

2) the liquid phase has to be cleaned, desaerated, some filters/chemical filters retaining them added to the cell .

You remember that Fleischmann and Pons have discovered that the boiling cells are working better than those at ambient temperature? Giving more excess heat? The complexity meta-theoiry does not explain this, but for the poisoning theory the effect is obvious- boiling cells contain less air, and less impurities, the cathodes are more active. Unfortunately, the poisoning metatheory is usually rejected ab ovo by our colleagues. I don't understand exactly why."

Experiments designed to test Peter's idea should be conducted after CMNS is transformed from protoscience to science. We are probably very close to this historic transformation now. Yes, I am assuming that the SPAWAR discovery is going to be confirmed in the TGP (the Galileo Project initiated by Steve Krivit)

The <http://yunus.hun.edu.tr/~saritan/cargo.htm> link was posted today on Phys-L, a list for physics teachers. It points to a piece written by Richard Faynman. In that piece, referring to a good theory, Faynman wrote:

"Details that could throw doubt on your interpretation must be given, if you know them. You must do the best you can - if you know anything at all wrong, or possibly wrong - to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it. There is also a more subtle problem. When you have put a lot of ideas together to make an elaborate theory, you want to make sure, when explaining what it fits, that those things it fits are not just the things that gave you the idea for the theory; but that the finished theory makes something else come out right, in addition.

In summary, the idea is to give all of the information to help others to judge the value of your contribution; not just the information that leads to judgment in one particular direction or another." I think it is good advice. A theory must predict at least one unknown fact in order to be testable. Addressing another issue Feynman wrote: "

"I would like to add something that's not essential to the science, but something I kind of believe, which is that you

should not fool the layman when you're talking as a scientist. I am not trying to tell you what to do about cheating on your wife, or fooling your girlfriend, or something like that, when you're not trying to be a scientist, but just trying to be an ordinary human being. We'll leave those problems up to you and your rabbi. I'm talking about a specific, extra type of integrity that is not lying, but bending over backwards to show how you're maybe wrong, that you ought to have when acting as a scientist. And this is our responsibility as scientists, certainly to other scientists, and I think to laymen.

For example, I was a little surprised when I was talking to a friend who was going to go on the radio. He does work on cosmology and astronomy, and he wondered how he would explain what the applications of his work were. "Well," I said, "there aren't any." He said, "Yes, but then we won't get support for more research of this kind." I think that's kind of dishonest. If you're representing yourself as a scientist, then you should explain to the layman what you're doing - and if they don't support you under those circumstances, then that's their decision. "

And here is another wise observation, from the same source: "I was shocked to hear of an experiment being done at the big accelerator at the National Accelerator Laboratory, where a person used deuterium. In order to compare his heavy hydrogen results to what might happen with light hydrogen, he had to use data from someone else's experiment on light hydrogen, which was done on different apparatus. When asked why, he said it was because he couldn't get time on the program (because there's so little time and it's such expensive apparatus) to do the experiment with light hydrogen on this apparatus because there wouldn't be any new result. And so the men in charge of programs at NAL are so anxious for new results, in order to get more money to keep the thing going for public relations purposes, they are destroying - possibly - the value of the experiments themselves, which is the whole purpose of the thing. "

Appended on 2/3/07:

An interesting, but not recent, review of the cold fusion situation has been "advertised " by one of the subscribers to an Internet list for CMNS researchers. The title of the 1999 article was: "Whatever happened to cold fusion,. It was published in PhysicsWeb.

http://physicsweb.org/articles/world/12/3/8

The occasion was the 10th anniversary of Cold Fusion. The author, David Voss wrote that the main motivation was to generate unlimited amounts of energy. Some people, like Patterson and Case, did expect material rewards but I do not think that this applies to most CF researchers. The field is still very far away from practical applications; the main task is to convince ourselves that a chemical process can trigger a new nuclear process. Those who stubbornly continue to conduct CMNS experiments are most likely motivated by desire to discover, or to confirm, new phenomena. Applications and theories will develop naturally after scientific claims, such as transmutation of isotopes and excess heat, are finally recognized as valid. The person who dug the old article asked: "a number of criticisms are made of CF expts. How valid are they?" Here is how this was answered by an active CMNS researcher, Scott Little. First he quoted this paragraph:

"Sporadic reports have continued to trickle in from various small research efforts, but in each case the results have proved erratic or impossible for other groups to replicate. It appeared to be a classic case of what the Nobel chemist Irving Langmuir called "pathological science", in which the results are always near the limit of detectability and the proponents always have an ad hoc answer as to why."

Then he added: "In my experience and area of study, particularly for excess heat results, this is largely true. One would reasonably expect to see some increases in the amount of excess heat obtained from cold fusion experiments as the years go by. I believe the opposite is the case. People keep building better and better calorimeters and, in general, the magnitude of their excess heat results keep shrinking.

As to the ad hoc explanations, isn't that just what you would expect in a field where nobody really understands how the "reaction" works in the first place? Another criticism, not mentioned in the article, which I would level at CF expts is

their tendency to believe that any anomalous "positive" result in their experiments is a sign of nuclear reactions. For example, if a complex calorimetry experiment is underway and the instrument shows a small excess heat of, say, 50 milliwatts (out of 15 watts total input), I know that some CF expts would say something like, "Ahhh. now we're getting some excess heat". In my opinion, the reaction should be more like, "Hmmmm. ...50 milliwatts is only 0.33% relative of my input power. Is my calorimeter that precise? This could easily be the result of a systematic error... how can I check for that?"

The 20th anniversary of the 1989 announcement of Cold Fusion is not very far away. Will the field be recognized as truly scientific by the majority of mainstream scientists, and by scientific establishment? I think I will be in a much better position to answer this question by the end of this year. The ongoing investigations of the SPEWER discovery (emission of nuclear particles when an electrolytic cell is placed into a magnetic field) has been described in The New Energy Times. The answer will depend on the outcome of The Galileo Project, directed by Steven Krivit. Look for the upcoming issues of The New Energy Times, published every two months. That online magazine is already an important source of information about CMNS research.

The person who dug out the Voss' article wrote that transmutation experiments seem to be less vulnerable to criticism than the excess heat data. What follows is the reply from Scott Little.

"At Earthtech we have expended some effort on the transmutation claims. We were one of the few participants in the RIFEX experiment promoted several years ago by CETI (Patterson et al). You should read the report(s) I wrote about this work to get some idea of the difficulties in confirming transmutation claims.

http://www.earthtech.org/experiments/rifex/rifex.pdf

Executive summary: trace contaminants in the electrolyte could easily explain most of the elements claimed to be created in the cell.

And this report on the purported excess heat made by Patterson beads:

http://www.earthtech.org/experiments/rifex/rifexcal.pdf

Executive summary: No excess heat was observed. Our detection limit was about 0.1 watts in this experiment.

[You wrote that methods of generating excess heat at much higher rates might have been developed but the inventors prefer to be quiet about this.] Indeed. Do you think a substantial cash prize, say $1,000,000 US dollars, would get such people to reveal their discoveries? Of course, I wouldn't expect them to give up their discoveries for a relatively small amount of money like that but what if we just asked for a simple demonstration of the excess heat effect and awarded the prize to the first group to succeed?"

Referring to the suggestion that effective excess heat devices might exist but information about them is hidden, Michel Julian asked: "But for how long? This argument couldn't hold indefinitely, not for 18 years anyway." Then he added: "CF researchers could be (well, are) accused in earnest by honest skeptics (like we ourselves may be in other topics such as free energy, UFOs, your own mind-matter stuff, etc.), of being either frauds, or incompetent experimenters, or self deceived indefectible optimists, or face savers. In support of the last point: How could Mizuno's sudden disinterest for his 100W-in 130W-out GDPE experiments be explained logically, apart from 'I was wrong but I won't admit it' ?

It can't be denied that apart from a few rare exceptions we do not criticize/analyze/verify each other's work and claims as diligently as we should. This is IMHO what keeps the field in this state of suspended life. We need more people to take up Earthtech's generous offer of free expertise, and if they pass Scott's hawk's eye they can go and claim the Randi prize right away ;-)" Another researcher reminded the list that in some experiments (Swartz et al. and Dardik et al.) excess heat was large and exceeded the input energy by the factor of ten and more.

This website contains other cold fusion items.
Click to see the list of links

I promised not to post this report before the item shown below was published in The New Energy Times by Steven Krivit.

http://newenergytimes.com/news/2007/NET21.htm

I hope that experiments now in progress will bring more clarity into the SPAWAR effect discovered by Stanislaw Szpak et al. They have been studying several codeposition effects for more than ten years. But their use of CR-39 detectors is relatively recent.

This website contains other cold fusion items.
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319) A contribution to Galileo Project

Ludwik Kowalski; 2/25/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

1) Introduction

As described in the unit #314, I volunteered to be part of the The Galileo Project (TGP) organized by Steven Krivit. Before reading beyond this introduction, please go to

http://newenergytimes.com/news/2006/NET19.htm

and click the link to item #7, entitled "Extraordinary Evidence." You also should read item #8; I do not want to repeat what has already been described on the above webpage. Read these two items carefully and then read what is below. This unit is about our attempt to replicate one of the recent SPAWAR experiments. The study was conducted together with Michael Stolarz, a physics major at Montclair State University, and with two colleagues -- Brian Scanlan and Simon Young. Michael will present a separate paper at the Sigma Xi student research conference. Richard Oriani, who lives in Minneapolis, was our adviser. It is important to emphasize that several papers about nuclear processes triggered by chemical processes in electrolytic cells have already been published by the SPAWAR team in professional journals (see references in 1). The experiment we attempted to replicate has also been published (6).

The heavy-water electrolyte we used was identical to that described in (1). The time table for the electrolysis (see below) was also very similar. We were able to replicate SPAWAR results that were presented, by Frank Gordon (2). But our interpretation of results is different from that offered by SPAWAR team. The diameters of dominant dark pits, on our CR-39 detector, were found to be too large to be attributed to to alpha particles, or to smaller nuclear projectiles.

Inserted on 3/3/07--> After finishing writing this unit, three days ago, I decided to make a presentation at the upcoming APS meeting. I wrote to the session chairman, Scott Chubb, that I have something important to share and would be happy to take place of someone who does not show up. He offered me a time slot and suggested that I write an abstract and distribute it at the conference. Here is my abstract"

Title: Nuclear or not nuclear? That is the question.
Authors: Ludwik Kowalski, Brian Scanlan and Simon Young.

Text: An experiment described by Frank Gordon, last summer (1), has been successfully replicated at Montclair State University. Copious pits were observed on the CR-39 chip that was in contact with the silver cathode wire in the magnetic field. No such pits were observed on the identical chip from the cell that was not in magnetic field. The two cells were in series with each other during the electrolysis. Diameters of large post-electrolysis pits have been compared with diameters of pits due to alpha particles from a radioactive source. On the basis of this analysis, and on the basis calibration curves reported by Roussetski et al. (2), we conclude that large pits on our CR-39 chip, from one experiment, cannot possibly be attributed to alpha particles, or to less massive nuclear projectiles.

References:
1) F. Gordon et al, at the Naval Science & Technology Partnership conference hosted by the National Defense Industrial Association and the Office of Naval Research (Washington, DC, August 2, 2006).) See items 7 and 8 in
2) A.S. Roussetski, A.G. Lipson and V.P. Andreanov; "Nuclear emission from titanium hydride/deuteride induced by powerful picisecond laser beam." Proceedings of the 10th international conference on cold fusion, pages 559-566.

2) Experimental Method

Two identical electrolytic cells were connected to power supply, as shown in Figure 1. The little boxes, at the upper left corner represent integrated circuits designed to maintain constant currents. To change the current we simply had to replace one box by another. The voltage of the power supply was sufficiently high (up to 50 V) to support this mode of operation. This inexpensive method of keeping the current constant was introduced to us by Dr. Winthrop Williams. The experiment would not be possible without his help in dealing with numerous practical problems.

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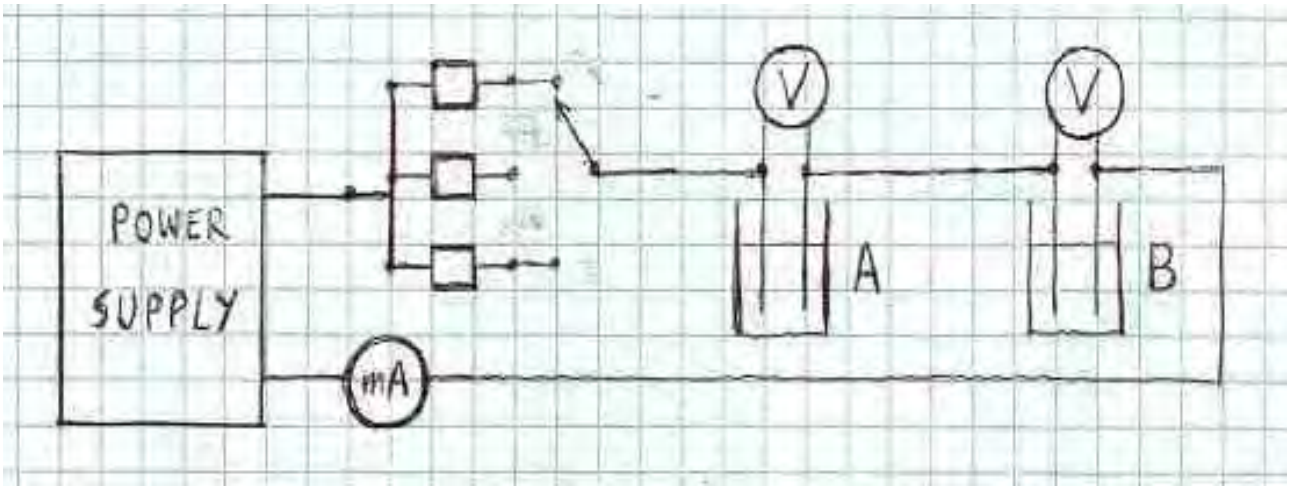

FIGURE 1
Our electric circuit contained nine integrated circuits (LM334 for lower currents and LM317T for higher currents). Only three of them are show in this diagram. A third cell was connected in series with A and B. It is not shown in this figure because it was part of another experiment (to be described in another unit).

- -

The two electrolytic cells, like those described in (1), were made from acrylic. Each has a wire-made platinum anode and a wire-made silver cathode, as shown in Figure 2. A CR-39 detector of nuclear particles was mechanically applied to the silver wire of each cell.

The only difference between the two cells was that one (A) was placed in a magnetic field crated by two very strong

neodymium magnets while the other (B) was far away from any source of magnetic field higher than terrestrial.

- -

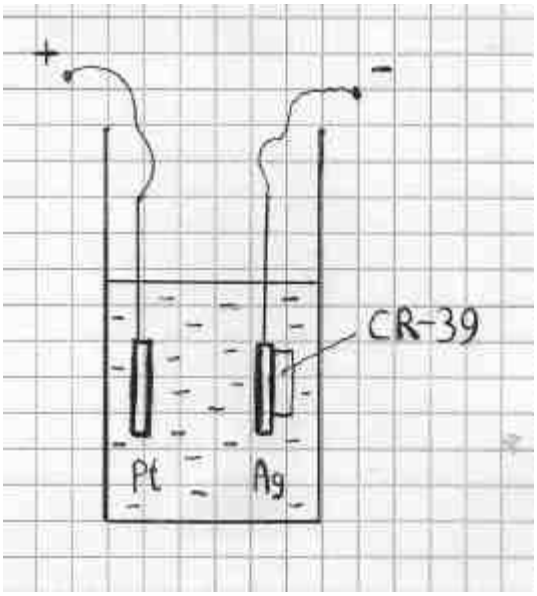

FIGURE 2
The distance between the anode and the cathode was approximately 15 mm. The height of the acrylic cell was 80 mm. The direction of the magnetic field lines (in cell A only) was from the anode toward the cathode. The magnetic field near the cathode was close to 0.2 T.

- -

The following time table was used during the electrolysis:

Current 0.11 mA, 24 hours
Current 0.21 mA, 24 hours
Current 0.56 mA, 12 days, till the electrolyte changed from brown to transparent
Current 1.0 mA, 24 hours
Current 5.0 mA, 24 hours
Current 10.1 mA, 24 hours
Current 26.9 mA, 24 hours
Current 55 mA, 24 hours
Current 127 mA, 24 hours

Heavy water had to be added to cells in order to compensate for what was lost due to evaporation and electrolysis. Increases in electric current were accompanied by increases of potentials across the cells. The potentials at the beginning of the electrolysis were 0.96 volts on each cell; the potentials at the end of the electrolysis were close to 8 volts. (At 1 mA they were close to 1.6 V and at 50 mA they were close to 5.5 V).

After the electrolysis the CR-39 chips were removed and visually examine. The opalescencent lines were seen on the chip from the cell A, more or less where the cathode wires were. No such lines were seen on the chip from the cell B. Another detail is worth mentioning. Before the electrolysis the cathodes in both cells were in contact with detectors along the entire length. After the electrolysis the cathode wire in cell B was still in contact with the detector along the entire length. But the cathode wire in the cell A bulged away from the detector, except near the edges. The chips ere etched together for 3 hours in the standard NaOH electrolyte (6.25 N and 68 C), rinsed and examined under the

microscope. The CR-39 chip removed form the cell A had several areas in which numerous pits were observed, as illustrated in Figure 3. In that respect our results are in very good agreement with results reported by the SPAWAR team. The CR-39 chip removed from the cell B had no areas covered with such pits. That observation is also consistent with what was reported by the SPAWAR team. We agree with them that the magnetic field plays a role in the formation of pits. The mechanism of this effect is not clear at this time.

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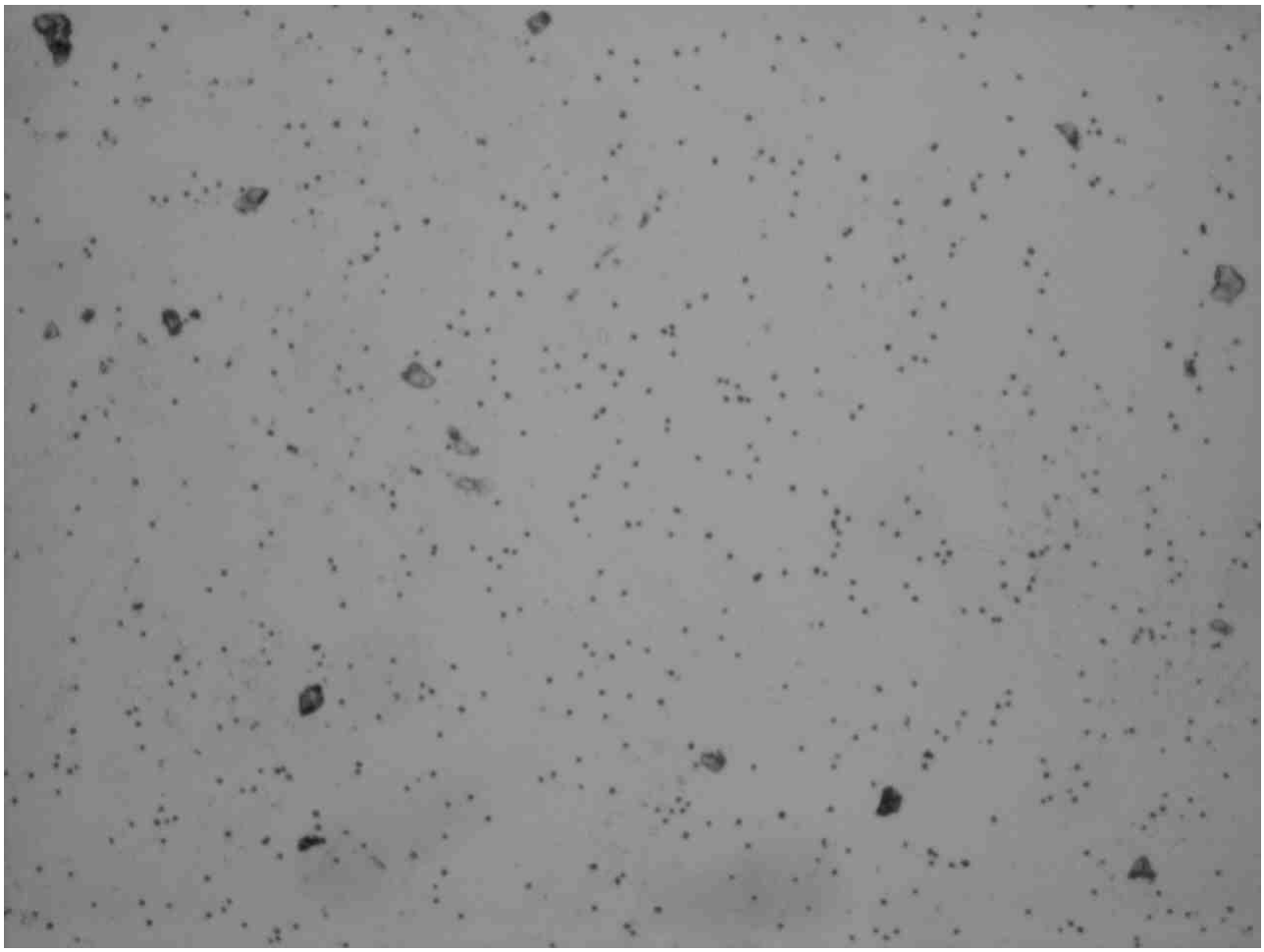

FIGURE 3A
Dot-like pits due to alpha particles from our Am-241 source at magnificatin 40. Surface defects or dust can also be seen. The area on this low magnification photo is 1.3 by 0.9 mm.

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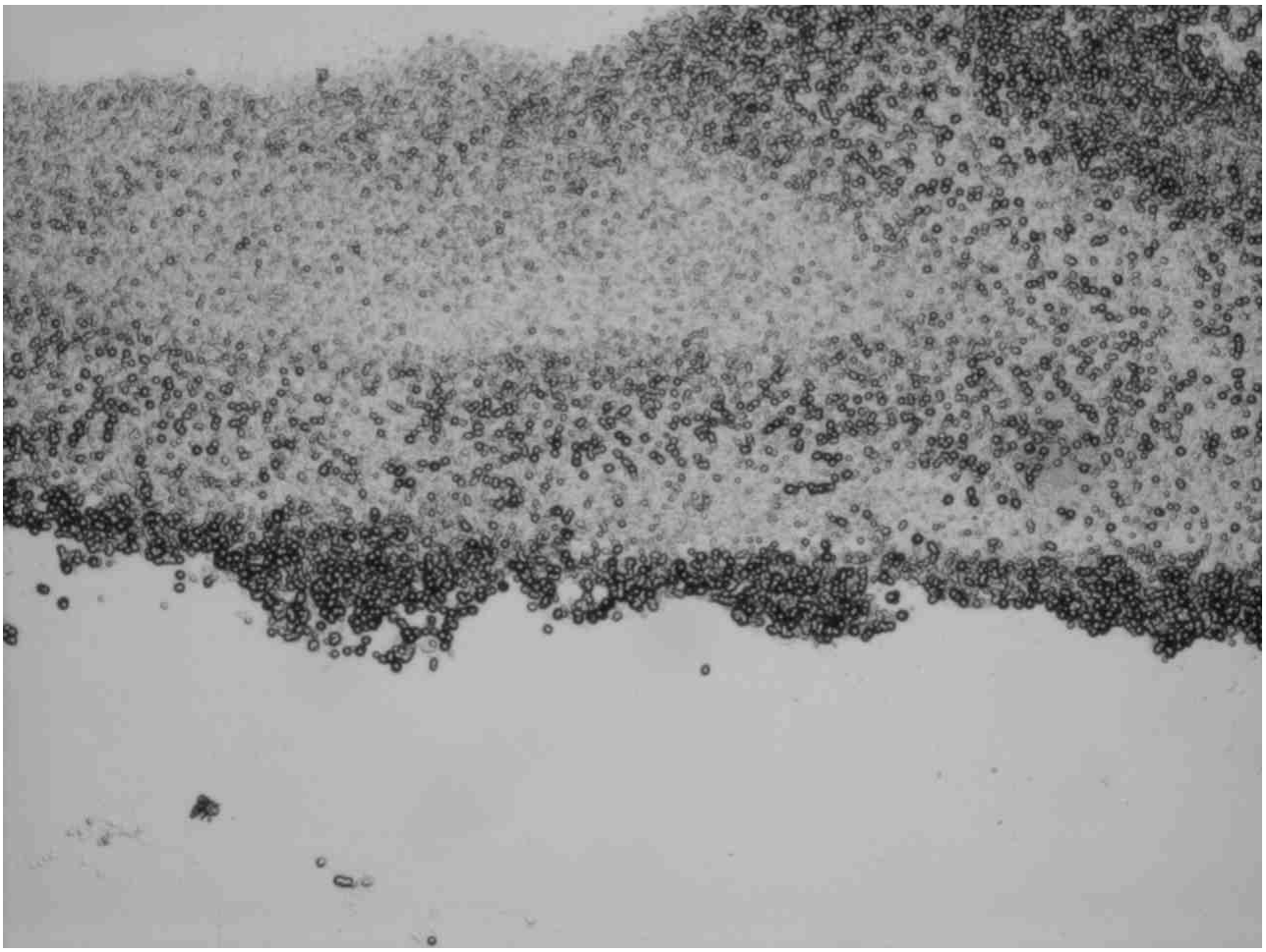

FIGURE 3B
Piits on the post-electrolysis chip from the cell A. Etching conditions and magnification were the same as for the photo on Figure 3A. The area on this low magnification photo is 1.3 by 0.9 mm. Next four figures should be used to compare diameters of post-electrolysis pits with diameters of pits due to alpha particles.

The claim of nuclear origin of pits, made by SPAWAR team, was based on comparisons of observed pits with pits that were known to be due to alpha particles. We followed the same approach. A fresh CR-39 chip was irradiated with alpha particles from an 241 Am source. It was etched together with the post-electrolysis chips. Figure 4 shows pits due to alpha particles, under the microscopic magnification of 200. Figure 5 shows pits on the chip removed from the cell A, under the same magnification. It is clear that the post-electrolysis pits are larger than pits due to alpha particles. The same conclusion can be reached by comparing pits in Figures 6 and 7. They were taken under the magnification of 400.

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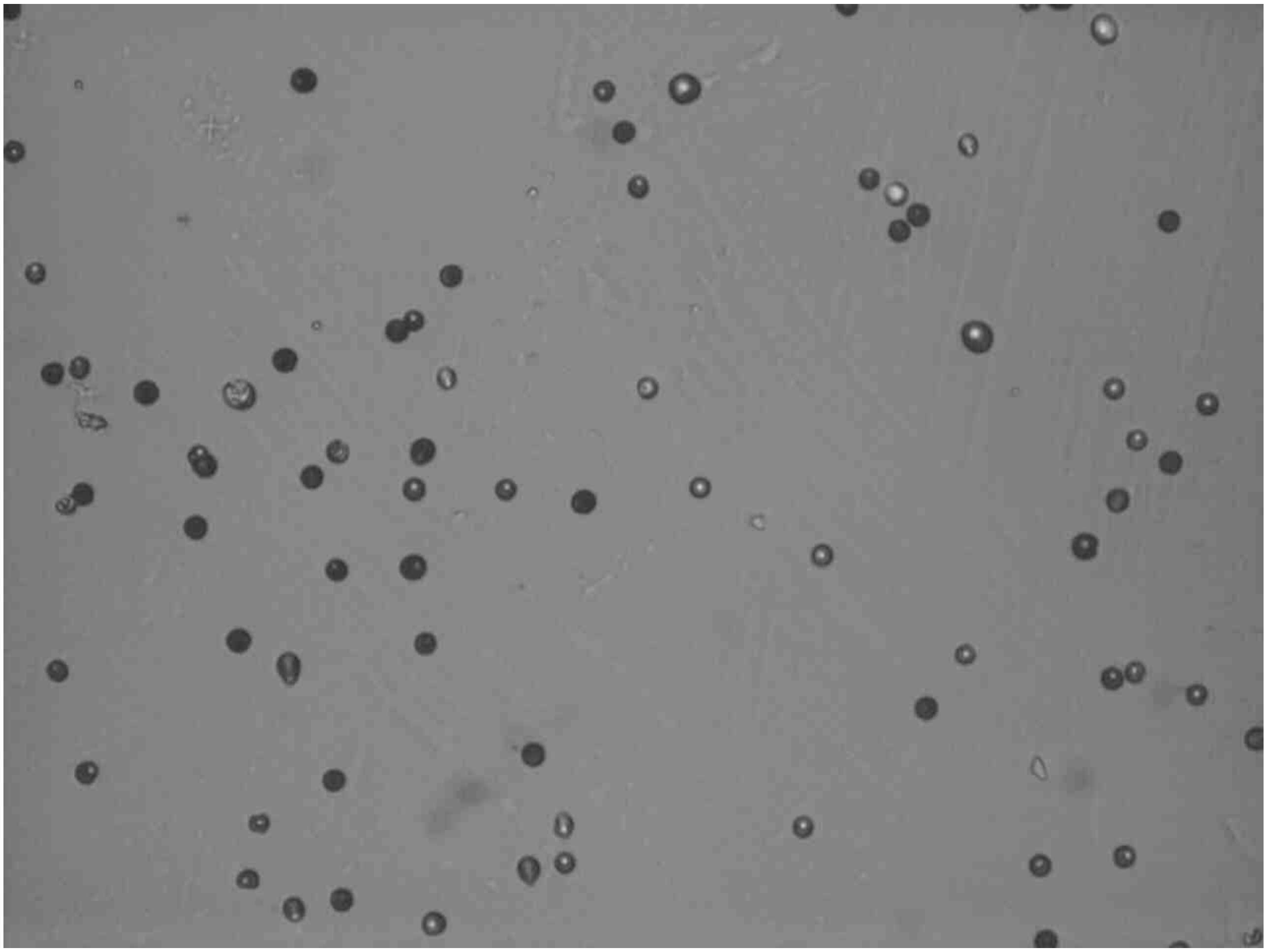

FIGURE 4
Pits which are known to be tracks of alpha particles, under magnification of 200

- -

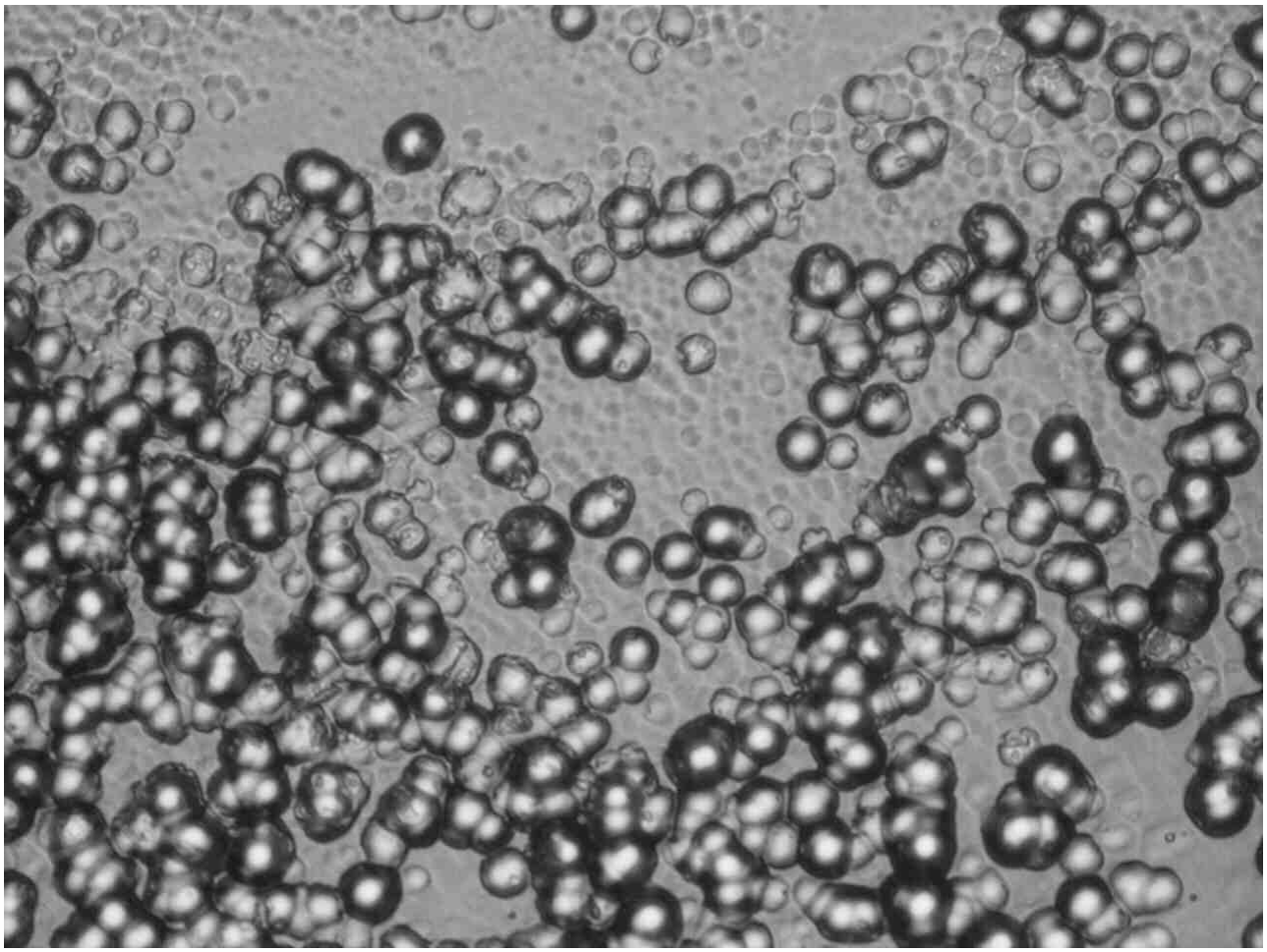

FIGURE 5
Pits on the post-electrolysis chip from the cell A. Etching conditions and magnification were the same as for the photo on Figure 4. Note that diameters of faint pits are about the same, and smaller, than diameters of dark pits in Figure 4. Large dark pits can't be due to alpha particles, or lighter projectiles.

- -

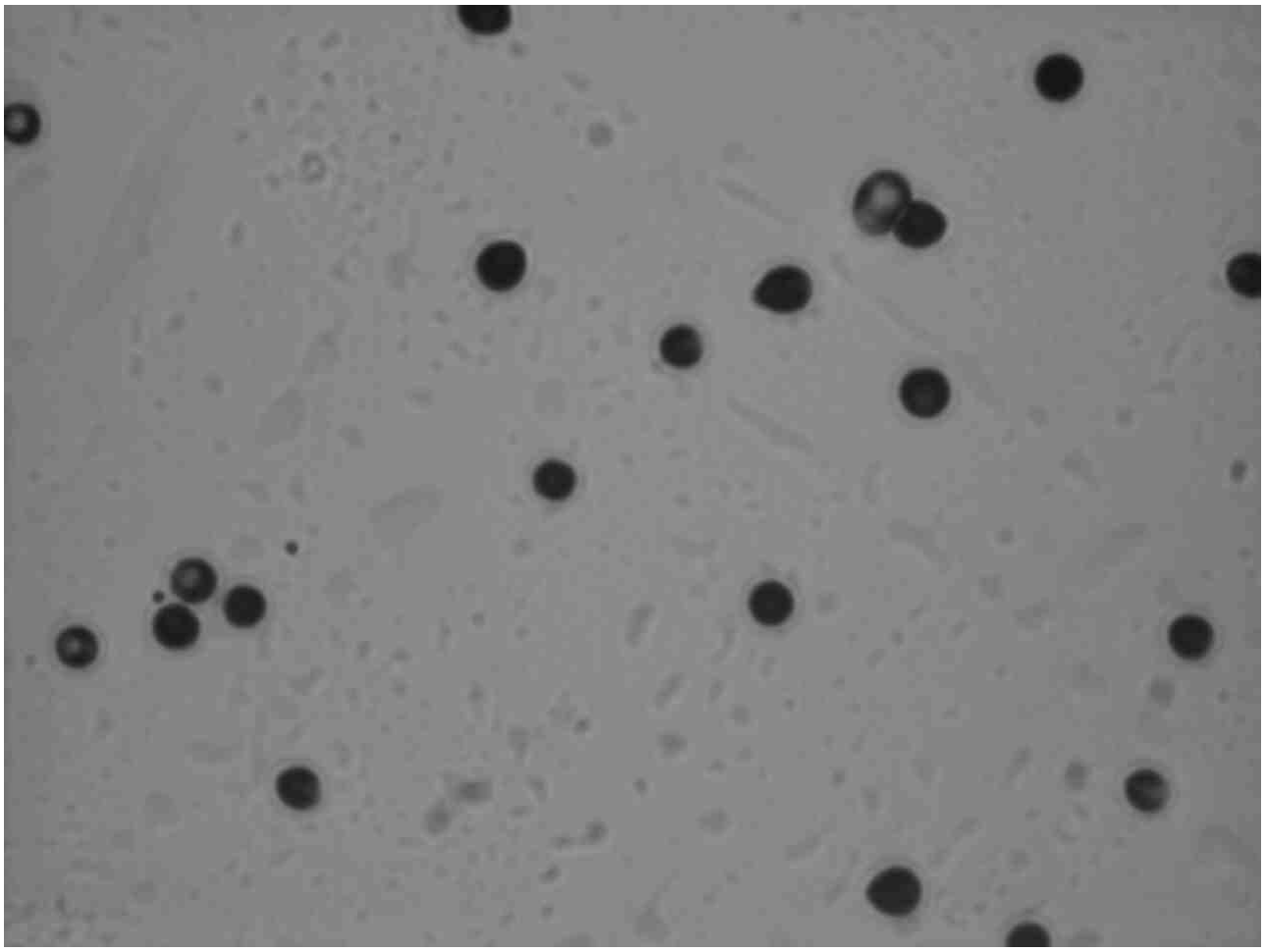

FIGURE 6
Pits which are known to be tracks of alpha particles, under magnification of 400

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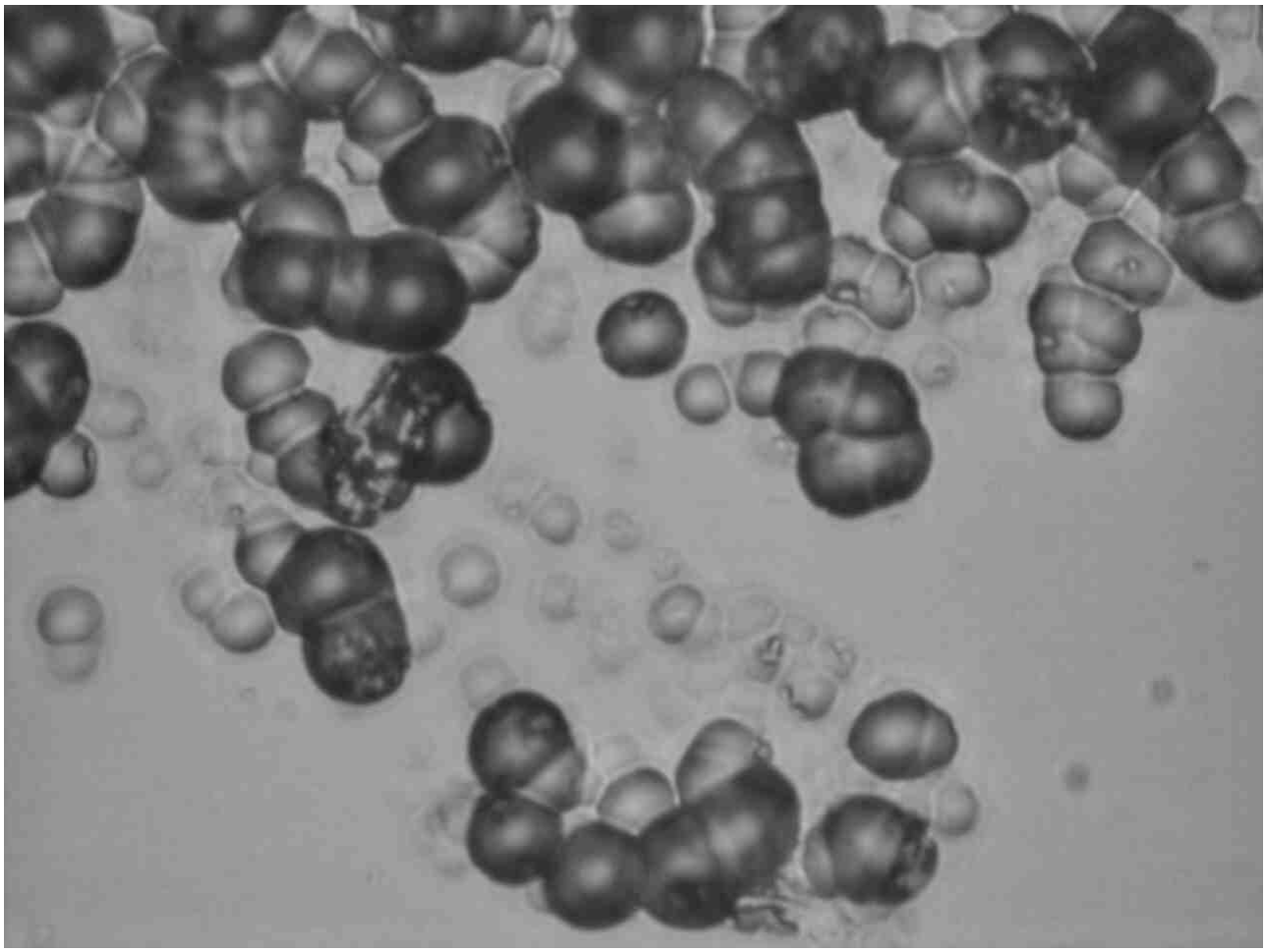

FIGURE 7
Pits on the post-electrolysis chip from the cell A. Etching conditions and magnification were the same as for the photo on Figure 6. Note that diameters of faint pits are about the same, and smaller, than diameters of dark pits in Figure 6. Large dark pits can't be due to alpha particles, or lighter projectiles.

- -

Use of CR-39 detectors in air is common (3). But their prolonged use in the electrolyte, initiated by SPAWAR team, is new. A separate test was necessary to verify that prolonged exposure of CR-39 to the electrolyte does not interfere with its ability to detect alpha particles. A small piece of CR-39 was cut from the chip A, after the electrolysis, and exposed to alpha particles. It was then etched and examined under the microscope. Presence of familiar tracks reassured us that ability to detect particles was not lost during the electrolysis. Furthermore, the thickness of the post-electrolysis chip A, after etching, was compared with the thickens of the blank chip that was etched with it. The difference, if any was, smaller than 5 microns. This additional test was necessary to rule out a possibility that exposure to the electrolyte changed the bulk etching speed.

Absence of pits on the post-electrolysis chip B was a strong indication that the pit-formation process was strongly influenced by the magnetic field. That fact reminded us Marie Curie's attempts to influence radioactivity by magnetic fields. These attempts, like those undertaken by other investigators, were not successful. Magnetic fields do influence some atomic processes but their ability to either slow down or to speed up emission of alpha particles (or protons) from atomic nuclei has never been demonstrated.

3) Discussion

Differences between ^{241}Am pits and pits found on the post-electrolysis chips are not limited to sizes (typical alpha

particles tracks are about 2.5 times smaller than typical post-electrolysis pits). Another difference becomes obvious when one tries to refocus the microscope under a larger magnification, such as 200 or 400. Isolated post-electrolysis pits seem to be more shallow than pits produced by alpha particles. Most post-electrolysis chips appear so close to each other that the individual boundaries are no longer identifiable. The areas covered by overlapping pits are whitish, as if they were affected by some kind of chemical corrosion.

Let us assume that individually identifiable post-electrolysis pits are due to nuclear particles, as claimed by the SPAWAR team (6), and not to a chemical, thermal or mechancal effect. What kind of particles can produce pits shown in Figure 3? To answer this question we will use results of measurements made by Roussetski et al. (4). These Russian scientists exposed CR-39 chips to protons and alpha particles of various energies. Then they etched the chips, measured the diameters and summarized the results in the form of a very useful graph. The smallest diameter (5 microns) was for protons of 3 MeV, the largest diameter (12 microns) was for alpha particles of 2 MeV. The etching in the NaOH (6N, 70 C) took 7 hours.

Our etching conditions were quite different, as specified in section 2. For that reason we decided to modify the original graph slightly, as shown in Figure 8. Our diameters are expressed in arbitrary units named prots. One prot, by definition, is the diameter due to 3 MeV protons, no matter how many microns it is under different etching conditions. The advantage of that representation is obvious; it decouples essential information (relative sizes of pits) from the laboratory-specific details about etching. The modified graph is likely to be valid, at least approximately, when etching conditions are not exactly the same as those used in (4).

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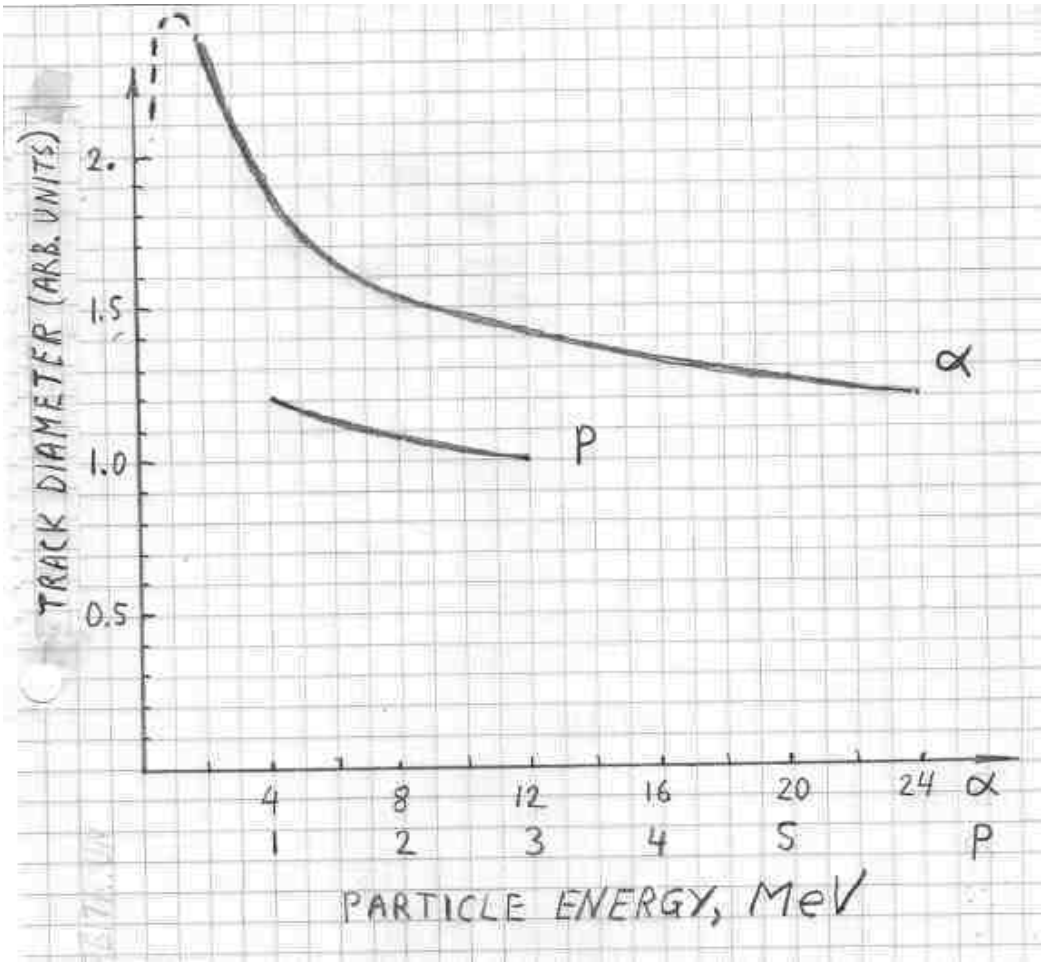

FIGURE 8
Relative diameters of tracks due to alpha particles and protons of various energies. The dashed line, at the upper left

corner, was added to indicate that alpha particles cannot possibly produce tracks larger than about 2.5 prots. This expectation is based on the well known fact that alpha particles at energies below 2 MeV tend to capture electrons when they slow down. The dE/dx values do not increase when E becomes sammler than about 2 MeV.

- -

According to Figure 8, the mean diameter of alpha particles from ^{241}Am (~ 5 MeV) is 1.7 prot. According to Figures 6 and 7, individually identifiable post-electrolysis diameters are about 2.5 times larger than those due to ^{241}Am. In other words, their average pit diameters are 1.7*2.5=4.5 prots This at once rules out a possibility that the post-electrolysis pits are due to protons or to neutrons; diameters of tracks due to these particles would be smaller than 1.5 ports. It also rules out a possibility that poste-electrolysis chips are due to alpha particles; diameters of these would be smaller than 2.5 prot. The only available avenue, for defending nuclear origin of post-electrolysis chips, it to assume that they are caused by nuclear projectiles that are much heavier than alpha particles. Fission fragments from a californium source, for example, were shown (5) to produce pits that were about 2.5 times larger than those due to alpha particles. But that does not mean that any large pit on the CR-39 surface is a track of a massive nuclear projectile. An additional evidence would be needed to defend such claim.

According to (6), "the density of tracks registered by a CR-39 detector was found to be of a magnitude that provides indisputable evidence of their nuclear origin." Our analysis of diameters of pits does not support this interpretation, unless only massive particles are responsible for the large pits shown in Figures 5 and 7. A more likely explanation is that post-electolysis pits are due to a non-nuclear process taking place near the cathode. This kind of interpretation is supported by presence of glossy regions on the chip from the cell A, visible before the chip was etched (see section 2). Prolonged exposures of CR-39 to alpha particles, undertaken to produce overlapping pits, did produce foggy (but not glossy) areas on chips, but such areas could be seen only after etching. One might speculate, for example, that hydroxyl ions, produced by reduction of water, somehow concentrated near the CR-39 surface, attacked the plastic.

Inserted on 4/6/07:
Let me comment on the four pictures at the bottom of:

http://newenergytimes.com/Reports/Krivit-KowalskiCR-39Discussion.htm

The photos were made by SPAWAR researchers. The right two clearly confirm what I was saying about relative sizes of pits. Pits on the post-electrolysis chips are about 2.5 times larger than pits due to our alpha particles, under identical etching conditions. The left two pictures show that for SPAWAR chips the ratio was close to 1.7. This lower ratio also supports my hypothesis. It implies that a typical diameter of promenent pits is 1.7*1.7=2.9 prots. This is larger than the maximum for alpha partcles, as indicated in Fifure 8.

External fields:
The most puzzling fact, discovered by the SPAWAR team (6), and confirmed by our experiment, is that the unexplained pits are produced only when magnetic field is present. Suppose the post-electrolysis pits are due to a non-nuclear process of some kind. In that case the role played by magnetic field is also far from being clear. As far as we know, magnetic fields do not drastically change rates of chemical reactions. If this is true then something new, and totally unexpected, has been discovered by the SPAWAR team. The magnetic field effect is interesting and worth investigating, even if pits are not of nuclear origin. The magnetic field effect, by the way, is a very good indication that observed pits are not due to a familiar process, such as radioactive contamination in the cell or an ordinary chemical attack.

The effect of the electric field, reported in (2), is even more puzzling. The difference of potential of 6000 volts, applied to parallel copper plates situated outside the cell with acrylic walls, should make a negligible contribution to the electric field inside a cell with the electrolyte. This is due to the very high resistivity of acrylic in comparison with the low resistivity of the electrolyte. And yet, the removal of an extremely small electric field stops a process by which track-looking pits are formed on the CR-39 detector. This effect is also interesting and worth investigating.

Additional reflections. (Inserted on 3/16/07):
Why do many scientists reject the idea that a nuclear process can be triggered by a chemical process? Because this idea conflicts with the atomic model they have already accepted. That model, progressively evolved during the 20th century, makes sense out of a large number of experimental facts. It states that chemical reactions are changes at the level of the outermost electrons orbiting around atomic nuclei. Nuclear reactions, on the other hand, are changes occurring in tiny central regions of atoms. Nuclear force bounding protons and neutrons in these tiny regions have very short ranges; they have no effect on orbiting electrons. Coulomb forces between electrons and protons are negligibly small in comparison with nuclear forces. That is why electronic rearrangements (chemical changes) are generally expected to have negligible effects on what is going on inside atomic nuclei.

In research, and in teaching, we progress from what is already known toward what remains hidden. New models of reality are refinements of models that are known to be consistent with experimental facts. That is why many scientists say that cold fusion claims are extraordinary. They feel that one has to be extraordinary reserved toward such claims. To "be reserved," however, does not mean to "reject." It means to ask many more questions than in the case of claims that are consistent with accepted models.

It is important to emphasize that that SPAWAR scientists, like many other cold fusion researchers, are experimentalists. They observe what happens and try to interpret it in terms of familiar concepts. Our photos (shown in Figures 3, 5 and 7) are very similar to their photos (shown in reference 2). But we do not know if large pits on their post-electrolysis chips are also considerably larger than pits on their chips exposed to alpha particles (under identical etching conditions). The disagreement is about the idea that large post-electrolysis chips can be attributed to alpha particles, and lighter nuclear projectiles. Fortunately, experiments seem to be reproducible. In that situation the disagreement can easily be turned into an agreement, one way or another. It becomes a simple matter of remeasuring, or repeating the experiment. Will the clear picture emerge before the next week anniversary of cold fusion? I hope so.

Inserted 3/23/07
Our paper was discussed extensively on the private list for CMNS researchers. Replying to one message I wrote:

From past experience (placing CR-39 on a Pu-Be neutron source) I know that most tracks due to neutrons (via collisions with hydrogen nuclei in that material C_{12}-H_{18}-O_7) have diameters that are clearly smaller than diameters due to alpha particles from the Am-241 source. Less than one percent of tracks due to neutrons had diameters comparable to those seen on the chip exposed to Am-241. I attributed these large pits to the (n,alpha) reactions. That is why I said that pits larger than those due to Am-241 (identical magnification and etching) cannot be attributed "to neutrons." Perhaps I should have said "to protons produced by neutrons." I am not denying a possibility that large dark pits on our photos might be due "to carbon or oxygen ions produced by neutrons," as suggested at the APS meeting by Larry.

Let me estimate the number of neutrons that would be responsible for large pits Larry observed on the "back side" of his CR-39 chip. The rule of thumb used by Frank Gordon (private communication during the APS conference) is that only one out of hundred thousand fast neutrons, passing through a CR-39 detector, interacts with with atomic nuclei insid the detector. Taking this for granted, let me estimate the number of neutrons from the number of large pits. To accomplish this I will make the following assumptions (which can easily be modifies). To accomplish this I will make four simplifying assumptions. These arbitrary assumptions can be modified, if necessary.

a) One half out of 100,000 intercepted neutrons is colliding with heavy nuclei in CR-39 producing ions of C and O. The other half is scattered on hydrogen.
b) The mean range of heavy ions in CR-39 is 1 mg/cm^2, which is 8 microns.
c) Larry's large dark pits are due to only 25% of heavy ions produced in the last 8 microns of CR-39.
d) The thickness of the CR-39 chip is 1 mm, which is 1000 microns.

Thus the fraction of ions responsible for Larry's pits is 0.25*8/1000=1/500. In other words, the number of large pits will be 500 times smaller than the number of neutrons interacting with heavy nuclei inside the CR-39 chip. If 100,000 neutrons produce one interaction then 500*100000=5*10^7 neutrons will produce one Larry's pit. For 1000 pits the

number of neutrons would be 5*10 . This is 50 billion. Another conclusion is that large pits should also be observed on the front surface of the CR-39 chip.

This speculation was posted on the private list for CMNS researchers. Here is a comment from one researcher: " If 50 million neutrons were emitted by the cell during the last 48 hours of the run when the current is at its highest value (and presumably making the nuclear reactions happen), the flux onto the CR-39 chip would be 290,000 neutrons/sec. That'd make any neutron detector sing, so to speak. Also, assuming the neutrons are 2.5 Mev, it only takes 20 neutrons/cm^2/sec to make a dose rate of 2.5 mrem/hr (1)... so the CR-39 chip would see a dose rate of ~36 rem/hour (assuming the chip was 1 cm^2). The dose falls off rapidly with distance but, still, such an experiment would be of considerable hazard to the health of bystanders. (1). Radiation Detection and Measurement, Glenn Knoll, 1979, p. 802."

After reading the 2002 paper of J.A. Frenje et al. (Review of Scientific Instruments, vol. 77, July 2002, p 2595) I realized that the assumption (a) above was highly unrealistic. Experimental data show that the number of large pits due to heavy ions represents only a very small fraction of one percent of all pits, even for 14 MeV neutrons. For that reason the 50 million neutrons was underestimated by orders of magnitude. To produce 1000 pits on CR-39, the number of neutrons had to be much larger than 5 billion. Fortunately, a Landauer neutron detector was used in one of The Galileo Project experiments. How many neutrons were actually detected during that experiment? I will answer this question as soon as the outcome is announced.

References:

(1) Stanislaw Szpak · Pamela A Mosier Boss, Charles Young and Frank E. Gordon; "Evidence of nuclear reactions in the Pd lattice." Naturwissenschaften (2005) 00: 1–4 DOI 10.1007/s00114-005-0008-7. he document can be downloaded from the library at http://lenr-canr.org

2) F. Gordon et al, at the Naval Science & Technology Partnership conference hosted by the National Defense Industrial Association and the Office of Naval Research (Washington, DC, August 2, 2006). The document can be downloaded from the library at http://lenr-canr.org

3) D. Nikezic and K.N. Yu, "Formation and growth of tracks in nuclear track materials;" Material Science and Engineering R 46 (2004) p 51-123.

4) A.S. Roussetski, A.G. Lipson and V.P. Andreanov; "Nuclear emission from titanium hydride/deuteride induced by powerful picisecond laser beam."Proceedings of the 10th international conference on cold fusion, pages 559-566. The document can be downloaded from the library at http://lenr-canr.org

(5) D. Paul et al., "An SSNT study of spontaneous fission fragments from the soil gas samples of Bekerswar thermal springs;" Radiation Measurements 33 (2001) p 167-169

6) Stanislaw Szpak, Pamela A Mosier Boss and Frank E. Gordon; "Further evidence of nuclear reactions in the Pd/D lattice." Naturwissenschaften (2007) DOI 10.1007/g00114-007-0221-7. Can be downloaded from the library at http://lenr-canr.org

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320) Phase 2 of Galileo Project

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1) Introduction (written in September of 2007):
CMNS researchers are fully aware that a simple reproducible-on-demand demo of a nuclear effect, such as emission of alpha particles, caused by a chemical process would have profound effect on mainstream scientific community. The accepted point of view is that chemical processes (interactions involving valence electrons in atoms and molecules) are too weak to produce emission of nucleons, or groups of nucleons, from atomic nuclei. Yet, several researchers, such as Lipson, Jones, and Oriani, have been reporting unexpected emission of alpha particles, for many years.

About two years ago I decided to replicate Oriani's effects. This was described in unit #192 at this website. I did observe results reported by Oriani but they were not sufficiently reproducible. Last summer I found out that Richard Oriani has a draft of an unpublished paper in which new results, confirming emission of nuclear particles, are described. The methodology used by him was essentially the same as that described in unit #192. But instead of investigating several effects he decided to focus on one effect -- emission of alpha particles from a cathode, after the end of electrolysis. Thirty seven similar experiments were performed; a typical electrolysis time was 3 days and a typical counting time was 2 days. The mean track density, attributed to nuclear particles, turned out to be 90 tracks per cm^2. The standard deviation was 35; and the lowest result was 40. This was significantly higher than results from 24 control experiments, where the mean track density turned out to be 20. The standard deviation for the control samples was 11 and the lowest value was zero.

2) Inserted on 3/28/07:
Impressed by the above results, I decided to perform similar experiments. But plans changed after The Galileo Project (TGP) was organized by Steve Krivit. The Phase 1 of that project, and our contribution to it, are described in unit #319. When we were busy with the Phase 1 Oriani was performing a new sequence of experiments. The major modification, in comparison with his earlier setup, was a very thin window (5 microns of polypropylene) placed between the O rings. The cathode wire was above that window (in the electrolyte) while the CR-39 detector was below that window (in air). With this setup Oriani was able to record tracks of nuclear particles, presumably alpha particles or protons. They were emitted during the electrolysis, not after the electrolysis. The numbers of tracks on chips below the cathode were about two orders of magnitude higher than on blank chips of the same size. The photo of tracks after one of his experiments is shown in Figure 1 below.

The idea of using thin foils separating CR-39 detectors from the electrolyte has been discussed extensively on a special Internet list for TGP researchers. But Oriani was the first to implement this idea. He was also the first to use the term "PACA detector," for a CR-39 chip "protected against chemical attack." In fact, he invented this term after being convinced that at least some of copious SPAWAR pits resulted from chemical, rather than nuclear, activity.

- -

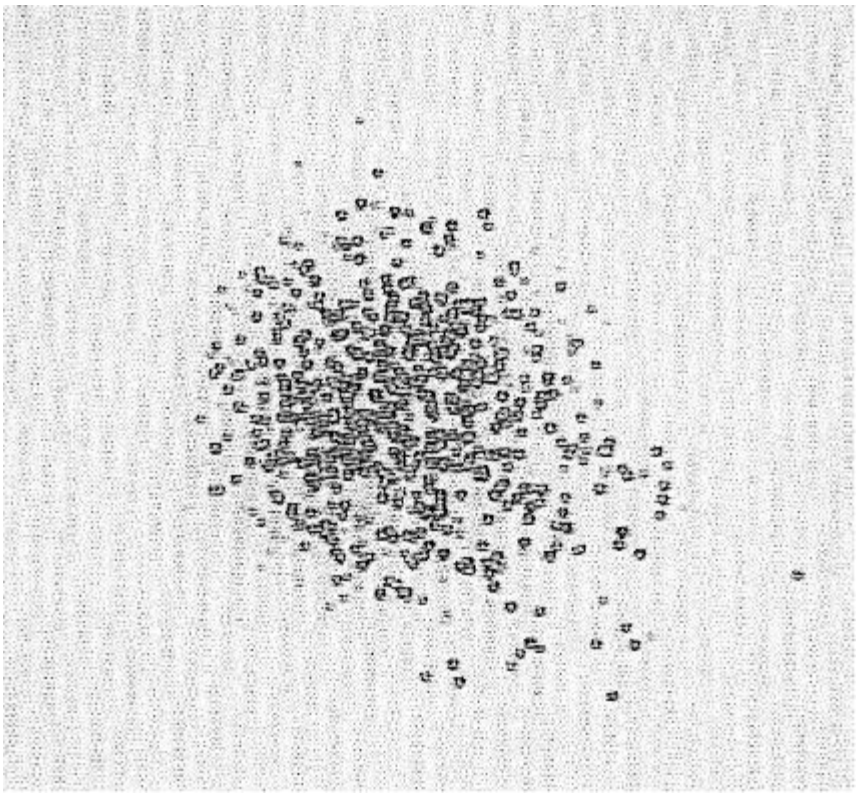

Figure 1
A spectacular shower of Particles reported by Richard Oriani. Magnification was 100; the area on the photo is about 1.5 mm^2." The clustering of tracks is remarkable. In a private message (4/2/07) Richard wrote: "In past work I have compared mean diameters of tracks produced by Am241 and by my experiments when both detector chips are etched at the same time, and have found that the tracks produced in the experiments are a bit smaller than those produced by Am 241. "

- -

Our Phase 1 contribution to TGP project has been described in a paper presented at the recent APS meeting (see unit #319). Proceeding toward the Phase 2 we wanted to replicate new Oriani experiments. What follows was composed from my own notes, and from pieces of information supplied by other people. Numbering of individual sections of this unit corresponds to chronological order, more or less, in which separate pieces were added and modified. I apologize for some repetitions.

3) Experimental method
Our version of Oriani's thin-window cell is shown in Figure 2 below. Note that only one O-ring was used in our first Phase 2 experiment. Oriani, on the other hand, used two O-rings, one on the wet side of the window and one on the dry side. In at least one case he used the same heavy water electrolyte as the SPAWAR team (see unit #319) in other cases he used Li_2SO_4 in light water. Results, he reported, were not very different. He did not use magnets. In our first Phase 2 experiment the electrolyte was also the same as that used by the SPAWAR team. In our second Phase 2 experiment, still being analyzed, we used the same electrolyte as Oriani (2.367 grams of Li_2SO_4 in 100 cc of light water)

- -

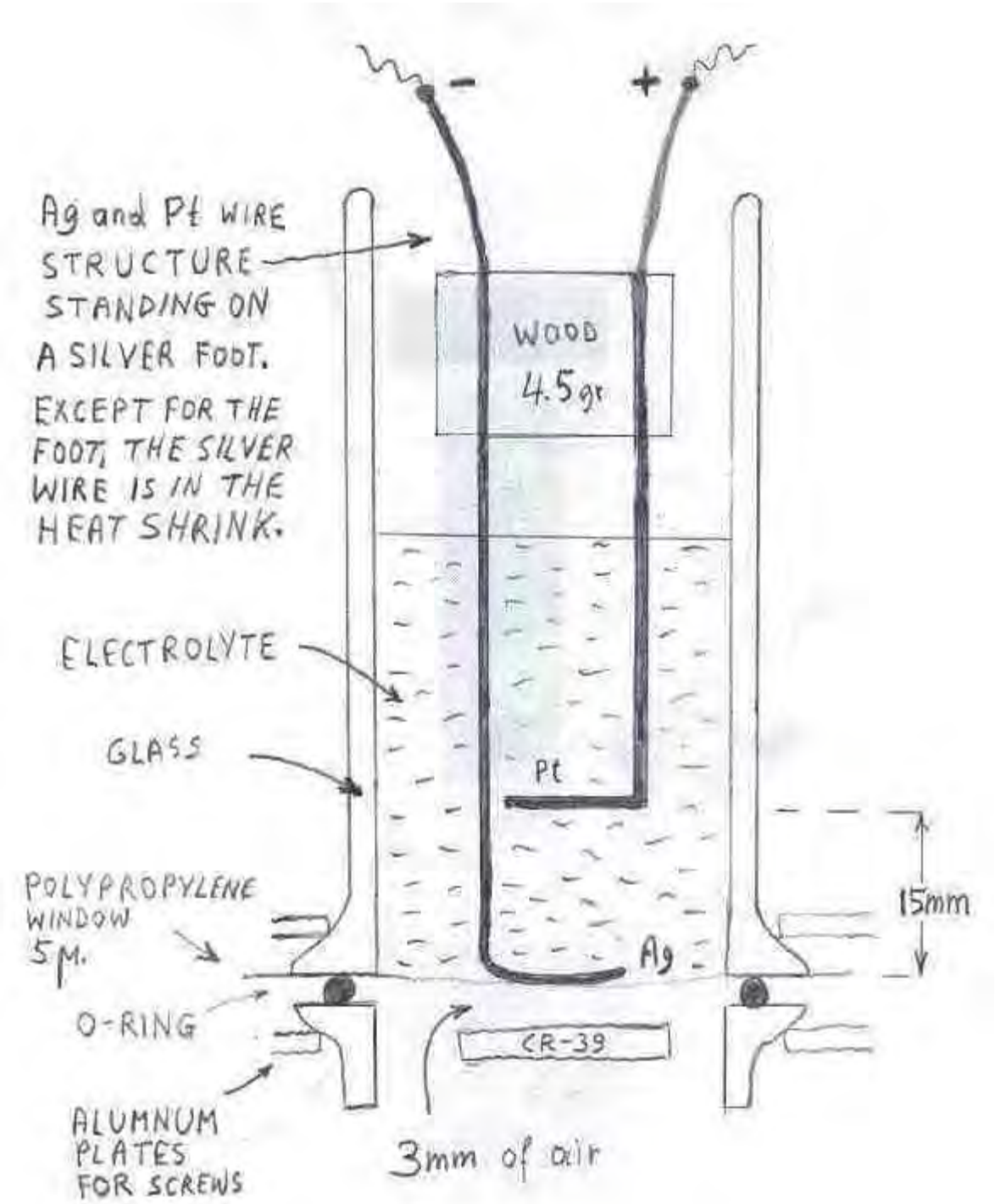

Figure 2
An Oriani-type cell with a thin polypropylene window. The rigid cathode-anode structure is supported by the thin layer of polypropylene.

- -

Contact between the polyethylene window and the silver-wire cathode (see Figure 2) was due to the weight of the cathode-anode structure. The total mass of that structure, in the first experiment, was close to 7 grams. In the second experiment the weight was close to 17 grams -- wood was replaced by a plastic cylinder and an iron nut.

4) Additional experimental details.
The electrolysis time schedule was as described in unit #319, except that the first day (I=0.5 mA) was skipped; the

third cell was inserted into the circuit one day after the first two cells. The CR-39 chip was located below the window. Another CR-39 chip was located next to the glass tube (see Figure 2). The two chips were etched and analyzed, as usual. Here is how the result was described in a message posted on our CR-39 list, on 3/1/07:

"We etched all chips in a fresh NaOH solution for 6 hrs at ~70 C and americium pits confirmed that etching was sufficiently long. But post-electrolysis chips did not show anything significantly larger than the background. Here are details:
a) The area of the chip below the thin window was about 0.95 cm^2. The surface facing up (looking at the cathode wire 3 mm away) recorded 17 track-looking pits.
b) The blank chip, exposed to air, far away from the cell, had the area of 1.6 cm^2. The number of pits one one side was found to be 17 while on the other side it was 10.
c) The area of the chip in contact with the glass tube (in air, looking toward the electrolyte above the anode, the bubbly region) was 1 cm^2. The number of pits on the side facing the tube was 27, the number on the opposite side was 14.

In my opinion the signal should be at least ten times stronger that the noise (number of pits on the blank chip) before it can be taken for granted. I know that three standard deviation is considered to be sufficient, under well defined conditions. But such conditions are not satisfied in our experiments. Oriani's signal/noise ratio was much larger than 10; our results are significantly different from his results. But Richard's experiment was not exactly like our's. Today we started another Oriani-type experiment; it will last about 10 days. The experiment will be supervised by Mike because I will be away. The geometry is similar to our first Phase 2 experiment but the cathode is made from nickel wire and the electrolyte is Li_2SO_4 in light water. "

The cell for our second Phase 2 experiment was essentially the same as in Figure 2 above. But the distance between the anode and a cathode was 24 mm. Here is how our preliminary result was described on 3/15/07 (in a message sent to Oriani, Brian and Simon).

"Oriani 2 experiment ended today. Working with Mike we examined three CR-39 chips that were positioned to detect particles during the electrolysis. Quick examination showed that there might be slightly more tracks on these chips than on the blank chip. But it will certainly not going to be the factor of ten difference (as reported by Oriani) or the factor of 7 (as in a recent SPAWAR experiment, according to Pam). Mike will analyze the chips and will report the actual number of pits. This will be the main part of his paper, to be presented at the Sigma-Xi student research conference. The other part will be comparison of background on CR-39 from three sources. And he will have an appendix explaining CR-39 detectors.

Before specifying details I want to mention an interesting observation. The chip that was below the cathode (originally about 2 mm from it, in air) had a naked-eye-visible circle when we removed it. Contrary to my expectation, the circle was not washed away after 4.5 hours of etching at ~78 C. Microscopic examination (after etching) showed that the circle is not made from track-looking pits; it was a grayish area matching the cathode wire "foot.". What can this circle be? Here is my guess. The polypropylene foil, supporting the weight of the electrolyte, and the weight of the cathode-anode structure, sagged and was actually in contact with the CR-detector. In other words, the cathode "foot" was separated from the CR-39 by only five microns of the plastic window. The circle (imprint of the cathode) was probably due to a thermal (or mechanical) effect. What else can it be?

a) Electrolyte: Li_2SO_4 in distilled H_2O (2.367 grams in 100 cc of water)
b) Distance from the circulat cathode (Ni) and pancake anode (Pt) was 24 mm.
c) Timing: 30 hrs with 10 mA, 72 hrs with 50 mA, 192 hrs with 127 mA and 30 hrs with ~270 mA (at the end, without a current stabilizer).
d) Three CR-39 chips were used. One bolow the polypropylene window (where the circular imprint was discovered), one in the air above the tube (exposed to escaping H_2 and O_2) and one applied to the external side of the cell (at the level below the anode). We also had two blank CR-39 chips and two chips exposed to Am-241. Pits due to americium were well developed."

5) A message from Oriani:

Here is a very interesting message that Oriani posted on the CR-39 list for TGP researchers: "I thought that it may be useful for me to submit for your consideration my results so far from experiments with film-protected CR39 during electrolysis in which the cathode is in close contact with the protecting film. The work so far has been designed only to see if one can get repeatedly more tracks than background level with this procedure. A systematic investigation to try to optimize the production of tracks is still to be done. . . .

I believe that one can conclude that the SPAWAR electrolyte composition in D_2O is not particularly advantageous. One sees also that the formerly used O-ring adds considerably to the tracks/cm^2 seen in an experiment. Additional experiments have shown that the O-ring retains a residual radioactivity after an electrolysis, and presumably so do other portions of the apparatus employed in an electrolysis experiment, as described by Fisher's polyneutron theory. All of the results so far share the characteristics that the track distributions are bi-modal, ie. groups and widely separated tracks, and that there is no discernible correspondence with the position of the cathode wires. There is also no correspondence between the tracks on one side of the detector and the tracks on the other side. These are independent events."

6) Future plans and ideas

After reading the above message, I decided to perform at least one more Oriani-type experiment. But not immediately; I want to learn more from the ongoing Phase 2 experiments of Pamela Mossier Boss et al. And I am not the only one who made such decision. In a private message received this morning Oriani promised to send me some "kindling" for another attempt to observe showers.

Will our result -- prominent pits on post-electrolysis chips are about 2.5 result larger than pits due to alpha particles, under identical etching conditions -- be confirmed by other researchers? So far I have seen only one confirmation (from TGP beta team #2) and not a single negation. This is not enough; hopefully other TGP researchers will soon ether confirm or negate our results. I also want to know if my interpretation of the above result is valid. This section will probably be expanded when new results, and new interpretations, become available. Please do not miss unit #321 which I am posting at the same time. It shows my view of scientific issues raised in The Galileo Project.

7) Polyneutrons

Oriani referred to a theory developed by John Fisher. My understanding of that theory has been described in units #191 and #227. But that was about two years ago. I know that John has been improving his theory constantly. Unfortunately I am not familiar with the curren version. Here is a recent message from John; it was posted on the CMNS list (3/13/07):

"Some researchers have regular success with starting nuclear reactions in their laboratories whereas others, notably those trying for the first time, can wait for weeks or months without success. This may not be the fault of the beginners. Polyneutron theory suggests that a free polyneutron is required to initiate a reaction (just as a free neutron is required to initiate a fission reaction), and that such polyneutrons are emitted by precursor particles in the air and water. The precursor particles are exceedingly rare and it can take considerable time for ignition of the first reaction in a laboratory. After that things get easier. A successful reaction produces large numbers of the required precursors, many of which stick to the experimental equipment or escape and condense on surfaces throughout the laboratory. On subsequent attempts they are available in the equipment, or are desorbed from the laboratory surroundings, enabling much prompter ignition. Hence it may be helpful to seed a start-up experiment with a bit of hardware or electrolyte taken from a successful one. This is equivalent to transporting an ember from a fire to a stack of kindling, rather than waiting for lightning to strike."

That is an interesting observation. Replying to the above I wrote: "Irreproducibility is taken by John as an experimental fact. Guided by that fact he found a theoretical explanation. That seems to be consistent with scientific methodology. But is the explanation valid? Oriani had eight consecutive successes in observing CINA (chemically induced nuclear activity). Our one recent attempt to reproduce it failed. That attempt was made at the same time (February and March 2007) at which we were able to successfully reproduce the SPAWAR experiment. Another attempt to reproduce Oriani's experiment is in progress. If it fails then I will ask Oriani to send us kindling pieces. I suggest that other researchers, using CR-39 and thin foils, do the same. Were we just lucky to replicate the SPAWAR

effect [without any kindling] or Fisher's theory does not apply to SPAWAR effect? . . . "

And here is a message I posted on the same list more recently (3/16.07). "In reading Steve Krivit's summary, just published in the #21 issue of NET, I see a reference to a recent PACA-type experiments (Protected Against Chemical Attack) at SPAWAR. It is an indication that large dominant pits, possibly due to corrosion (term first used by Oriani, if I recall correctly), mask presence of smaller and much less abundant pits due to nuclear projectiles. Oriani's eight consecutive experiments confirmed presence nuclear tracks on PACA chips. SPAWAR team also reports presence of tracks "more than seven times the background." Unfortunately, our (beta 5) results, and preliminary results reproted by beta 2 and beta 6 teams of TGP, seem to be very different in that respect. I hope that the situation will become more clear when other beta teams, and other investigators, share the results of their Phase 2 observations on this list. It would be interesting to know if tracks observed on PACA detectors are also formed only when magnetic and electric fields are present. That would be a highly convincing argument that tracks are not from radioactive contamination.

9) Other things.
This section is a place for an insertion that has nothing to do with science of TGP. I plan to write that insertion after it becomes clear whether or not the SPAWAR effect is an example of a chemically induced nuclear activity. Keep in mind that only one half of the Phase 1 teams reported the results so far. According to our leader, Steve Krivit, the Phase 2 was supposed to be less rigid than Phase 1. It was not to be a project in which each beta team was expected to follow a protocol developed by alpha teams. Each beta team researcher was expected to participate in Phase 2. Let us hope for more reproducible and convincing data from the second phase of The Galileo Project.

Also keep in mind that the SPAWAR claim is not based only on recent discoveries made with CR-39 detectors; it is also based on other observations, as described in numerous papers. References can be found in the 2007 paper of S. Szpak et al.:

<http://lenr-canr.org/acrobat/SzpakSfurtherevi.pdf>

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321) Scientific Issues in The Galileo Project

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Available results from The Galileo Project (TGP), as described in the March issue of this journal, confirmed the presence of copious SPAWAR pits. That is very important; it shows that the effect is reproducible. But is this discovery an example of nuclear activity (emission of nucleons from atomic nuclei) initiated by electrolysis? That question remains unanswered. About 30 researchers, from different laboratories, (two alpha teams, six beta teams and two advisers) continue to address it. I am one of these researchers. Let me outline six pending scientific issues that have emerged from the ongoing investigations..

1) Unconventional use of detectors
CR-39 detectors, used in TGP, are also used in other areas of science and technology. But we used them in the electrolyte, rather than in air. Is it possible that prominent pits, recorded in the electrolyte, are due to chemical corrosion and not to nuclear projectiles? That possibility was raised by the beta 2 team, after examination of their CR-39 detectors.

2) Relative sizes of prominent pits
Absolute sizes of CR-39 pits, due to nuclear particles, are known to depend on the time of etching and on the temperature of the etching solution. But relative sizes of pits, for example, in comparison with those due to alpha particles from an americium source, are likely to be much less dependent on etching parameter. According to two TGP teams (beta 5 and beta 2) diameters of prominent pits are about 2.5 times larger than diameters due to alpha particles (under identical etching conditions). Will this experimental fact be confirmed by other teams? That remains to be seen. My interpretation of this fact was offered at the recent APS meeting. It has also been described at

http://csam.montclair.edu/~kowalski/cf/319galileo.html

Will other scientists agree with me that large SPAWAR pits “cannot possibly be due to alpha particles or protons?” That also remains to be seen.

3) Emission of neutrons:
Two researchers, R. Oriani and L. Forsley, independently reported seeing pits on both sides of thick CR-39 chips. This led to a hypothesis, offered by Forsley, that at least some nuclear particles emitted in the SPAWAR effect are neutrons. Fortunately, two neutron detectors were used in one TGP experiment. Will the analysis of results confirm the hypothesis? That also remains to be seen.

4) PACA detectors:
Suspecting that dominant pits on post-electrolysis chips are due to some kind of corrosion, and expecting to detect less prominent pits due to nuclear projectiles, Richard Oriani started experimenting with PACA detectors. The acronym stands for Protected Against Chemical Attack. In his setup the CR-39 detector is separated from the electrolyte by a thin polypropylene film (5 microns). The wet side of the film is in contact with the cathode while the dry side is in contact with the CR-39 chip (located in air). He performed 8 consecutive experiments with that setup and reported positive results (at least ten times more pits on a PACA detector than on the control detector). Similar results were mentioned by Pamela Boss. Our beta 5 team was not able to replicate such results. This, however, does not necessary

conflict with positive results because experimental conditions were not identical. A confirmation of positive or negative results is likely to emerge when experimental conditions are normalized.

5) Electric and Magnetic fields:
According to the November issue of NET, tracks of nuclear particles are detected only when electrolytic cells are located in external fields, either electric or magnetic. The magnetic field effect was confirmed by our beta 5 team. But, according to the beta 2 team, magnetic field has no effect on the pit formation process. Results from other teams have not yet been announced. The issue remains unresolved.

6) Other nuclear signatures:
It is important to be aware that the SPAWAR claim, about a nuclear effect resulting from a chemical process, was not based only on CR-39 detectors in the electrolyte. It was also based on production of tritium (and other new elements), on generation of characteristic X rays, and on morphological changes on surfaces of cathodes. Hopefully these observations will also be subjected to scrutiny of independent investigations.

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What follows are some useful references, for those who did not follow the short history of The Galileo Project.

1) November 2006, the project is announce by Steve Krivit. See items 7 and 8 in the November issue of New Energy Times. <http://newenergytimes.com/2006/NET19.htm>

2) January 2007, publication of the SPAWAR paper in a refereed journal. Stanislaw Szpak · Pamela A Mossier Boss, and Frank E. Gordon; "Further evidence of nuclear reactions in the Pd/D lattice." Naturwissenschaften (2007) DOI 10.1007/g00114-007-0221-7. Can be downloaded from the library at <http://lenr-canr.org>

3) March 2007, Two Cold Fusion sessions at the meeting of American Physical Society, Denver, Colorado. See item 10 in the March issue of New Energy Times <http://newenergytimes.com/news/2007/NET21.htm>

3a) March 2007, Pamela A. Mossier-Boss, Stanislaw Szpak, Frank Gordon; "Production of High Energy Particles Using the Pd/D Co-deposition Process," Paper presented at the American Physical Society meeting, March 5, 2007, Denver Colorado.

3b) Lawrence P.G. Forsley, Garry Philips, J Khim, Pamela Mossier-Boss, Frank Gordon, Stanislaw Szpak; "Time Resolved, High Resolution Gamma Ray and Integrated Charged and Knock-on Particle Measurements of a Pd:D Co-deposition Cell," Paper presented at the American Physical Society meeting, March 5, 2007, Denver Colorado.

The above note was submitted to New Energy Times today. Unfortunately, the editor of this online journal, Steve Krivit, rejected the note without providing any explanation. SEE CORRECTION BELOW ! ! !

APPENDED ON 12/24/07
Let me clarify what I meant by the term "rejected." Steve Krivit actually suggested what the note should be improved. I decided not to do this because what is shown above seemed to be sufficiently clear. That is why I decided to post this summary note at my website. But the phrase "without any explanation" was wrong. This will be clear to anyone who reads his message (see below). I apologize for this misleading phrase. I also thank Steve Krivit for sending me copies of two messages shown below; they have been deleted from my mailbox long time ago.

Ludwik to Steve (4/6/07):
Hi Steve,
Many days ago you mentioned that the deadline to submit a piece for the next issue of TNT is April 20. With this in mind I am sending you this piece. I hope you will publish it. Your help in improving my style will be appreciated.

Steve to Ludwik (4/11/07):
Dear Dr. Kowalski,
Please attend to all of these required changes if you wish us to publish your letter.

1. You must explicitly refer to text or data that I wrote in my report regarding calibration curves, not what you think I believe.
2. If you wish to critique what I wrote in my March 16 article, you must do so only with what was known at the time, not what you have learned afterwards, because that would become an extension of the discussion, not a direct rebuttal.
3. If you wish to refer to data or results of experiments in your rebuttal you must include only that which has been presented in a legitimate science conference or journal publication.
4. If you want to discuss pit size, you must provide a scaled image, as well as an image of that area which shows the relative size of such pits that support your distinction of "dominant" pits.
5. In your presentation, you stated your conclusion, "analysis of our single experiment does not support the idea that dominant pits, on our CR-39 chips, are tracks of alpha particles, or less massive nuclear projectiles." In your letter of 6 April you stated it differently, "prominent large pits on post-electrolysis chips cannot possibly be due to alpha particle, to directly emitted protons, or to protons ìkickedî by neutrons." Please explain why, in your presentation, you did not state that the pits could be from more massive nuclear particles. Since your 6 April letter uses the absolute characterization, "cannot possibly be," please provide absolute proof that the tracks cannot be caused by protons kicked by neutrons.

Thank you,

Steve Krivit
Editor, New Energy Times

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322) Rutherford-Bohr model questioned

Ludwik Kowalski; 4/11/2007
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

This morning I received an e-mail message from a stranger, P.J.J. He wrote: ". . . This is a long and clumsy prelude to my quest for your help in determining the validity if any in the posit of the hydreno (not hydrino) theory..... "

Well I never heard about hydrenos. But Google helped me. A book entitled "Lattice Nested Hydreno Atomic Model" by Mark Porringa, is available over the Internet:

http://www.lnhatom.com/products.html

Jumping from link to link I finally found something scientific at

http://www.lnhatom.com/bohrrutherford.html

It is an extract from Porringa's book. What follows are my comments on his objections to the Bohr-Rutherford model of atoms. His words are in blue; my comments are in black.

"..... Aside from its undisputed accuracy in the prediction of the spectral lines of hydrogen, the Bohr-Rutherford atomic model appears to be in need of some serious rework.....Another objection is raised regarding Rutherford's [29] calculation of the size of the nucleus, which could be off by a very wide margin, due to several invalid assumptions including the erroneous exchange of terminology used in his analysis that permitted the "surface" of the nucleus, to be considered equivalent to the "center" of the nucleus. From the outset, Rutherford presumes a point like nature for the nucleus with a dimension conjectured to be less than 10^{-14} m. If the nuclear charge is in reality dispersed over a considerably larger volume than the presently conjectured 10^{-42} m^3, the coulomb interaction with a high velocity charged particle would be much different than Rutherford's presumption. A larger nucleus, with a much reduced electric field intensity, would permit the high energy alpha particles used in the Gold foil experiment to come much closer to the nucleus, possibly striking it."

Yes, this would happen if the nucleus were much larger than 10^{-14} m. and the dependence of the differential cross section on the angle would be very different from what was actually measured in 1911. (Scattering would not be due to Coulmbe force only). I measured differential scattering cross sections many times (not only on gold, and not only for alpha particles) and I know how well the results confirm the point-like approximation. This approximation, by the way, is no longer valid when the energy of the projectile is sufficiently large. The size of a nucleus can easily be estimated from the energy at which a cross section, for example at 150 degrees, starts to deviate from what is predicted by the point-like approximation. I have done this, several times, on the basis of data collected during my postdoctoral work at Columbia university.

" Rutherford also makes the mistake of presuming that the probability of large angle deflections is directly related to the thickness of the gold foil, ignoring the obvious tendency of the alpha particle to be channeled within the highly organized atomic lattice by the presence of the concentrated charge of the gold nuclei, and secondarily by the electron cloud surrounding it. "

I see no mistake in Rutherford's analysis of experimental data. He made an assumption (independent scattering of

alpha particles by individual point-like nuclei) and showed that experimental data, for very thin foils, were in good agreement with this model. What is wrong with this kind of validation?

"The beam trajectory being normal with respect to the foil, and therefore parallel to the internal lattice structure, would effectively prevent large angle deflections beyond the first two staggered layers of the cubic close-packed crystal structure.

A gold foil is not a monocrystal. Therefore the internal lattice structures are not always parallel to trajectories of alpha particles.

Barring a direct hit, or very close miss on a gold nucleus within these first two layers, the high energy alpha particle would simply continue on its merry way between the nuclei, effectively channeled through the entire foil, without any further prospects of high angle deflections. Only small deflections would be possible for the remainder of its travel, and these would be entirely insufficient to bump the alpha particle out of the defined channel provided by the lattice... "

But scattering at large angles does take place. The results, at low energies, are found to be consistent with the idea of independent scattering by individual point-like nuclei. That was the central point of Rutherford's discovery (1911). the only mistake he made, as far as I know, was in an argument (in the 1937 book) that practical applications of nuclear energy will not possible. He did not anticipate a possibility of chain reactions. But that is a different topic.

"The thickness of the foil used in the experiment is cited, near enough, as 10^{-8} m. Given the well-established atomic dimension of 10^{-10} m, the foil was less than 100 atoms thick, making it extremely unlikely that a single alpha particle could experience more than one large angle deflection. The proportion of large angle deflections greater than 90^{o} was found to be only one in 20,000. Following the reasoning above, about half of these deflections happen at the surface of the foil, the remainder occurring at the staggered layer of atoms immediately below the surface of the foil of the ccp structure. "

What evidence is available to support a statement that "about half of these deflections happen at the surface of the foil." ? In my opinion it is a speculation that has nothing to do with reality. All experimental data with which I am familiar, including my own, are consistent with the idea that the above assumption has nothing to do with reality.

"In other words if the foil could have been reduced to only two layers thick the number of high angle deflections would have been essentially the same as that observed for the 100 atom thick foil. The remaining 98 atomic layers would have virtually no effect with regard to high angle scattering. Rutherford's inclusion of a thickness factor in his probability analysis would therefore appear quite unjustified, and would by itself give rise to a 100 fold error in the size of the nucleus..... "

When the target thickness is increased from 2 to 100 layers, the number of high angle deflections increases by the factor of 50. Once again, this is a well established experimental fact.

"Making no presumptions about the relative size of the nucleus, and the nature of the Coulomb interaction, a reasonable estimate could be asserted by considering only the high angle scattering at the surface of the foil, which clearly indicates that only about 1/10,000 of the surface area is occupied by something substantial (a heavy nucleus); the remainder being mostly empty space occupied by the orbital electron clouds. "

Which experimental data are being used to justify this 1/10,000 factor?

"This clearly and simply implies that the diameter of the nucleus could conceivably be roughly 1/100 of the atoms diameter. The result of this "ball park" analysis is that the nuclear radius may be about a thousand times larger than the presently conceded dimension of 10^{-15} m, which results in an atomic diameter in the range of 10^{-10} m, with a nucleus of 10^{-12} m; a much different view from that presently accepted. Even at 10^{-12} the absolute Casimir pressure is still a very substantial 10^{18} kPa and certainly adequate to hold the nucleus together against the substantially reduced

coulomb repulsion of the protons. ”

That reminds me a well known Polish saying: "Gdyby baba wasy miala toby dziadem sie nazwala." And the Russian sayng "Yielsi-by-da dakaby da wortu rosli boby to polutchilsia by nie rot a celyi ogorod". The English proverb, I was told, is "If wishes were horses beggars might ride."

“A thousand fold increase in the nuclear diameter, also results in a million fold reduction in the intensity of the mutual coulomb repulsion of the protons of the rarified nucleus, assuming for simplicity a radial field attenuation, according to the normal $1/r^2$ relation.

Strong nuclear forces have short range. What would keep protons inside atomic nuclei?

Such a dramatic reduction greatly improves the prospects of fusion at low energies and calls into question the present understanding of thermonuclear explosions, which are heavily reliant on the notion of an extremely intense coulomb repulsion within the nucleus....

Yes, thermonuclear reactions would be taking place at lower temperatures if atomic nuclei were much larger than they really are.

Thermal neutron absorption cross-sections cast further doubt on the notion of an extremely small nucleus. Gold 198 and Cadmium 113 for instance have cross-sections approaching 27,000 barns, which on a statistical basis, just so happens to equate to an apparent nuclear dimension of 10^{-12} m, again a thousand times larger than the presently accepted nuclear dimension. “

Very large cross sections (for 0.025 eV neutrons) do not conflict with small sizes of atomic nuclei. Cross sections become very small at 0.1 MeV and above.

“. . . Why is it that no stable isotopes exist for elements having an odd number of neutrons and protons? Why is it that only one stable form exists for over 25 elements while others have up to 10? What is behind the enigmatic chemistry of Nitrogen. Why does the hot fission of Uranium result in unequal fission fragments. How does cold fission frequently occur without radioactivity or the release of the vast amounts of energy characteristic of thermonuclear reactions. Why is it that no one seems to even bother anymore with such fundamental questions?”

Yes, there are many things we do not understand, including cold fusion. That is why being a scientist is so exciting. Rutherford and Bohr are giants on whose shoulders we are standing.

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323) Defending the unit 319 conclusion

Ludwik Kowalski; 4/14/2007
Department of Mathematical Sciences
Montclair State University, Montclair, NJ, 07043

The note shown below was sent, on April 6, 2007, to the Editor of New Energy Times, Steve Krivit. I wanted it to be published in the next issue of that online journal. The last sentence was: “Your help in improving style will be appreciated.” Unlike my previous short submission, described in unit #321, the second note was tentatively accepted by Krivit. But I decided to withdraw it, as explained below. Why do I elaborate on this episode? Because I believe that some readers might be interested.

Addressing our controversy

Ludwik Kowalski; 4/6/2007
Department of Mathematical Sciences
Montclair State University, Montclair, NJ, 07043

In item 10 of the March 2007 issue, referring to my APS report, Steve Krivit wrote: “In Ludwik's case, we have a result that appears positive, yet he purports it to be negative. Neither case is beneficial to science, and neither case should be permitted to go unchecked.” The purpose of this note is to respond to this by describing the outcome of a new experiment. But first let me thank Steve for the last four photos in:

http://newenergytimes.com/Reports/Krivit-KowalskiCR-39Discussion.htm

The photos were made by SPAWAR researchers. The two right-hand pictures clearly confirm my statement about relative sizes of pits. My post-electrolysis chips are again about 2.5 times larger than pits due to our alpha particles, under identical etching conditions. The two left-hand pictures show that for the SPAWAR chips the ratio was close to 1.7. That lower ratio also supports my claim, as shown in:

http://csam.montclair.edu/~kowalski/cf/319galileo.html

The main issue is not the experimental facts (relative sizes of pits); it is their interpretation. That interpretation, as explained at the above webpage, is based on the CR-39 calibration curve of Russian scientists. That curve does not cover the region of very low energies. Steve apparently believes that the CR-39 calibration curve, for alpha particles, continues to go up when E goes down at very low energies. If this were true then I would agree with him and admit that the conclusion was tentative.

But I am a nuclear physicist and I know how density of ionization changes with energies of alpha particles. On that basis the dashed line was drawn in Figure 8 of the above webpage. On April 5, 2007, I performed an additional

experiment; it confirms, qualitatively, that Steve's expectation does not agree with reality. The diameters of pits do not rapidly increase when energies of alpha particles become very small. In that experiment very thin mylar foils were used to reduce energies of alpha particles, in small steps. Chips irradiated with alpha particles of several energies were etched together and their diameters were compared. No rapid increase in diameters was observed at very low energies.

This constitutes an additional validation of my main conclusion -- prominent large pits on post-electrolysis chips cannot possibly be due to alpha particles, to directly emitted protons, or to protons "kicked" by neutrons. Please note that I am referring to prominent pits only. I have good reasons to believe that tracks due to nuclear particles are also present on post-electrolysis chips. But these tracks are not as numerous as pits whose diameters we measured. Will this expectation be confirmed by those who are now addressing the issue? That remains to be seen.

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The reply from the editor came on April, 11, 2007. It was a suggestion that I make some changes, as shown below. Krivit wrote:

"Please attend to all of these required changes if you wish us to publish your letter.

1. You must explicitly refer to text or data that I wrote in my report regarding calibration curves, not what you think I believe.
3. If you wish to critique what I wrote in my March 16 article, you must do so only with what was known at the time, not what you have learned afterwards, because that would become an extension of the discussion, not a direct rebuttal.
4. If you wish to refer to data or results of experiments in your rebuttal you must include only that which has been presented in a legitimate science conference or journal publication.
5. If you want to discuss pit size, you must provide a scaled image, as well as an image of that area which shows the relative size of such pits that support your distinction of "dominant" pits.
6. In your presentation, you stated your conclusion, "analysis of our single experiment does not support the idea that dominant pits, on our CR-39 chips, are tracks of alpha particles, or less massive nuclear projectiles." In your letter of 6 April you stated it differently, "prominent large pits on post-electrolysis chips cannot possibly be due to alpha particles, to directly emitted protons, or to protons "kicked" by neutrons." Please explain why, in your presentation, you did not state that the pits could be from more massive nuclear particles. Since your 6 April letter uses the absolute characterization, "cannot possibly be," please provide absolute proof that the tracks cannot be caused by protons kicked by neutrons."

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Here is my reply to the editor, followed by comments on the above requirements.

1) You are asking too much for a little comment.
2) I do not think that it is worth our time to improve the draft. I will post it on my website.
3) To explain why I decided to withdraw the draft I would like to say something like this:

This piece was submitted as a note to be published in the New Energy Times. The letter was conditionally accepted but I decided to withdraw the submission. The alternative would be to spend more time on it in order to satisfy several preconditions, as explained to me by the editor. . . .

I asked for permission to quote the editor's reply and it was given to me today. The first precondition would be easy to to satisfy. I do not know what the second precondition was. But it does not matter because it was already deleted by the author. Now about the third precondition. No, I do not wish to critique what Steve wrote, I wish to inform readers that new information supports my claim. I think that "an extension of the discussion" is much more important than

pointing fingers at each other. What is wrong with leaning on factor 1.7 which was recently inserted into Steve's March publication? By the way, insertions should be dated. I was not aware that I was addressing an insertion and not the original text. I am glad that SPAWAR people also reported that dominant pits are much larger than pits due to alpha particles.

The fourth precondition points to the policy of the journal. According to <http://newenergytimes.com/news/news.htm>, NET "contains all the latest low energy nuclear reaction news and developments from around the world, as well as meaningful analysis and expert perspective." That makes it an appropriate place to submit a very short note directly related to The Galileo Project. The author of the recent article, the editor himself, wrote about my contribution. That prompted me to collect more experimental data and show that they also support the conclusion criticized by the author. I would also be happy to report that emission of nuclear particles resulting from a chemical process has been identified. Yes, my new results were not published elsewhere because I expected the NET to be the most appropriate place for this. First I posted the result on a list for CMNS researchers, expecting some criticism. No potential errors were identified. That encouraged me to publish the results at my own website. But, after some hesitation, I decided to share them with readers of New Energy Times. After all, I was addressing Steve's NET accusation that "Ludwik's conclusion at the APS conference does not appear to be supported by fact."

As far as precondition five is concerned, I was referring to photos and pit sizes that were already shown in Steve's publication. The last precondition can be split into two parts. What was initially called "less massive particles" is now qualified (by me) as protons that are emitted directly or are kicked by neutrons. Where is the conflict? The idea of "more massive nuclear particles," such as recoils from collisions with neutrons, was introduced by Larry Forsley. Why should I be addressing it while defending a statement about alpha particles and lighter projectiles? The second part of the last precondition has to do with the "absolute proof." I know what a proof in mathematics is. But what is an absolute proof in experimental science? Results of measurements can be more or less reliable but not absolute. The same is true about conclusions based on experimental data.

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Inserted on 4/20/07
The Galileo Project Team #2 posted their results at I know that other GPT reams, and the SPAWAR team, are still conducting their experiments. When will their results be known? Contrary to our results, the team #2 was not able to replicate the magnetic field effect. They did much more work than we did. But they used CR-39 that was purchased 4 years ago. That might be responsible for some differences. It would be better if all TGP teams were supplied with CR-39 from the same batch. Yes, it is easy to be smart in retrospect.

The space below is reserved for comments, if any, on my letter to the editor, and on what I wrote about his preconditions. Please send them to me at:

<kowalskil@mail.montclair.edu> .

P.S.
One does not need thin foils to verify that diameters of pits do not become very large at very small energies. The experiment can be conducted in air by changing the distance between the source and the CR-39 detector, up to about 60 mm. The well known range-energy relation for alpha particles can be used to calculate the average energy at each distance. The only disadvantage of this method, in comparison with the use of thin foils, is that exposure times for long distances must be much longer than for short distances, according to the $1/r^2$ law. This can be a very inexpensive student research project. Here are mean energies at different distances, at STP, for alpha particles of 5.5 MeV

Dist (mm) - - -Energy (MeV):
.0 - - - - - - - - - - -5.5
10 - - - - - - - - - - 4.5

20 - - - - - - - - - - 3.5
30 - - - - - - - - - - 2.3
40 - - - - - - - - - - 1.1
43 - - - - - - - - - - 0.0

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324) Defending my APS conclusion

Ludwik Kowalski; 4/23/2007
Department of Mathematical Sciences
Montclair State University, Montclair, NJ, 07043

1) Unit #323 was an attempt to defend the conclusion that prominent SPAWAR pits could not be due to alpha particles or lighter nuclear projectiles. The defence was based on an experiment whose purpose was to show that diameters of pits due to alpha particles decrease at very low energies. But that was an experiment belonging to the quick-and-dirty category. It was good enough to convince me (that the claim made in unit #319 was valid) but probably not good enough to convince others. A much better experiment was finished today. The results are more or less consistent with what has been reported in 2000 -- the diameters of pits due to alpha particles decrease when energies of alpha particles become samller than approximately 1 MeV. This is illustrated in Figure 1 below.

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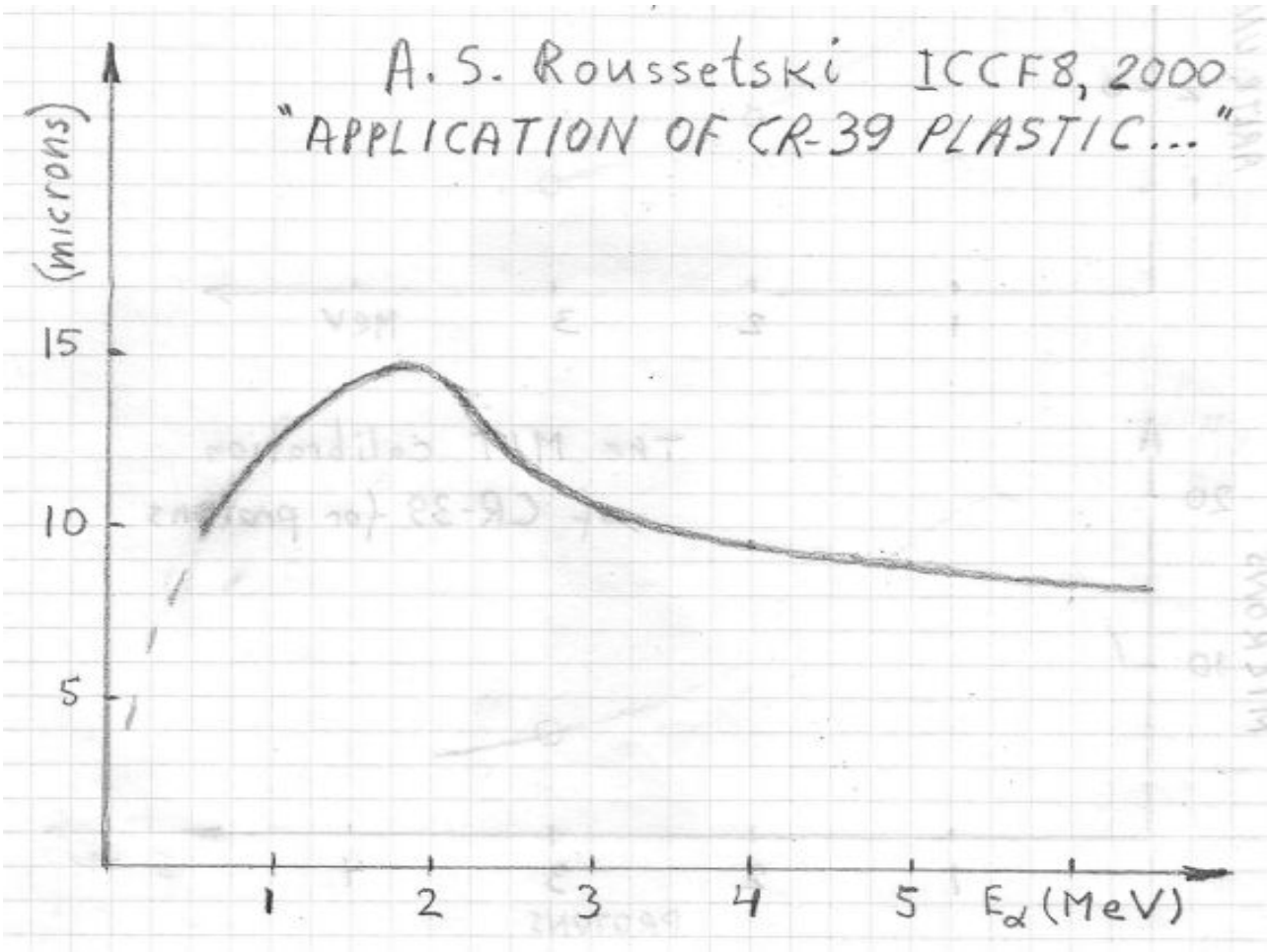

Figure 1
Dependence of pit diameters (in microns) on energies of alpha particles (in MeV), as reported in 2000 (at ICCF8) by

Roussetski. His important report can be downloaded from the library at <http://www.lenr-canr.org>.

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Using our Am-241 source, from a smoke detector, six chips were exposed to alpha particles, one after another. The distance between the source and the CR-39 detector (in air) was different for each chip as summarized in the table below. The exposure times were adjusted on the basis of the $1/r^2$ dependence.

dist (mm)	mean diameter (microns)	st. dev. (microns)	~ E (MeV)
5	8.0	0.53	3.56
10	8.8	0.62	2.98
15	10.3	0.90	2.19
20	11.0	0.74	1.18
25	6.3	1.85	0.20
30	only background tracks could be seen for that distance.		

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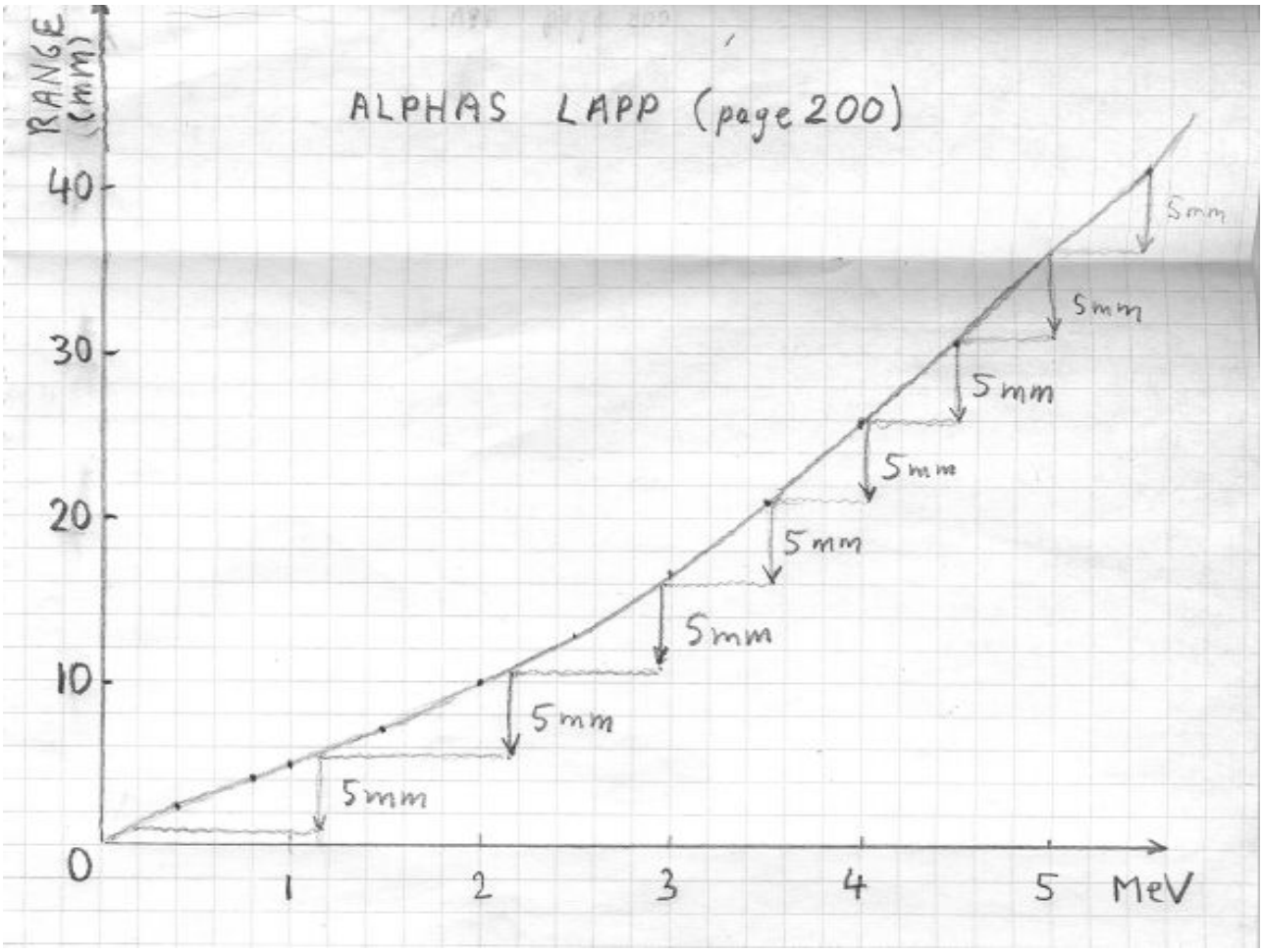

Figure 2
Dependence of the mean range of alpha particles in air (in mm) on their energies (in MeV).

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The next figure shows how the mean diameters of pits depend of the distance in air. The etching was in 6.25 NaOH, at 73 C, for 5.75 hours. For each distance 20 tracks were randomly selected and their diameters were measured. The points in Figure 3 are mean values while the error bars are standard deviations. Note that energies become smaller when distances become larger. Rapid decrease of diameters, at energies below 1 MeV is in very good agreement with the dashed line in Figure 8 of unit #319. That is why I think that this experiment validates the controversial conclusion made at the APS meeting (see unit #319).

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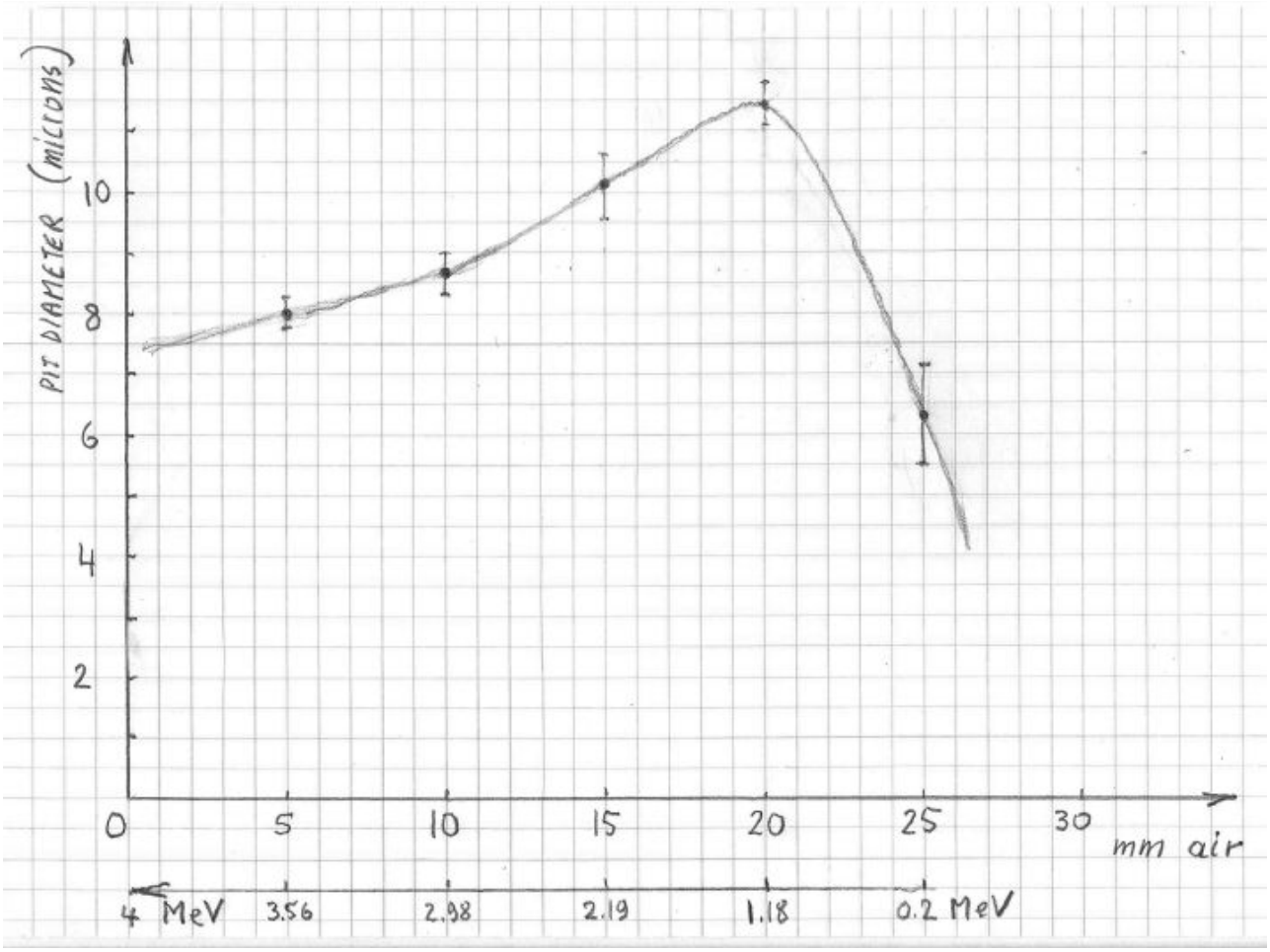

Figure 3
Dependence of the mean diameters (microns) on the distance from the source in air (cm). According to Figure 2, if alpha particles emerging from the source into air had the energy of 5.5 MeV then the maximum diameters would be near the distance of 35 mm. The shallow tracks, in that case, would be seen at 40 mm, and not at 25mm.

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Inserted on 4/24/07:
The following message was posted (three days ago) on the private list for CMNS researchers. I do not know how to interpret the fact not not a single Galileo Project researcher responded so far.

"It is important not to get lost in details. What do we want to accomplish? A claim was made, 18 years ago, that a nuclear activity of some kind can be induced by a chemical process. Many highly qualified scientists confirmed the validity of such claim. They measured amounts of thermal energy produced, they counted light nuclear projectiles, such as alpha particles, they showed that tritium, helium and heavier isotopes were produced [and destroyed] during some chemical reactions. What we need is a protocol for a simple, reproducible-on-demand test --we need an experiment to convince honest skeptics that a nuclear effect can indeed by induced by a chemical process.

The Galileo Project is an attempt to convince ourselves that the SPAWAR effect is an example of CANR (chemically assisted nuclear reaction). As demonstrated in the EarthTech report, based on a large number of experiments, the SPAWAR pits are quite easy to observe. Seven teams, studying the SPAWAR effect at the same time, should produce seven yes or no answers about reality of a particular kind of CANR. So far only two Galileo Projects (beta 2 and beta 5) have been completed. Their answers seem to be negative. When will we know about conclusions reached by remaining teams? The Galileo Project will become a missed opportunity unless all outcomes are reported. Please report the status of your project on this list, even if your work is still in progress."

The Galileo Project Team #2 conducted many experiments and their reseults are posted at <http://earthtech.org/CR39/>

Inserted on 4/24/07:
The corresponding curve for protons has the same general shape as the curve for alpha particles (see Figure 1 above), as illustrated by Steve Krivit in

http://newenergytimes.com/Reports/Krivit-KowalskiCR-39Discussion.htm

It was shown by the MIT team. Experimental details can be found in their report. According to the Internet, that report was published in Rev. Sci. Instrum. 73 (2002) 2597. But I did not check this. I simply went to Google, entered the "A modified accelerator for ICF diagnostic development" into the search box, and clicked on the first link to download the pdf file.

Inserted on 4/26/07:
a) The distances between the Am-241 source and the CR-39 chips were determined by numbers of iron nuts, each 5 mm thick, mounted on each other to support a detector. Alpha particles traveled along a tube formed by nuts. But energies in Figure 3 were estimated by using the range-energy relation. If I had a silicon detector I would measure energies as well. If the energy at d=5 mm were found to be 4.5 MeV, for example, rather than 4.0 MeV, then the energy scale in Figure 3 would changed accordingly. My subjectively estimated uncertainty (the location of 4.0 MeV with respect to d=0) is plus or minus 0.5 MeV.

b) Fortunately, such uncertainty does not weaken the initial conclusion -- pits whose diameters are 2.5 times larger than those due to alpha particles of Am-241 cannot possibly be due to alpha particles or lighter projectiles. Unfortunately, no comments concerning that conclusion have been posted on the CMNS list so far. I do not know how this should be interpreted. Does it mean that most subscribers consider my conclusion to be obvious or does it mean they did not read this unit? The posting of the unit was announced on the list and the URL was given. Steve Krivit was also informed about this unit.

c) Using a silicon detector to measure energies directly would increase the accuracy, in comparison with the method I used. And it would have another advantage; in addition to mean energies the distribution of energies would also be known. The ideal way to calibrate the CR-39 would be to use nearly mono-energetic particles form an accelerator. That approach was described in the 2002 paper of Roussetskii et al., downloadable from the library at http://lenr-canr.org .

d) My simple study was undertaken because I wanted to know what happens to pit diameters at very low energies. I was not aware of the existence of the curve shown in Figure 1 above. The location of the maximum on that Figure does not coincide with the location of the maximum in Figure 3. At least part of the difference is due to limited accuracy of two experiments. Fortunately, my conclusion about copious SPAWAR pits does not depend on the location of the maximum, it depends only on the relative sizes of pits at the maximum, in comparison with sizes of pits due to alpha particles from the Am-241 source.

Appended on 4/27/07:
Hmm, patent protection? If I were motivated by a patent I would unsubscribe from the CMNS list and would not talked at ICCF conferences. The list, as far as I know, consists mostly of people whose motivation is scientific. We are interested in the main CF claim -- a nuclear process, such as emission of neutrons or alpha particles, can be induced by a chemical process, such as electrolysis or diffusion. This is not patentable. The sounding board function is as important to us as the peer review is important to readers of a traditional journal.

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325) Larry's Findings

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The May 10 issue of The New Energy Times, at

http://newenergytimes.com/news/2007/NET22.htm

contains an interview that Steve Krivit had with Larry Forsley. Larry was the first author of a SPAWAR team paper entitled "Time Resolved, High Resolution, Gamma-Ray and Integrated Charged and Knock-on Particle Measurements of Pd:D Co-deposition Cells." That paper was presented at the March 5 APS meeting in Denver, Colorado. Other authors were: P.A. Mosier-Boss, G.W. Phillips, S. Szpak, J.W. Khim, and F.E. Gordon. Larry's talk was based on twelve Power Point slides. Steve's interview was entitled "Charged Particles for Dummies: A Conversation with Lawrence P.G. Forsley." My purpose here is to make some comments on the content of slides, and on what was said in the interview. Please download Larry's slides from the above URL before reading the rest of this piece.

1) SPAWAR results were obtained by using a method of codeposition. That method, invented by Stanislaw Szpak and Pamela Mossier-Boss, has been used in numerous experiments during about ten years. The goal was to gather evidence that a nuclear process takes place during the electrochemical process of codeposition. But methods of detection were different in different experiments. Use of the CR-39 detectors was relatively recent and Larry became one of the partners. The content of Larry's slides is interesting but not everything is clear to me. I do not like the telegraphic style -- inevitable in a slide show -- I prefer traditional papers with illustrations.

2) On the slide #12 Larry states: "Tracks are not of chemical origin, although chemical damage may occur." I am glad that a possibility that some pits might not be due to nuclear projectiles is now recognized. That was the main point of my own APS presentation, as described in the unit #319. And what about the remaining pits? They do look like tracks of nuclear projectiles. Taking this for granted, Larry advances several interesting hypotheses. One of them is massive presence (about 75% of all tracks on the front surface) of low energy projectiles -- protons with energies below 0.5 MeV and alpha particles with energies below 1.5 MeV. These particles are stopped in the mylar foil of six microns. Another hypothesis is about presence of neutrons; they are said to be responsible for the tracks on the back surface.

3) Slide 1 is the title of the talk at the APS meeting. Slide 2 tells us that each field under the microscope is 0.6 by 0.5 mm. Thus, a chip whose size 20 by 10 mm, will have to be photographed 200/0.3=666 times. In practice this becomes 1000 times because some overlapping of fields is desirable. That is why the task of analysis is performed by a computer (TASL scanner) and not by a human being. But the scanner was not designed for very high densities, such as those discovered by the SPAWAR team. The dilemma of "what to count and what not to count" has been addressed and the software was set up to be very selective. Even a shadow of doubt was sufficient to assume that a pit is not a track. Larry found that the numbers of tracks per field, determined by visual inspection, were often ten times larger than what was determined by the TASL scanner.

4) Slide 3 shows range-energy relations for various nuclear projectiles in CR-39. According to these curves, expected nuclear projectiles would be stopped in CR-39 chips. This becomes important in the context of explaining presence of tracks on both sides of detectors. Slide 4 informs us that PACA detectors with mylar were used in a "dry experiment" by SPAWAR team. But, as far as I can tell, most data on other slides refer to "wet experiments," that is to experiments in which CR-39 were in the electrolyte. In the interview Larry tells us that track density is reduced considerably when

mylar film of 6 microns is placed between a wire and the CR-39 detector. It means, according to Larry, that protons with energies below 0.5 MeV and/or alpha particles with energies below 1.4 MeV are emitted from the cathodes. About 3/4 of all identified tracks are, according to Larry, due to these low energy projectiles. What is the difference between green and red rectangles on the right chip of slide 4? I do not know.

5) Slide 5 is not important. Slides 6 and 7, to which the first half of the interview was devoted, are indeed very interesting. They show that three different wires, Pt, Ag and Au were used as cathodes at the same time. For some reason track densities, on the back side of the chip, are different for different wires. Is this because each metal is chemically different or because contacts areas between the wires and the CR-39 chips were slightly different? Later in the interview Larry states that silver is "special." I am not convinced. The most interesting thing on slides 6 and 7 is presence of pits on the back side of the chip. They are more numerous than in the estimated background. That, according to Larry, is a clear indicator that neutrons are also emitted during the electrolysis. But why is the number of neutron-induced tracks, on the back of Ag, much smaller than on the back of Pt and Au? That is not clear.

6) Slide 9 tells us that only one neutron out of many (for example, out of 100,000) is actually detected. Multiplying numbers of pits displayed on the back of the chip (see Slide 6), by 100,000 I am inclined to think that the number of neutrons exceeds the number of charged particles. Is this consistent with the results obtained with two detectors of neutrons during one of the SPAWAR experiments? I do not know. For some reason nothing has been reported about what was learned from neutron detectors. Why is it so? I think that all experimental results should be reported. Another interesting thing, on slide 9, is a suggestion that neutrons of energies larger than 12 MeV can be detected not only via elastic scattering on protons but also via decomposition of C-12 into three alpha particles.

7) Slide 8 show distributions of pit sizes. The U-238 source was probably "thick;" otherwise the distribution of track diameters (actually major axes) would be narrower. Larry shows that dominant frontside pits have diameters between 6 and 16 microns. The dominant backside pits, on the other hand, have diameters ranging from 10 to 22 microns. That is indeed puzzling. Distributions of pit sizes, due to neutrons of various energies (between 1.2 MeV and 19 MeV), are shown on slide 9. They are taken from a 2004 reference. Were the etching condition in the reference the same as in the SPAWAR work? i suppose they were; otherwise comparing what is on the slide 8 with what is on the slide 9 would be meaningless. Larry thinks that large tracks, on the back side of the CR-39 chip, are due to high energy neutrons. That what I would expect if higher energy neutrons were main contributors to the neutron-alpha reactions.

8) Slide 10 compares tracks on the back of the SPAWAR chip with tracks due to neutrons from a Pu-238 source. Larry states that etching conditions were not identical; tracks from the Pu-238 experiment were etched about 25% longer that tracks from the SPAWAR experiment. Taking this under account, he concludes that sizes the largest tracks are roughly the same. Then he addresses the issue of smaller tracks. I agree with him that tracks due to neutrons would begin at different depths inside the chip. That would indeed produce a mixture of tracks of different diameters. In fact that is what I saw, several years ago, when chips irradiated with neutrons from a Pu-Be source were examined.

9) Slide 11 shows preliminary results deduced from gamma rays detected during the electrolysis. Larry identified them as coming from the Sr-92 and Zr-97. These are, presumably, transmutation products resulting from nuclear reactions induced by neutrons. The intensity of gamma ray radiation from the Sr-92 was found to be about ten times higher than from the Zr-97. This, according to Larry, is consistent with what one would expect if the numbers of atoms were identical. But he also mentions missing lines in the recorded gamma ray spectra. This weakens his argument about the presence of the above two radioactive isotopes. An identification is certain when all known gamma rays are present and when the rates of decay matchhttp://csam.montclair.edu/~kowalski/cf/325forsley.html the known values (2.7 and 16.9 hours). Why were the rates of decay not measured? I would make several measurements of counting rates, each lasting about 30 minutes, not 13 seconds. Perhaps they were looking for isotopes with much shorter half-lives.

10) How do SPAWAR results differ from the results of Richard Oriani? The most obvious difference is the number of tracks, and the fact that these tracks are concentrated near wires. Oriani-type clusters would be difficult to identify among the much more numerous pits produced in the SPAWAR codeposition experiment.

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326) Online logbook of a PACA experiment

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Introduction
This unit is the continuation of unit #320; please read unit #320 first. As you know, Oriani and Fisher wrote that the secret of reproducibility, in working with PACA detectors, is in a bit of hardware taken from a successful experiment. Oriani had eight consecutive successes because his cell was preconditioned to reproduce the effect. A success leads another success, he believes. He suggested that I incorporate the O-rings, and the mylar, that were used in his most successful experiment into my setup. These pieces of hardware were sent to me today (1/9/07). This unit will be the electronic logbook for the experiment that I will start very soon. I want it to be open to all who are interested. Hopefully, people will look into my reporting each day and make useful suggestions or critical comments. The experiment will probably last several days; this will be followed by the analysis of CR-39 chips, and possibly by additional control experiments. Entries of this unit will be numbered sequentially.

1) Anticipated protocol:
The goal is to replicate Oriani's experiment I will follow his procedure. The electrolyte will be Li_2SO_4 in light water (concentattion 20 grams per 100 cc). The cathode will Ni (wire whose diameter is 0.25 mm) and the anode will be Pt, more or less as in Figure 2 of unit #320. The exposed length of the cathode will be 2 cm and the constant current will be 25 mA, as recommended by Oriani today. I will have two cells in series; one made of glass (a replica of Oriani's cell) and another made from a polyethylene bottle (as described in the appendix -- see below).

Important insertion (5/1/07):
A serious mistake was made in the experiment described in this unit. The concentration of the Li2SO4 electrolyte in water was 9 times larger than that used by Oriani (it should have been 2.2 grams per 100 cc of H2O). Is this the reason why I was not able to replicate his results? Yes, that is a possibility. The mistake was discovered today, as I was examining my notes critically. The experiment with correct concentration, to be performed this week, will be described in unit #327. The CR-39 chip used in the new experiment will by about 3 by 3 cm.

2) Anticipated control experiments:
The most obvious control experiment, testing the radioactivity of the electrolyte (both as powder and after dissolving it in discilled water) are not necessary because I have already done this one and a half years ago. Alpha radioactivity from glass, from nickel and from platinum were also shown to be negligible. But I will perform these tests again if a significant excess of tracks is found (in comparison with background). I will also check for alpha radioactivity of mylar. Oriani urges me to begin the experiment as soon as possible because the O-ring and mylar might be loosing what is responsible for favorable conditions.

3) About statistical levels of confidence.
Suppose a Geiger counter is used to measure radioactivity from a weak source. One measurement, lasting two hours, gives N1=100 counts when the source is near the detector. Another measurement, also lasting two hours, gives N2=49 counts, when the source is removed. Is the difference 100-49=51 significant? And if so then what level of confidence should be assigned to this significance? That is well known problem. Knowing that distributions of results (if measurements were repeated many many times) are essentially Gaussian we estimate standard deviations as square roots of the numbers of counts. Thus S1=10 and S2=7. To simplify the problem slightly we use the mean standard

deviation, 8.5. The difference of counts, 51, is six time larger than standard deviation. That corresponds to a very high level of confidence. The well known rule is that, for example, the level of confidence is about 99.7% when (N1-N2)/S is equal to 3. According to statistical tables, the level of confidence becomes higher than 99.999% when the ratio is 6.

But that kind of analysis would be questionable, for example, if the electronic threshold of detection were not very stable, or if the high voltage fluctuated during each experiment. Under such conditions the S=sqr(N) would be strongly underestimated. It is conceivable that by repeating each measurement many times one could find that S1=60 and S2=50. Now the mean S is nearly the same as the difference between the mean values of N1 and N2. The level of confidence, in such case, becomes 68%.

A well designed Geiger counter is usually more stables than in the above illustration. But the illustration has a pedagogical value. Suppose that a measurement lasting four days yielded N1=100 tracks in the post-electrolysis chip and only N2=49 tracks in the control chip. I am assuming that chips were etched identically and that their areas are identical. How to estimate standard deviations. This if far from being obvious because of the “count or not to count dilemma. I know from experience that in addition to tracks of nuclear particles the surface of an etched CR-39 chip displays other defects, some of them do not resemble tracks while others resemble them. It is very difficult to be consistent about what to count and what not to count. This is equivalent to a Geiger counter fluctuating threshold of detection.

Ideally, the values of N1 and N2 should be determined by several, for example ten, equally trained people. This would allow one to calculate mean values of N and corresponding standard deviation. The alternative, which I will use, is to count how many times, N’, the “to count or not to count” hesitations were encountered. A typical example might be N1=100 with N’=60 and N2=49 with N2’=20. In that example I would assume, arbitrarily, that S1=60 and S2=20. Note that the mean S’=40 is not very different from the N1-N2=51. Also note that N1’ and N2’ would probably not be very different if the N1 were much larger, for example, 300. In that case the ratio of (N1-N2)/S’ would be 251/40=6.3 and the level of confidence (believing that the effect is real) would be very high. All this simply illustrates the obvious, the value of S must be estimated and the (N1-N2)/S must be large to be certain that the effect is real.

I know from experience that S is often not very different from N2/2. Taking this for granted, one has the following table for the levels of confidence:

99.7% confidence when N1/N2 =2.5
95% confidence when N1/N2 =2.0
68% confidence when N1/N2 =1.5
0% confidence when N1/N2 =1.0

In other words, under specified assumption (to be verified each time) the number of tracks in the post-electrolysis chip must at least twice as large as on the control chip. Otherwise the level of confidence (in claiming that the effect is real) becomes questionable. In a much more favorable case, when the “count or not to count” dilema is faced in only 10% of counting for N2 (instead of 50%, as above) one has:

99.7% confidence when N1/N2 =1.3
95% confidence when N1/N2 =1.2
68% confidence when N1/N2 =1.1
0% confidence when N1/N2 =1.0

The above is true when only the “count or not to count” uncertainty is responsible for random fluctuations of experimental results. And only when results are qualitatively reproducible. Random fluctuations contribute to limited precision while systematic errors, for example personal bias in dealing with the “count or not to count” dilemma, contribute to limited accuracy. It is very difficult not to be biased when one knows what is at stake. The way to avoid personal bias is to have an assistant who does not know what is better, too many tracks or not enough tracks. Fortunately, I am trying to replicate experiments in which N1 was always at least ten times larger than N2. Therefore I do not have to worry about gray areas in which statistics becomes intolerably sadistic.

4) Experimental setup

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The geometry of cell #1 is essentially the same as shown in Figure 2 of unit #320. But geometry of the cell #2 is slightly different. The bottom of the cell is the bottle cap. Inside that cup is one CR-39 chip (area 2 cm2); it is protected from the electrolyte by a mylar film. As in the cell #1, the anode-cathode system, shown below, is rigid. It stands on the nickel cathode (a foot), supported by CR-39, below the mylar film. A large copper ring fits loosely inside the bottle. The nominal thickness of my mylar, 6 microns, corresponds to the surface density of 0.84 g/cm2 (based on the mylar's density of 1.4 g/cm3). This is small in comparison with ranges of alpha particles of 2 and 6 MeV (1.10 mg/cm2 and 5.14 mg/cm2, respectively, according to "Charged Particles tracks in Polymers" by R.P. Henke and E.V. Benton, 1966.)

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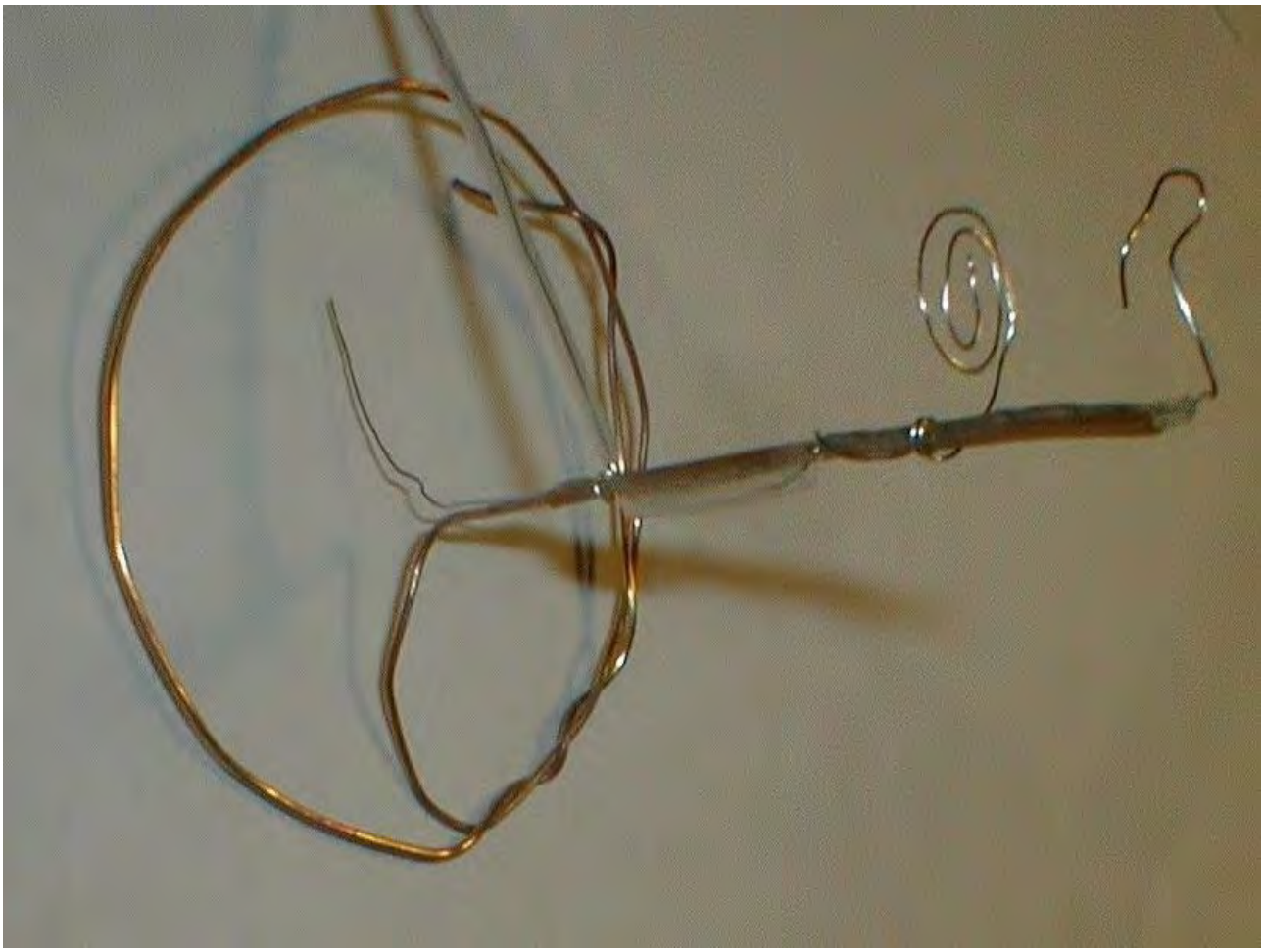

Figure 1

The rigid cathode-anode setup for the cell #2. The end of the nickel wire, near the upper right corner, is going to be the foot, standing on mylar above the CR-39 chip. Only 2 cm of that wire is outside the heat shrink. The other end of the cathode wire will comes out of the heat shrink above the electrolyte. A thick copper wires also inside the heat shrink. But it is not in contact with the electrolyte. The purpose of this wire is to provide rigidity, in the form of the large circle, inside the bottle, The spiral platinum anode is mounted on the heat shrink column; its distance from the cathode (foot) is 22 mm. Diameters of the Ni and Pt electrodes are 0.25 mm.

.....

5) It is 4/12/07; I was ready but . . .

The two O-rings that Oriani sent me arrived today. So I started preparing cell #2 (made from the bottle). I am using a new mylar film. Here are labels of my CR-39 chips:

1) Chip below the mylar film (the cathode wire is on top of it) -- 8810900
2) Control chip (blank) in the next room -- 8810888
3) Chip outside the cap -- 8810896
4) Chip outside the cap (below it) -- 8810901

5) Chip outside (on the conical part of the bottle -- 8810891
6) Chip outside (about 60 mm above the cathode)

The cell has been positioned and connected to the cirquit. But it is still empty. Then I started preparing the cell #1 (replica of Oriani's cell). The mylar film has dark spots on it, probably some kind of dirt from the experiment in which it was used. At that moment I decided not to start the experiment today. I took another CR-39, chip 8810899, and wrapped the mylar around it. It would be a big mistake not to check for alpha radioactivity on mylar foil. I know that Oriani already did this; but it is a good idea to make another check. I will start the experiment tomorrow, without etching this last chip. It will etched with all other chips, probably in one week or so. It will exposed to Oriani's mylar for only 12 hours. The mylar in cell #2 is not going to be tested for alpha radioactivity before the experiment. I can do it later if a lot of tracks are found.

6) Observations during the electrolysis:
a) The CR-39 that was exposed to Oriani's mylar was put into another room (where the balnk chip is kept). Am-241 was used to irradiate a chip (half of it through one layer of mylar). The cell #1 has only two chips, one about 2 mm below mylar and another on the outside glasss wall. The center of that chip is at the level of the anode.

b) The electrolysis stareted, with two cells in series, at 13:45 of 4/13/07. With the current of 25 mA the potential differences were 4.49 V and 4.17 V, on cells #1 and #2 respectively. Bubbling of hydrogen and oxygen is not very intensed.

c) One hour later. All is about the same. Below is volt-amp lines for my two cells. They were taken at about 2:00 p.m. I suppose that the common intercept, at about 3 volts has something to do with work functions; two cells are chemically identical. Small differences in slopes are are probably due to differences in geometry inside the Li_2SO_4 electrolyte.

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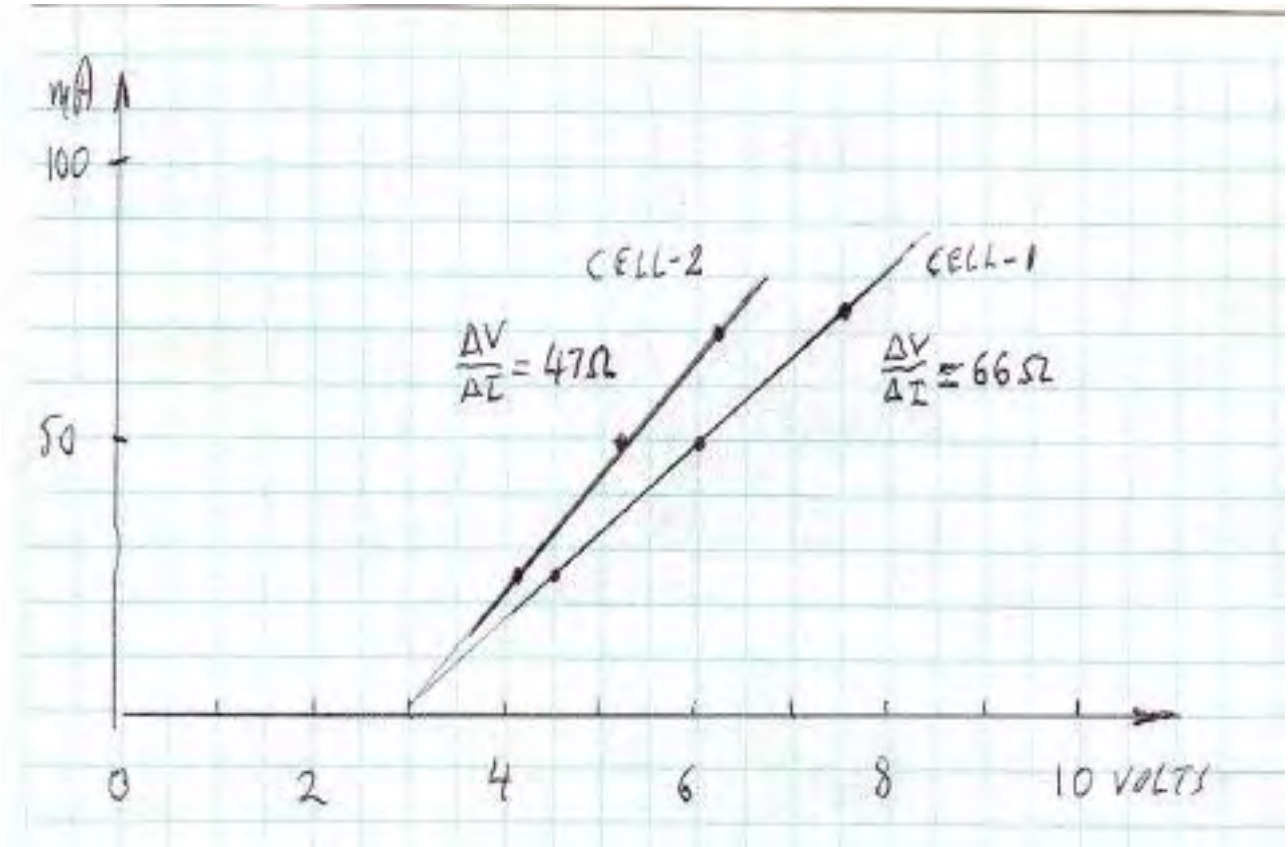

Figure 2
The volt-amp lines for my two cells.

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d) 4/14/07: I forgot to mention that no electronic current stabilization is used in this experiment. But everything is very stable. Loss of water has been small so far. Here are the values from my notebook:

4/13/07 --18:00 -- 25 mA, -- 4.44 V and 4.08 V
4/13/07 --21:00 -- 25 mA, -- 4.42 V and 3.98 V
4/14/07 - - 5:30 -- 25 mA, -- 4.49 V and 4.05 V
4/14/07 --10:30 -- 25 mA, -- 4.43 V and 4.02 V
4/14/07 --12:00 -- 25 mA, - 4.38 V and 4.05 V ..... adding 1 cc of water to each cell
4/14/07 --12:30 -- 25 mA, - 4.43 V and 4.02 V

My milliammeter is analog and fluctuations of up to one or two mA might remain unnoticed. The current 25 mA is used because it was Oriani's last recommendation.

e) An observation: Tiny bubbles continuously rise up, like a cigarette smoke, toward the surface. Occasionally large bubbles appear. I have seen this many times before. But my cell #2 is very wide and I discovered something peculiar. Big bubbles, above the spiral Pt anode suddenly, yes very suddenly, stop rising and turn toward the wall of the bottle, or toward the heat shrink tube. It seems that they are attracted toward the plastic maescape from the electrolyte at once, like tiny bubbles?
terial. After traversing at least 1 cm of the electrolyte (sometimes 2 or 3 cm), the bubbles stick to the surface and remain there for long time, for example an hour or more. Their sizes are between 1 mm and 3 mm. At any given time I can see at least 20 or 30 bubbles. Why large bubbles are formed during the electrolysis? Why do they not
g) It is 8:00, Monday, 4/16/07. No significant changes in the current and voltages. The cell #2 needs more water to compensate for more evaporation from its much larger surface, in comparison with the cell #1. Losses due to electrolysis are probably identical because the same current flows through two cells.

h) It is 23:00, all as before. My notebook has many nearly identical lines, like those on the first day.

i) It is 10:20, Tuesday, 4/17/07. Oriani suggested to run the experiment for four days. That means I have about 3 more hours. But I am tempted to add another day, at much larger current. This will not destroy tracks that are already on The chips. The only disadvantage will be loss of information about when the tracks were formed. But that does not matter. At this stage I just want to see a lot of tracks with my own eyes. I would not mind performing several additional experiments, one for each current, if this two-in-one experiment produces the same result as Oriani's experiments.

j) Tuesday, 4/17/07; last line before going to the higher current.

4/17/07 --9:30 -- 25 mA, - 4.42 V and 3.98 V (total duration nearly 4 days, 92 hrs)

k) Increasing the total voltage to get higher current
4/17/07 -- 9:44 -- 150 mA -- 10.03 V and 8.17 V
4/17/07 -- 10:12 -- 150 mA -- 9.34 V and 7.86 V
4/17/07 -- 10:30 -- 150 mA -- 9.50 V and 7.79 V
4/17/07 -- 16:40 -- 168 mA -- 9.60 V and 7.93 V
4/17/07 -- 17:00 -- 150 mA -- 9.36 V and 7.35 V
4/17/07 -- 17:45 -- 150 mA -- 9.86 V and 7.42 V (Stop electrolysis. 92+8 = 100 hrs)

Note that 8 hrs at 150 mA --> 48 hrs at 25 mA (same number of coulombs).

7) Dismounting the cells.
a) The CR-39 below mylar were found to be dry below each cell. That is VERY GOOD.
b) In cell #1 (glass) only a small part of cathode (perhaps 3 mm) was in contact with mylar because the foot was nos not horizontal.
c) The cathode in cell #2 was nearly horizontal. That means the contact between the cathode and the mylar was probably on about 10 mm of the wire.

d) Visual examination of chips (before etching) did not reveal anything abnormal.

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Figure 3
Cell #1. Two O-rings and the 6 micron mylar film (received from Oriani) are located between two parts made from glass. Cell #2 is not shown because it is just a polyethylene storage bottle with a removed lower part, as described above. The coin leaning on the cell has the diameter of 25mm. Let me add that glass components of Oriani cell can be ordered from: http://www.kimble-kontes.com/html/pg-671750.html The inner diameter of tubes are close to 16 mm.
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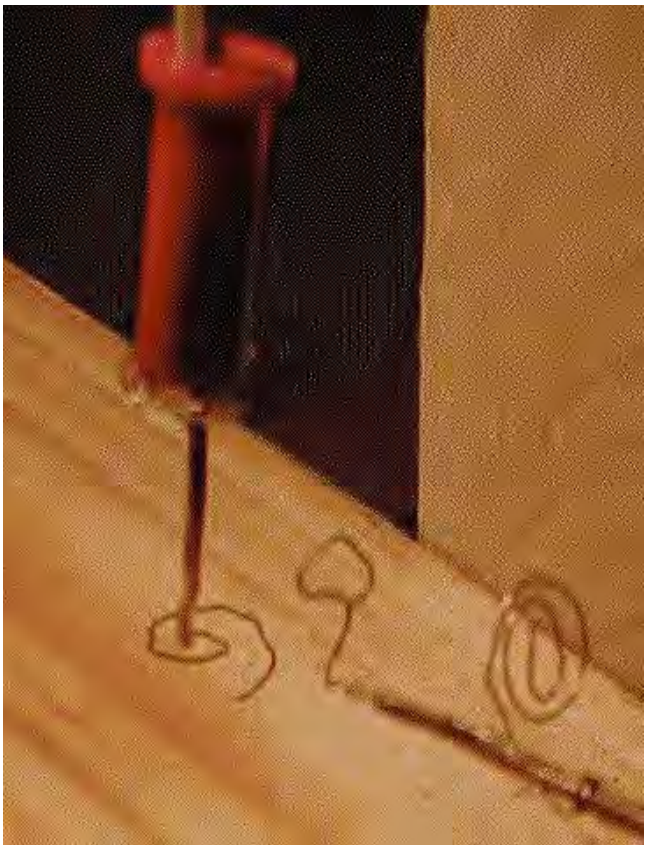

Figure 4
The rigid cathode-anode setups for two cells. The setup from the cell #1 is on the left (vertical) while the setup from the cell #2 is on the right (horizontal). Spiral anodes can also be seen.

8) Etching:
(NaOH at 6.25 N, temp ~73 C) 6 hours.

9) Results:
a) Executive summary: Nothing above background.

b) Detailed results of counting:
Chip exposed to Am-241 -- numerous tracks, as usual
Chip 8810-0888 (blank, 1.8 cm^2) 9 tracks on the labeled side and 4 on the other side.
Chip 8810-0899 (15 hrs exposure to Oriani's mylar, 1.8 cm^2) 5 tracks on the labeled side and 4 on the other side.

Chip 8810-0888 (PACA chip, 0.9 cm^2, below mylar cell #1) 4 tracks on the labeled side and 7 on the other side.
Chip 8810-0900 (PACA chip, 1.8 cm^2) below mylar cell #2) 11 tracks on the labeled side and 8 on the other side.

Chip 8810-0898 (1.8 cm^2, chip outside cell #1) 5 tracks on the labeled side and 4 on the other side.
Chip 8810-0891 (1.8 cm^2, chip outside cell #2, lower) 4 tracks on the labeled side and 14 on the other side.
Chip 8810-0885 (1.8 cm^2, chip outside cell #2, higher) 8 tracks on the labeled side and 8 on the other side.

10) Discussion:
I do not know why my three consecutive attempts to replicate Oriani's results were not successful. Something essential must be present in his cell but in my cells. How else can this be interpreted? Will other TGP researchers, still conducting experiments, be more successful? That remains to be seen. For some reason the CMNS list for cold fusion researchers is silent about TGP experiments performed in March and April.

11) Inserted on 4/19/07:
From a private telephone conversation with Oriani I heard about another spectacular result. He is now investigating the effect of another parameter. Meanwhile he asked me to put the CR-39 between the O-ring for several days. I started this experiment today.

12) Appendix :
The easiest way to describe my polyethylene cell is to reproduce a message that I posted several days ago, on a private list for CMNS researchers. Here it is:

POSITIVE RESULTS WITH A DIFFERENT BOTTLE ! THANKS KEVIN.

On Apr 5, 2007, at 11:06 AM, Ludwik Kowalski wrote:

2) In a private message to me a TGP researcher wrote: "Ludwik, Thanks for suggesting this very low-tech way to make a cell with Mylar protecting the CR-39 in the screw cap of a vitamin bottle. Upon having a repeatable result, this could be a simple setup for others to duplicate without machine tools or laboratory glassware. Have you leak-checked it yet?" Well, the test performed yesterday turned out to be negative. Water did manage to leak (during the night) into the space that was expected to remain dry. Perhaps the cap was not screwed strong enough. I will try again, also with water (and with dry tissue instead of CR-39). Please join me in the effort to offer a simple setup for students and teachers. Weeks ago Scott reported on using the epoxy glue. That is worth trying.

Here is my original description:
a) Take a small plastic bottle with a screw-top lid. I am experimenting with a bottle in which vitamins are sold in the USA.
b) Using a knife, remove the bottom from the bottle (the created opening will become the top of your cell).
c) Place the CR-39 detector into the lid.
d) Cover the detector with the six-micron-thin mylar foil (~ 5 by 5 cm) and screw the bottle into the covered lid.
e) Gently pour the electrolyte into the bottle. The electrolyte will not be in contact with CR-39.
f) Mount the electrodes (more or less as in Figure 2 mentioned above) and start the experiment.

I mentioned the 2-HDPE label on the bottle. Francis Tanzila commented: "2-HDPE is a grade of high density polyethylene and the white pigment is probably TiO_2. These should be fine with Li_2SO_4 but I would look for an un-pigmented plastic bottle just to be safe."

The second test (a better bottle) turned out to be positive. Here are details, for those who might be interested:

a) The vitamin bottle cap used in the first test was wider than the cup on the new bottle -- inner diameter 45 mm versus 29 mm.
b) The first cap takes only about 150 degrees to either screw or unscrew it. The second cap takes slightly more than 360 degrees.
c) The second cap has an O-ring-looking plastic rim at the bottom, to seal the bottle very well. That built-in plastic O-ring was probably the most important contributor to my success.

The material is "high density polyethylene" but without pigment. It is a standard 500 cc bottle used by chemists; the inner diameter 70 mm. Such storage bottles can be purchased from any supplier of chemical hardware. I used the same mylar sheet that failed in the vitamin bottle test. First I had a column of water of 5 cm. I emptied the cell after 6 hours and found that the mylar-protected tissue was dried. Then, using the same mylar sheet, I increased the height of the water column to 9 cm. This test was also positive, after another 6 hours. The 500 cc bottle is probably too large for our purpose. But that what they use in our chemistry lab.

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327) Oriani's effect in Phase 2

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1) Introduction:
This unit is continuation of what was in unit #326. In what follows I will assume that the reader is familiar with the content of units #320 and #326. About a week ago I asked myself about what to do next. The idea to abandon TGP work did come to my mind. But that would not be consistent with my goal -- to reach a clear positive or negative conclusion about the SPAWAR claim. The only conclusion I reached so far is that prominent SPAWAR pits cannot possibly be attributed to alpha particles, or lighter projectiles. That is only a small fraction of pits produced during the electrolysis. Oriani claims that some smaller pits are tracks of nuclear particles. He told me that nineteen consecutive experiments convinced him that a nuclear effect is induced during the electrolysis. Several days ago he had only seventeen results, as shown in the table below.

That table was posted on the private list for CMNS researchers. Column A shows the experiment number and column B shows which cell was used. Column C shows the current or currents (in mA) used in the experiment. Column D is the duration of the electrolysis, in hours. Columns E and F show track densities. The first track density (column E) refers to the CR-39 surface that was facing the mylar and the electrolyte The second track density (column F) refers to tracks on the opposite side of the CR-39 chip. The area, on each side of the chip, over which tracks were counted was close to 10 cm^2. Thus numbers in columns E and F must be multiplied by 10 in order to estimate the actual number of counts on each side of the CR-39 chip. According to Oriani, tracks are often clustered; the numbers in columns E and F refer to mean track densities.

The first eleven experiments were made by using the CR-39 in which the mean background density was 26 tr/cm^2 (st.dev. 12). The last six experiments were performed by using the CR-39 in which the mean background density was 12 tr/cm^2 (st.dev. 6). The last column, G, shows the signal to noise ratio -- ratios of numbers from columns E and F divided by mean background densities. A symbols cb indicates that the surface was coated by a layer of blue plastic that stops alpha particles of up to 10 MeV. The symbol D (experiment 15 and 17) indicated that a small container with heavy water was located below the cell and the detector. In experiment #3 the chip was lost before the actual counting. The >100 and >100 are estimates based on preliminary quick examinations.

A	B	C	D	E	F	G
		mA	hrs	tr/cm^2	tr/cm^2	sign/noise
1	S	.2 to 25	168	209	163	8.0, 6.3
2	S	.1 to 45	120	144	120	5.5, 4.6
3	S	12,41,100	101.5	>100	>100	>3.8, >3.8
4	S	70,117,143	67	162	96	6.2, 6.7
5	S	30,75	96	267	289	10.2, 11.2
6	S	50	97	75	39	2.9, 1.5
7	S	5,10,25,50	96.5	151	75	5.8, 2.8
8	S	10,25,75,100	94	120	81	4.6, 3.1

9	S	300	65(?)	48	42(cb)	1.8,	1.6
10	B	10,25	98	74	22(cb)	2.8,	0.8
11	S	12,28	97	234	40(cb)	9.0,	1.5
12	S	20	95	35(cb)	78	2.9,	6.5
13	B	27	95	--(cb)	778	*,	64.8
14	S	20	94	43	104	3.6,	8.7
15	B	19	94	97	323(D)	8.1,	26.8
16	S	40	94	26(cb)	87	2.2,	7.2
17	B	39	94	264	200(D)	22.0,	16.7

These results are really impressive. After talking with Richard on the phone I decided to follow his new protocol in order to obtain similar results. Following Oriani's footsteps I will again use a PACA (protected against chemical corrosion) CR-39 detector. Note that three previous attempts to use these detectors yielded negative results. Our first PACA detector was used in the SPAWAR electrolyte, our second PACA detector was used in Oriani's electrolyte, as described in the unit #320. The third experiment was described in unit #326. Unfortunately, due to an error, the Li2SO4 electrolyte, used in the third experiment, was about ten times more concentrated than Oriani's electrolyte. That might be a reason for failure to replicate Richard's results. Another reason for the failure might have to with the small size (only 0.9 cm^2) of our CR-39 detectors located below the mylar inside the cell tube.

Time zero; starting the experiment (13:00, May 1, 2007):
The initial concentration of the electrolyte is the same as in Oriani's cells (2.0 grams of Li2SO4 in 100 cc of distilled H2O). I am using a 3 by 3 cm CR-39 chip that Richard sent me. The current is 41 mA; it is kept constant with the TL317T operational amplifier, as described in the unit #319. The potential difference on the cell is 6 volts. I know from experience that fluctuations of the cell current are small when a constant difference of potential is applied to it. That is why I think that using the TL317T is not essential in this experiment. I am using the operational amplifier because I have it. The cell has been operating for three hours.

Time=22 hrs (11:00, May 2, 2007):
Oriani suggested that I stop adding water to compensate for losses. He allowed the concentration of the electrolyte to increase during each experiment. The rate at which the level of the electrolyte is going down is not high enough to reach the anode in four days. The inner diameter of the cell is close to 16 mm.

Time=32 hrs (21:00, May 2, 2007)
All is stable. Let me describe the control experiment, also in progress since yesterday. The second large CR-39 chip, sent to me by Richard, has been squeezed between two O-rings far away from any electrolytic cell. The blue protective cover has not been removed from that CR-39 chip. I just want to have experimental evidence that pressure alone cannot be responsible for clusters of pits on both CR-39 surfaces. The control experiment O-rings are pressed toward each other by two flat wooden plates (using four small bolts with nuts). Both large chips will be etched at the same time in the same solution. I forgot to mention that one little corner (about 2 mm^2) of my experimental chip has been exposed to alpha particles from the Am-241 source. That will allow me to compare sizes of pits, after identical etching conditions.

Time=45 hrs (10:00, May 3, 2007)
I noticed that the the cell potential is no longer 5.96 V; it is 5.86. V. At least part of this is likely to be due to changes in the room temperature (probably by no more than 5 C). But this is also consistent with the fact that the concentration of the electrolyte (ion density) increases. The potential would not change if water was added to keep the concentration constant. The current is constant because I am using the TL317T stabilizer. Otherwise the current would go up and I would have to lower the potential, to bring the current back to 41 mA. That is the advantage of the stabilizer. But such

advantage would be only important if keeping the current at exactly the same value were important. Oriani's data (see the table above) seem to indicate that the effect does not depend on the current. The cell potential is nothing else but I*R, where R is its electrical resistance, for a given ion density.

Time=53 hrs (18:00, May 3, 2007)
The level of the electrolyte keeps going down and the cell potential decreases; it is 5.75 V now.

This morning, while cleaning my digital desktop I discovered a piece of text that was composed before Oriani's table (see above) was posted. I simply forgot about this draft; it should have been appanded to unit #326. It describes control experiments performed after my unsuccessful attempt to replicate Richard's results with a small CR-39 chip. Note that in the experiment described in unit #326 only mylar was squeezed between the two O-rings sent to me by Oriani. That CR-39 chip was not squeezed, it was resting on a solid support, about 2 mm below the mylar film. I wrote:

"After learning about my negative results, Oriani asked me to apply the CR-39 to his O-rings for 4 days. He also asked me to take the CR-39 that was below his mylar (in the experiment described in unit #326) and etch it again. After four days the three chips that were in contact with O-rings and the previously etched chip were etched for 5.75 hours.

(a) Chip that had 7 and 4 tracks after 6 hours of etching, had 14 and 7 tracks after additional etching. I think that this is not significant because of the frequent "count-or-not-to-count" dilemma.

(b) Chip #1 (2 cm by 0.5 cm) applied to O-rings; 8 pits on one side, 5 on the other.
(c) Chip #2 (2 cm by 0.5 cm) applied to O-rings; 3 pits on one side, 6 on the other.
(d) Chip #3 (2 cm by 0.5 cm) applied to O-rings; 8 pits on one side, 6 on the other.
(e) Chip #4 (2 cm by 0.5 cm) blank; 2 pits on one side, 4 on the other side.

In other words, additional etching did not reveal more tracks than the first etching. In all cases there were nothing but usual background. These results convinced me that Oriani's was correct in claiming that O-rings were not contaminated by something alpha-radioactive."

Time=58 hrs (23:00, May 3, 2007)
About 25% of water has been lost (electrolysis + evaporation), The cell potential is 5.65 V.

Time=68 hrs (9:00, May 4, 2007)
The cell potential is back to 5.86 V. That is not what I expected. The current remains the same as it was at time zero.

Time=76 hrs (17:00, May 4, 2007)
The cell potential is 5.54 V. The current remains the same as it was at time zero. I suspect that presence or absence of a large bubble, just below the anode, is responsible for fluctuations of the resistance of the cell. Bubbles are sometimes grow slowly below the anode, before escaping. I did not pay attention to this before.

Time=90 hrs (7:00, May 5, 2007)
The cell potential is 5.56 V.

Time=95 hrs (12:00, May 5, 2007)
The cell potential is 5.38 V. In one hour it will be four days. Instead of finishing at that time I plan to do the following.

(a) Disconnect the current stabilizer and collect data on the mA versus volts.
(b) Add water, to find our exactly how much was lost in four days.
(c) Collect data on current versus volts for the original concentration.
(d) Restart the electrolysis at higher current, for example 100 mA, till tomorrow.
(e) Etch both CR-39, post-electrolysis and control.

What else is worth doing? I will examine the chips next week.

Time=97 hrs (14:00, May 5, 2007)
The cell potential is 5.57 V. I am going to perform a, b, and c, as described above.
It is now 14:10 and I am at point d (see above). The current is 160 mA and the cell potential was 16.2 V at the first moment. But now it is 13.2 V while the current is 150 mA. It fluctuates, probably due to bubbles.

The amount of water lost in 4 days was about 2.75 cc. Taking the Volts-Amps data showed that things are not simple. For some reason the curve seem to be nearly the same at both concentrations. I am not sure because of the hysteresis effect -- coming to 10 V from below does not give the same current as coming to it from above. I do not want to deal with this; it is not my area. Roughly the R is about 90 ohms. But this seems to depend on many factors. Fortunately, it does not matter. I will keep the current more or less constant by manually adjusting the power source voltage.

Time=~0.5 hrs (14:30, May 5, 2007)
All seems to be going on smoothly. I=160 mA at 12.5 V

Time=2 hrs, 16:00; 5/5/07
All is fine, only small corrections of voltage (up to about 10%) have to be made from time to time. If I do not make them then the current fluctuates slightly. But this is OK; Oriani does not claim that the effect occurs at specific currents. But he said that the experiment with I=300 mA did not produce good results. Recalling this, I decided to lower the current. At t=2 hrs the potential difference was reduced to 8.10 volts and the current became 80 mA. This is only two times larger than the current I used in the first 4 days.

Time=6 hrs, 20:00; 5/5/07
Current 80 mA, potential 8.02 Volts.

Time=18 hrs, 8:00; 5/6/27
Current 85 mA, potential 8.3 Volts. I will bring the current back to 80 mA now. The increase of current was qualitatively consistent with a significant loss of water, about 20% or so.

The increase of current was qualitatively consistent with a significant loss of water, about 20% or so. This morning I informed the CMNS list that my experiment will end this afternoon. In that message I asked for advice on what else should be done before this to maximize the amount of information. Here is a reply from Scott Little. My only suggestion is that you take care to handle the CR-39 from this experiment very gently. It is surprisingly easy to make tiny scratches in the surface of CR-39 and scratches will often produce pits upon etching. We will be starting our version of this same experiment early next week.

That is great; nothing is better than a situation in which several laboratories are performing the same experiment. That is the only way to find hidden truth, and to validate a scientific claim. Hidden truth about “cold fusion” does not depend on our wishes. It exists and scientists try to discover it. Then they try to validate what has been discovered, according to well established rules. I think we are following these rules correctly. We are not pseudo scientists, as many believe. We know how to distinguish wishful thinking from objective reality. The rules of the game have been established long time ago and most of us are simply following them. Pseudo scientists are those who refuse to learn about current investigations conducted by qualified researchers, not those who are trying to either confirm or refute various claims.

Why did I put quotation marks around cold fusion? Because we have no evidence what kind of nuclear process we are trying to validate. Fusion of two independent atomic nuclei is only one mechanism. That is why the term cold fusion is now often replaced by CMNS (condense matter nuclear science), LENR (low energy nuclear reactions) and CANR (chemically assisted nuclear reactions). My preference is CANR because the central issue is, as it has been since 1989, reality of a strong nuclear process triggered by a much less strong chemical process. If reality of such process is established then scientists will try to understand it. I would not be surprised to learn that there are different “cold fusion” phenomena. Replicability is essential at this stage of the game. That is why I am so happy that Scott and Marissa are also going to perform the same Oriani’s experiment.

Time=20.5 hrs, 10:30; 5/6/07

The current is still 80 mA, potential 7.96 Volts.

Time=24 hrs, 14:00; 5/6/07
The current is 80 mA, the potential is 7.95 Volts. The electrolysis will be ended in two hours.

Time=26 hours, End of electrolysis.

The current was 80 mA, the potential was 7.97 V. The total electrolysis time was four days at 41 mA, one day at 80 mA and two hours at 160 mA.

I will continue reporting in this unit.
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1) Etching of two large CR-39 chip (post-electrolysis and control) started at 16:15. Before etching the corner of the control chip was irradiated with alpha particles from my Am-241 source. Two scratches were made near that corner, on the same side that was exposed to alphas. Following Scott's advice, I skipped the cleaning with the toothbrush step. I know it does not create scratches but . . .

2) Etching (in NaOH 6.25 N) took 6 hours. The temperature fluctuated between 73 and 75 C; most of the time it was at 74 C. Tomorrow I will go to school and examin the chips.

3) The first thing I did was to count tracks on the control chip. My fear was that I might discover them. That would indicate that an artifact (mechanical pressure) is responsible for Oriani's successes. There were twenty of them, he told me today. I was afraid because CR-39 chips are usually not used under pressure, just as they are normally not used in electrolytes. The result was reassuring. I counted 8 pits on one side of the chip and 16 on the other. These numbers were highly inflated because I was very very generous in deciding what to count as pits. By my usual criterion the count would be 3 and 6. The chip area was 9 cm^2 and this amounts to 2 tr/cm^2 or 0.5 tr/cm^2. No matter how I count the result is much lower than the expected background (mean=12 and st.dev.=6).

On that basis I decided to start another experiment; what was the control chip became the experimental chip. The new experiment started at 18:15 today (5/7/07). Marissa Little also received large CR-39 chips from Oriani, plus the O-rings and mylar. These were used by him up to last Saturday morning. They also started the experiment today. I am using the same cell, the same O-rings, the same mylar, and the same electrolyte as in the previous experiment. The current is again 41 mA; the cell potential is 6 V (the power supply potential is 15 V. Will the results will be equally spectacular? I will probably know next Monday.

4) After examining the control chip I started to examine the post-electrolysis chip.

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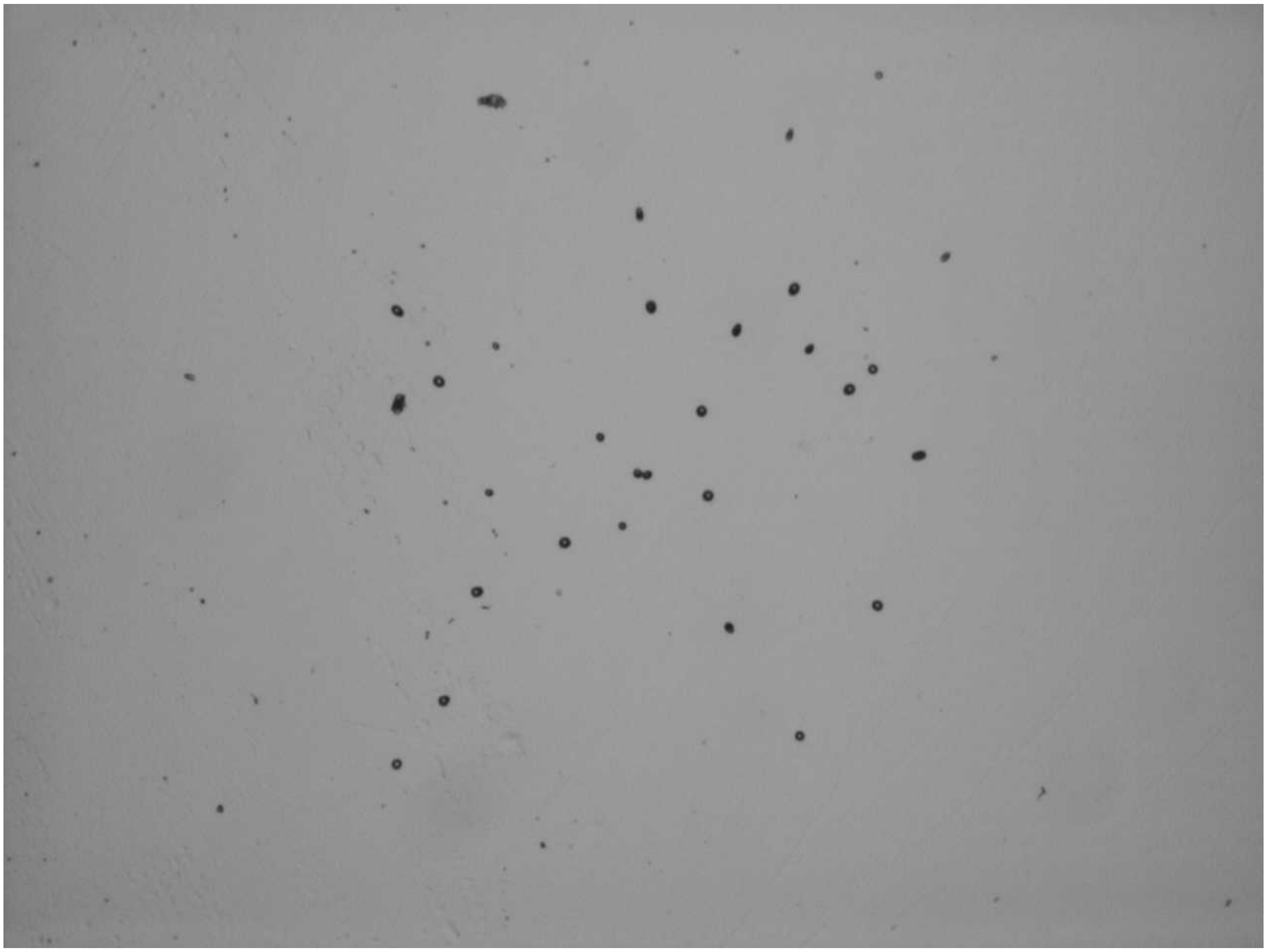

Figure 1
A small cluster of tracks. Magnification 40. The photographed area is about 1.3 mm by 1.0 mm
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The figure above is a cluster of 29 tracks which could be due to alpha particles, and several smaller tracks that can be due to protons. What is remarkable is that the cluster is surround by fields without tracks. The mean track density, over the area of Figure 1 is close to 23 tr/mm^2. This is 2300 tr/cm^2. The track density in the surrounding area is only about 6 tr/cm^2. The next figure shows tracks due to alpha particles from the Am-241 source, under identical etching conditions.

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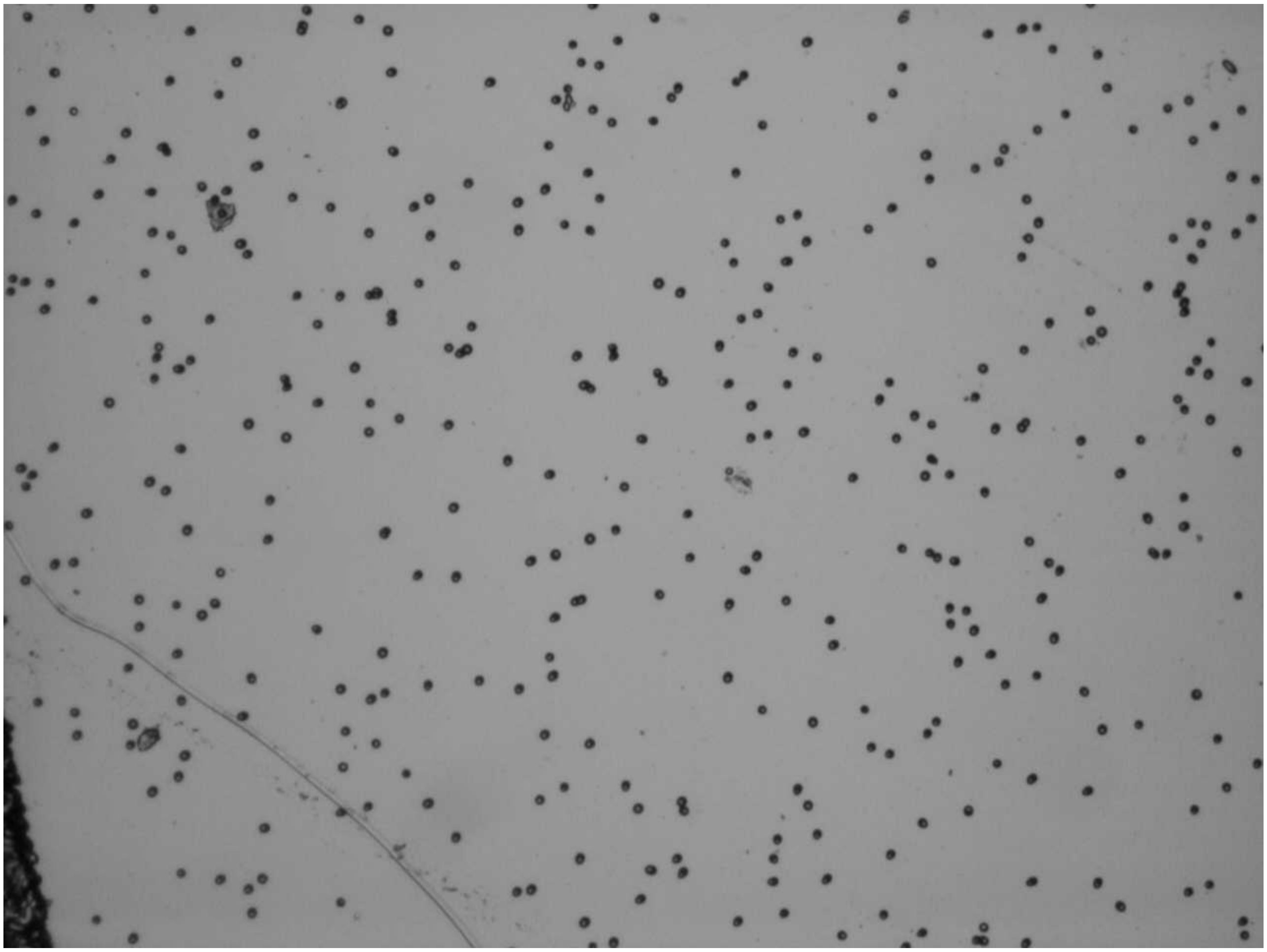

Figure 2
Tracks due to alpha particles. Magnification 40. The photographed area is about 1.3 by 1.0 mm

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A much larger cluster is shown in Figure 3 and Figure 4. In fact, tracks in the lower part of Figure 4 also appear in the upper part of Figure 3. The area of this larger cluster is nearly 2 square millimeters.

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Figure 3
A larger cluster. Magnification 40. The photographed area is about 1.3 by 1.0 mm

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In that case many tracks have considerably smaller diameters than tracks on pervious figures. Furthermore, tracks on Figures 3 and 4 seem to be more shallow (as revealed by the effect of focusing).

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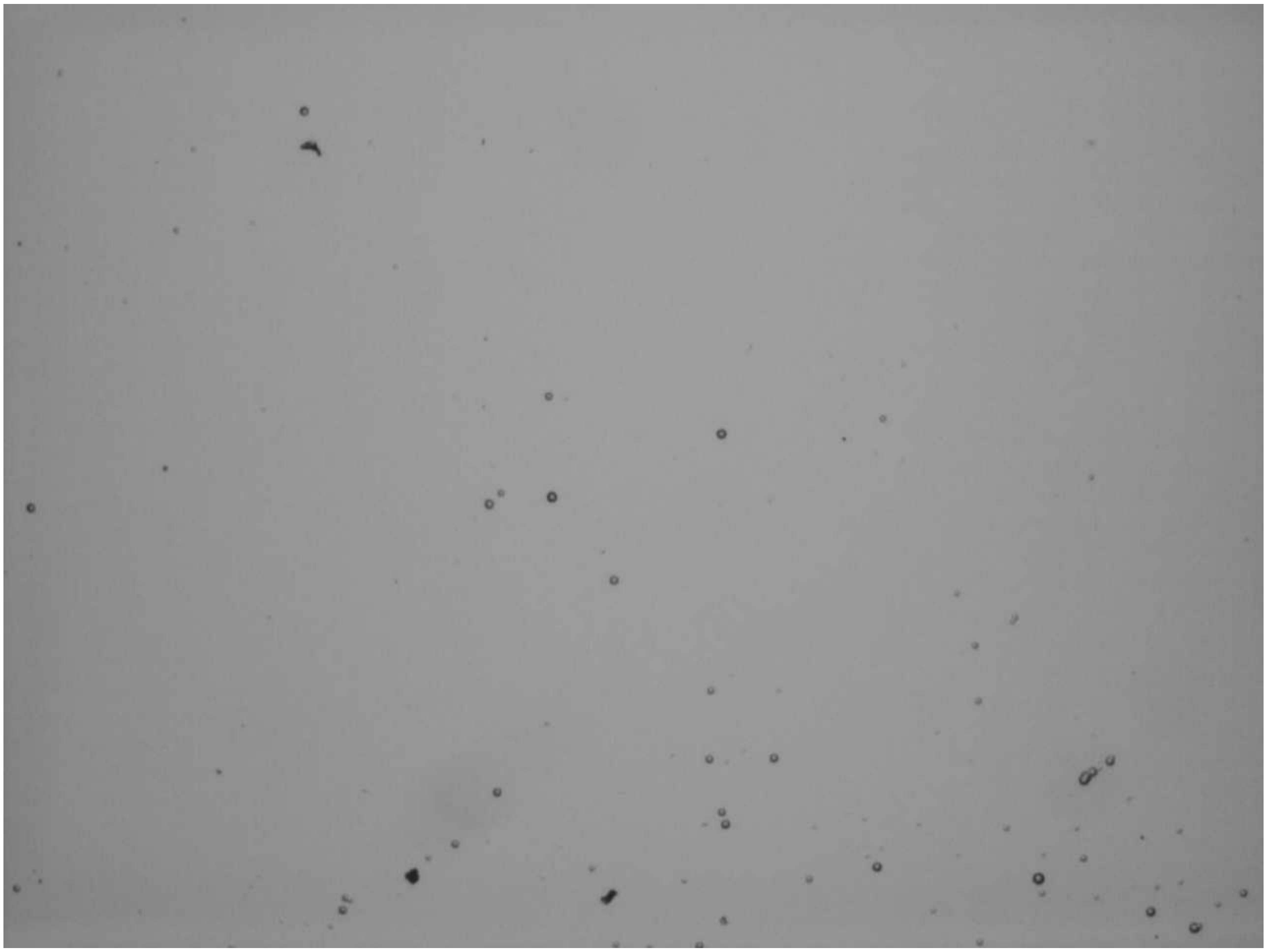

Figure 4
A small cluster of tracks. Magnification 40. The photographed area is about 1.3 mm by 1.0 mm. This figiure shows what is above Figure 3.

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The next figure shows a spectacular cluster of tracks that seem to be due to nuclear particles moving from a point source. Oriani already reported similar clusters on the private list for CMNS researchers. Someone might think that tracks are due to cosmic rays. What kind of cosmic ray collision can produce such a tiny cluster? How frequent are such collisions? Perhaps some cosmic rays experts will answer these questions.

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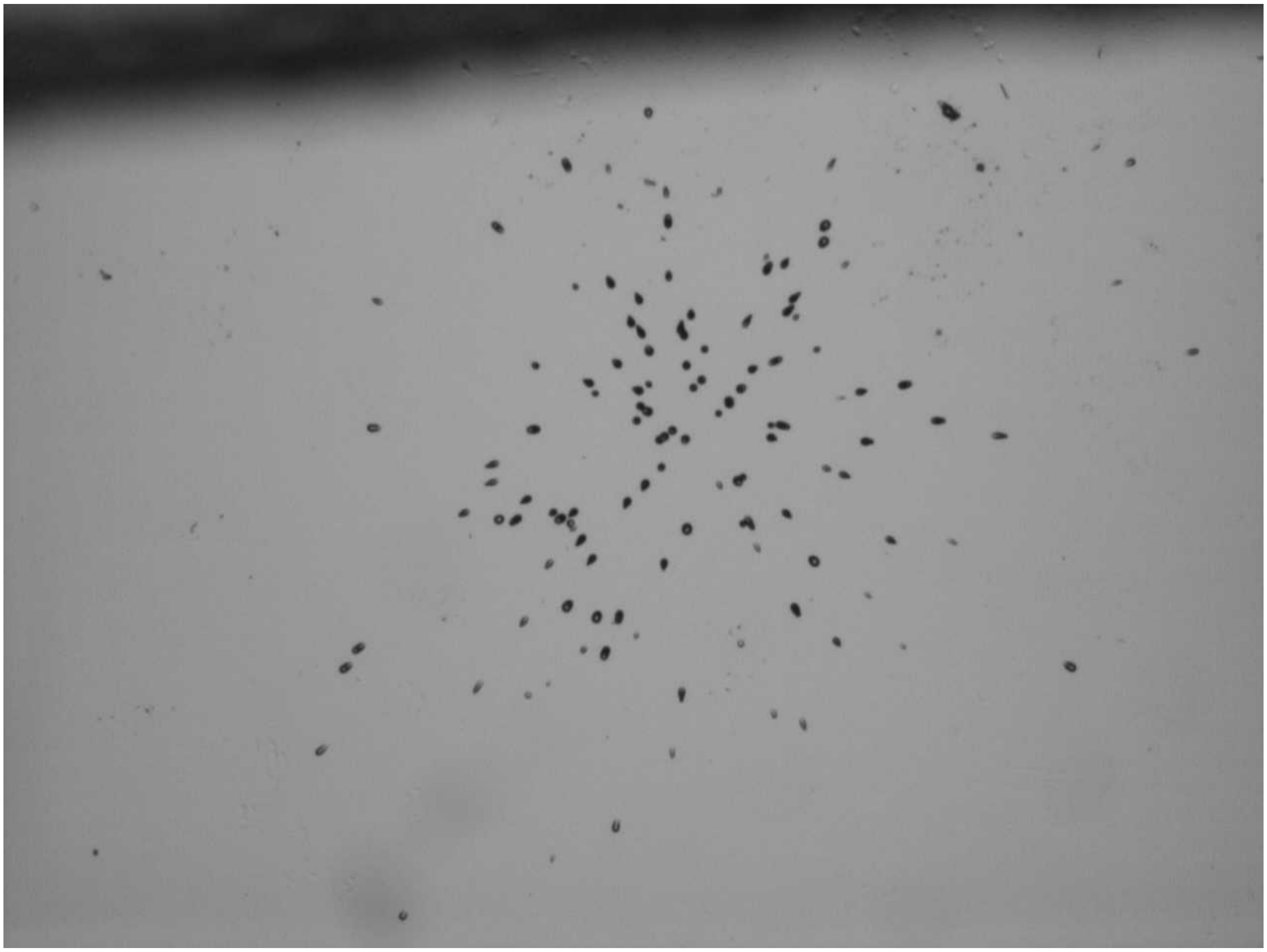

Figure 5
A small cluster of tracks. Magnification 40. The photographed area is about 1.3 mm by 1.0 mm Some of the tracks of this figure are also shown in Figure 6, at much higher magnification.

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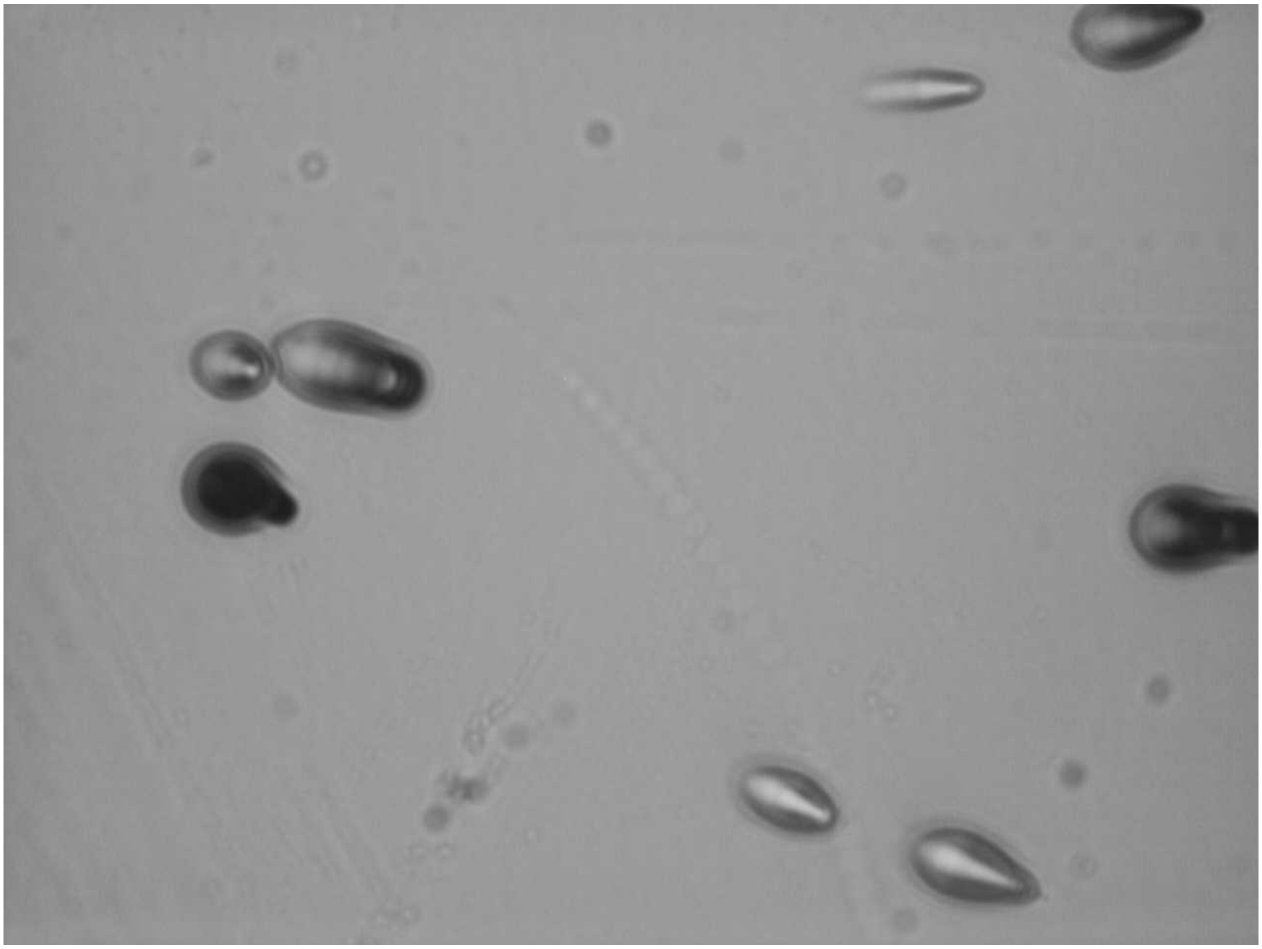

Figure 6
A small cluster of tracks. Magnification 400. The photographed area is only about 0.13 mm by 0.10 mm. Tracks seen here belong to the cluster shown in Figure 5. In fact, one can identify them in Figure 5 by looking along the "one o'clock direction" from the center on the cluster. More than one half of track in Figure 5 are elliptical.
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How can there be any doubt that most of tracks on my figures are indeed due to nuclear projectiles? Oriani told me today that he now has twenty consecutive successes. My first experiment seems to confirm the idea that a starter of some kind (seeding, kindling, etc.) does play an important role. My attempts to replicate his results failed without a starter and succeeded with the starter. Three cascades in one experiment ! Let us see what the next two experiments will bring, my experiment and Marissa's experiment. Marissa is a very young researcher; that is very important, considering ages of most CMNS researchers.

5) In a private message a nuclear physicist wrote: "Cosmic rays cannot be responsible for small clusters. A number of arguments can be used to support this opinion. Here is one of them. CR-39 chips are widely used as dosimeters. That application would be impossible if the number of tracks observed in only 100 hours was as large as on your pictures." I agree; dosimetrists would discovered small cosmic ray clusters long time ago. Note that my control CR-39 chip, of same size, that was etched at the same time, showed zero clusters. In fact, it had practically no tracks at all, as described in unit #327. Also most of the 1.3 by 1.0 mm areas, away from my three clusters, are practically without tracks (less than one track per field, on the average).

6) On the CMNS list Scott Little wrote: "Your cluster is wonderful. It closely resembles a photo Richard shared with us a while back. Frankly, it looks to me like the result of placing a small particle of radioactive material on the CR-39 for a while. There are hundreds of tracks which all appear to originate from a common point. How could a single high-energy particle (i.e. cosmic ray) produce such a shower?" Primary cosmic rays contain relativistic heavy ions. But such ions are very rare, I think. Collisions between relativistic heavy ions, such as gold, have been studied intensively at BNL (Brookhaven National Laboratory) for at least ten years. My understanding is that nuclear projectiles produced

in such collisions have much longer ranges than particles responsible for tracks in Figure 6. .....BNL is only two hours away (driving) from my home, and I have friends working there. It would not be difficult to consult with them. But first we need more data.

7) Time=15 hours, 9:15, 5/8/07 (Second experiment started at 18:15 on 5/7/07)
All is fine with the ongoing experiment. I=41 mA, cell potential is 5.93 volts. I will keep this current till the end of the experiment.

8) Time=27 hours, 21:15, 5/8/07
I=41 mA V=5.97 V

I forgot to mention something very important; the cluster shown if Figure 5 is located at edge of my CR-39 detector. That edge was actually in air, outside the cell. Why do we put the cathode in contact with the CR-39 chip? Because that was in the SPAWAR protocol. Richard Oriani modified that protocol by placing a very thin mylar film between the cathode and the CR-39 chip. The purpose was to protect the chip from a chemical attack of the electrolyte; hence the acronym PACA: Protection Against Chemical Attack. The film was sufficiently thin to transmit alpha particles.

This morning I read Oriani's and Fisher's papers presented at ICCF10. Their clusters were observed above the electrolyte, in the escaping gasses, as illustrated on page 568 (of the conference proceedings). That was several centimeters away from the cathode. Below is their Figure 2 (from page 571 of the ICCF10 proceedings). The upper photo is the chip before the electrolysis while the lower photo shows the same chip -- exactly the same location -- after the electrolysis. What a difference! A surface deffect near the lower left corner proves that the same area is being photographed.

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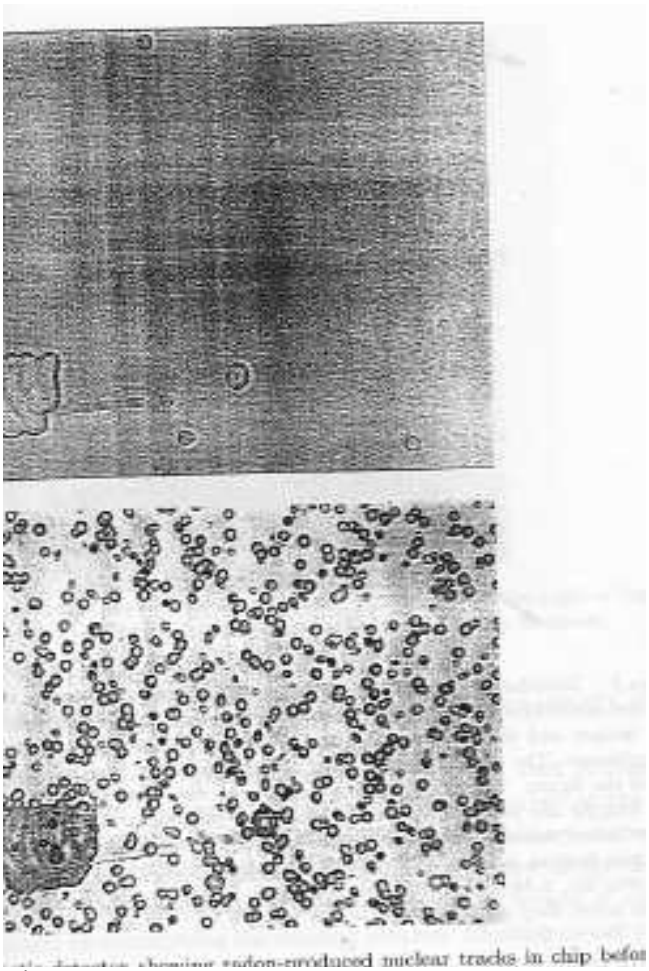

Figure 7
An illustration from the 2003 paper of R.A. Oriani and J.C. Fisher.

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9) On the basis of Oriani's table (which was posted before his additional successes, and before my one replication) I tend to believe that experiments are reproducible. Taking this for granted I just decided to run my experiment for much longer than five days. In a reproducible situation, an experiment that is run ten times longer should produce ten times more cascades, subject to usual statistical fluctuations. I do not have another large CR-39 chip to start another experiment. Is there any good reason for me to stop after five days?

And here is another reason for very long experiments. A success, according to Fisher and Oriani, is highly unlikely, unless "a starter" is provided. We do not know what the starter (seed, NAE, etc.) is. But we know that it is in the O-rings and/or in mylar from a successful experiment. We use it as kindling to start another fire. We also do not know how long the kindling will remain effective. Therefore, for the sake of future experiments, we must keep the fire going on continuously. In other words, running very long experiment, one after another, is desirable, till a reliable "igniter" is invented.

10) How solid is the evidence that kindling is essential? To answer this question one should perform several consecutive experiments with new O-rings, mylar and the electrolyte. Would five or more such experiment produce negative results? If so then the essential effect of a starter (or a combination of starters ?) Would be established. My failures to replicate Oriani's results without a starter might be due to the fact that the CR-39 chip was too small and not in a right location. After all, the central part of my large CR-39, the one with three clusters, has no tracks in the central region.

11) The beauty of Oriani's method -- squeezing the entire sandwich (mylar + CR-39) rather than squeezing mylar only -- is its mechanical strength. The electrolyte is no longer supported by mylar; it is supported by the entire sandwich. I can push down the anode-cathode setup without being afraid to puncture mylar The only precondition for this is that the cathode wire, the foot, should have no sharp points. In fact, I pushed that setup down several times, to make sure the cathode is really in contact with the sandwich. Fortunately, no complications are introduced by squeezing the CR-39 between two O-rings.

12) Time = 37 hours, 7:15, 5/9/07
I=41 mA, V=5.73 volts.

13) One scientist on the CMNS suggested a theoretical explanation of what is going on. That interpretation is not at all obvious, or easy to defend. Responding to this, also on the private list for CMNS researchers, I wrote: What is the best strategy to convince mainstream scientists that CANR are real? I think that the issue is worth discussing. My advice would be to ask all theoreticians not to inject theoretical interpretations until facts are recognized as real. Remember what happened in 1989. Instead of focusing on real experimental facts (generation of excess heat) discussion quickly shifted to theoretical considerations, such as coulomb barrier, expectations based on wrong models, etc. It would be much better if the F&P phenomenon was called UEH (unexplained excess heat) rather than CF (cold fusion), until the reality of UEH were recognized by all scientists.

If it were up to me I would recommend focusing on our new experimental facts. Let us agree that clusters of tracks are not due to artifacts, such as radioactivity or cosmic rays. Let us agree that clusters are likely to be due to unexplained nuclear projectiles (UNP). Then let us try to convince others that UNP are real. Trying to mix experimental facts with theories might backfire again. We want people to look at our experimental data; we want them to perform experiments; we do not want the debate to shift toward the as-yet-unaccepted ideas.

I know it is a touchy issue. Theoreticians do not want to be told what to do, what to publish and how long to wait. And we all believe that pure empiricism is not science. Theoretical debates are essential. But, like other powerful tools, theories can have both positive and negative effects. I am afraid that premature theoretical considerations can produce more harm than good at this delicate stage. What do other people think?

14) Time = 52 hours, 22 :15, 5/9/07
I=41 mA, V=5.47 volts.

15) John Fisher informed the CMNS list that he also replicated Oriani's experiments. He tried three times and each time he saw clusters. In two of his experiments small CR-39 pieces (probably 2 by 1 cm) were entirely in air, at the level of O-rings. He is going to send me one of his chip and I will make pictures of his clusters. Inspired by this, I just placed four small chips (each of 2 by 1 cm) on little wooden pedestals, also at the level of O-rings. Each chip is along a diagonally of my large CR-39, which has been under the electrolyte for 52 hour (when four additional chips were positioned). Each of these four chips has a scratch on the facing up surface. The distribution of chips is shown in Figure 8 below.

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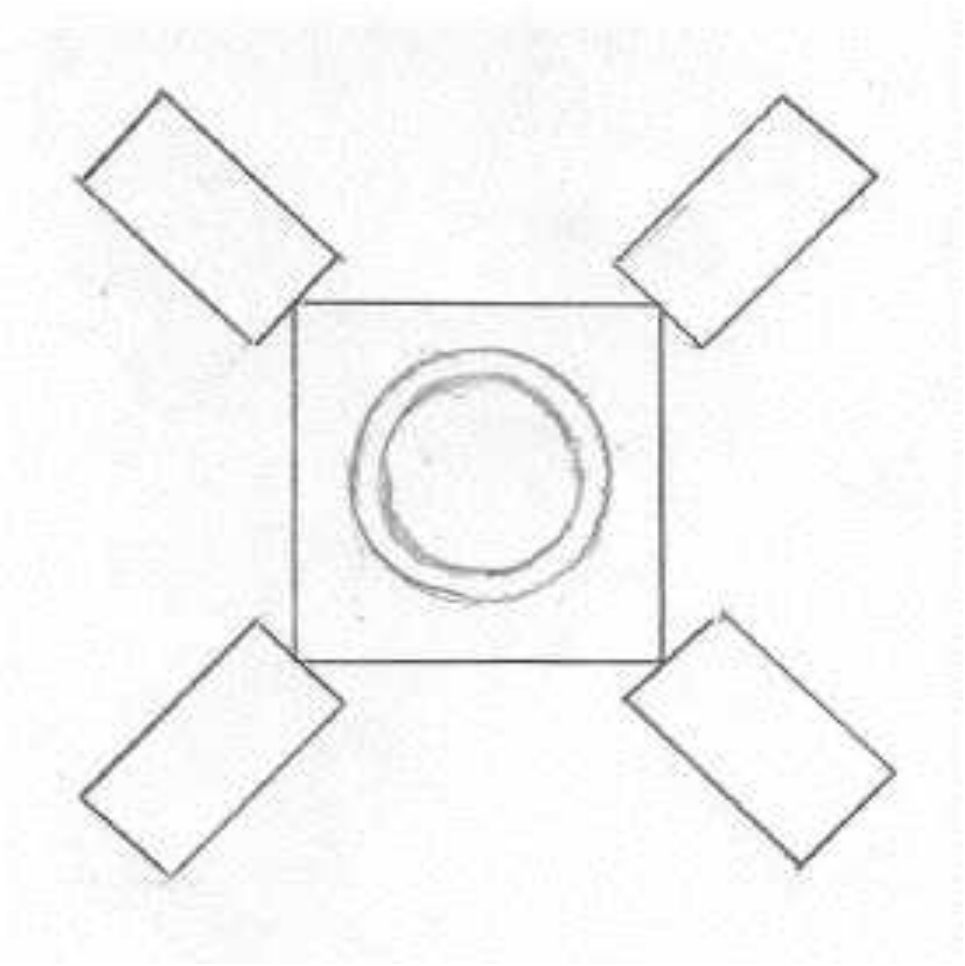

Figure 8
Configuration of five chips in the second experiment. The cirle represent the O-rings, the large square represents the 3 by 3 cm CR-39 chip, and diagonally oriented rectangles represent four smaller CR-39 chips (each 2 by 0.9 cm). Note that the total CR-39 area is nearly doubled. Small detectors are supported by pieces of wood. They are in the same plane, approximately, as the large rectangular detector.

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16) Time = 72 hours (3 days), 18:15, 5/9/07 I=41 mA, V=5.43 volts.

17) Today my large post-electrolysis chip (9 cm^2) was examined again. Here are some additional observations and suggestions: a) Three clusters were observed in an experiment that lasted 5 days. That corresponds to the detection rate of 3/5=0.6 clusters per day. Let me assume that 0.6 is typical. How many cascades would be observed, after 5 days, on a chip whose area is only 0.9 cm^2, as in my unsuccessful experiment? The answer is 0.3. This might be an explanation why my experiments described with small chips were not succesful. To replicate Richard's clusters I should have used the detector of the same size he used. My suggestion is that Marissa runs her experiment for much longer that the anticipated 92 hours. How can this hurt? Tracks created during the first 92 hours would not go away.

b) It is desirable to report numbers of clusters, and to show that areas surrounding clusters have no tracks. That is likely to be more meaningfull than mean track densities, as in Oriani's table above.

c) The cluster shown in Figure 3 and 4 is much larger than clusters shown in Figure 1 and 5. But it does not contain many times more tracks than smaller clusters. Is this typical? We need more data.

d) It turns out that the large cluster is on the side of the CR-39 chip that was facing down (away from the electrolyte). Larry would say it was on the “back side.” Two smaller clusters, on the other hand, were on the side facing up (toward the electrolyte). Is this a coincidence? We need more data.

e) Also note that all pits in the large cluster (a) are mostly round, (b) have a broad distribution of diameters and (c) appear to be more shallow than in the two smaller clusters. Is this a coincidence? We need more data.

f) Richard performed twenty experiments. I would very much like to know if his observations about clusters are similar to my observations.

g) From the public relation point of view, displaying pictures of clusters is highly desirable. The more the better, at this stage of the battle.

d) If the mean number of cascades is 3, and if the distribution of results is Poissonian, as in the case of Geiger counts, then the probabilities, P, for the number of n clusters (in 5 days on a 9 cm^2 chip) are as follows:

```
n = 0,    1,    2,    3,    4,    5,    6     etc.
P = 0.05, 0.15, 0.22, 0.22, 0.22, 0.10, 0.04, etc. (the tail where P-->0)
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In other words, good replicability is not expected in experiments lasting only 5 days, even for 9 cm^2 chips. About 5% of results are expected to be failures. We need either a large number of short experiments, like those performed by Oriani, or a small number of much longer experiments. The second seems to be less demanding.

18) Time = 92 hours (four days) 18:15 5/11/07
I = 41 mA, V=5.26 V. About 40% of water was lost; the level is about 1 cm above the anode. I will replace the lost water tomorrow.

19) Time = 107 hours 9:15 5/12/07
I = 41 mA, V=5.15 V. About 50% of water was lost; the level is about 0.5 cm above the anode. It is time to replace lost water. I did this and the potential difference on the cell became 6.19 V. A correlation between the concentration of the electrolyte and the cell resistance is obvious. But the potential difference did not decrease by the factor of 2 when the concentration is doubled. It is clear that the measured potential difference is not only I*R.

20) Time = 144 hours 18:15 5/12/07 (end of day 6):
I = 41 mA, V=5.88 V.

* *

21) What follows are five microphotographs from the CR-39 that John Fisher sent me. Each picture covers an area of 1.3 mm by 1.0 mm (magnification 40). The photos would be much more informative if they could be compared to pits due to alpha particles of known energy under identical etching conditions. Unfortunately, a source of alpha particles was not available to John.

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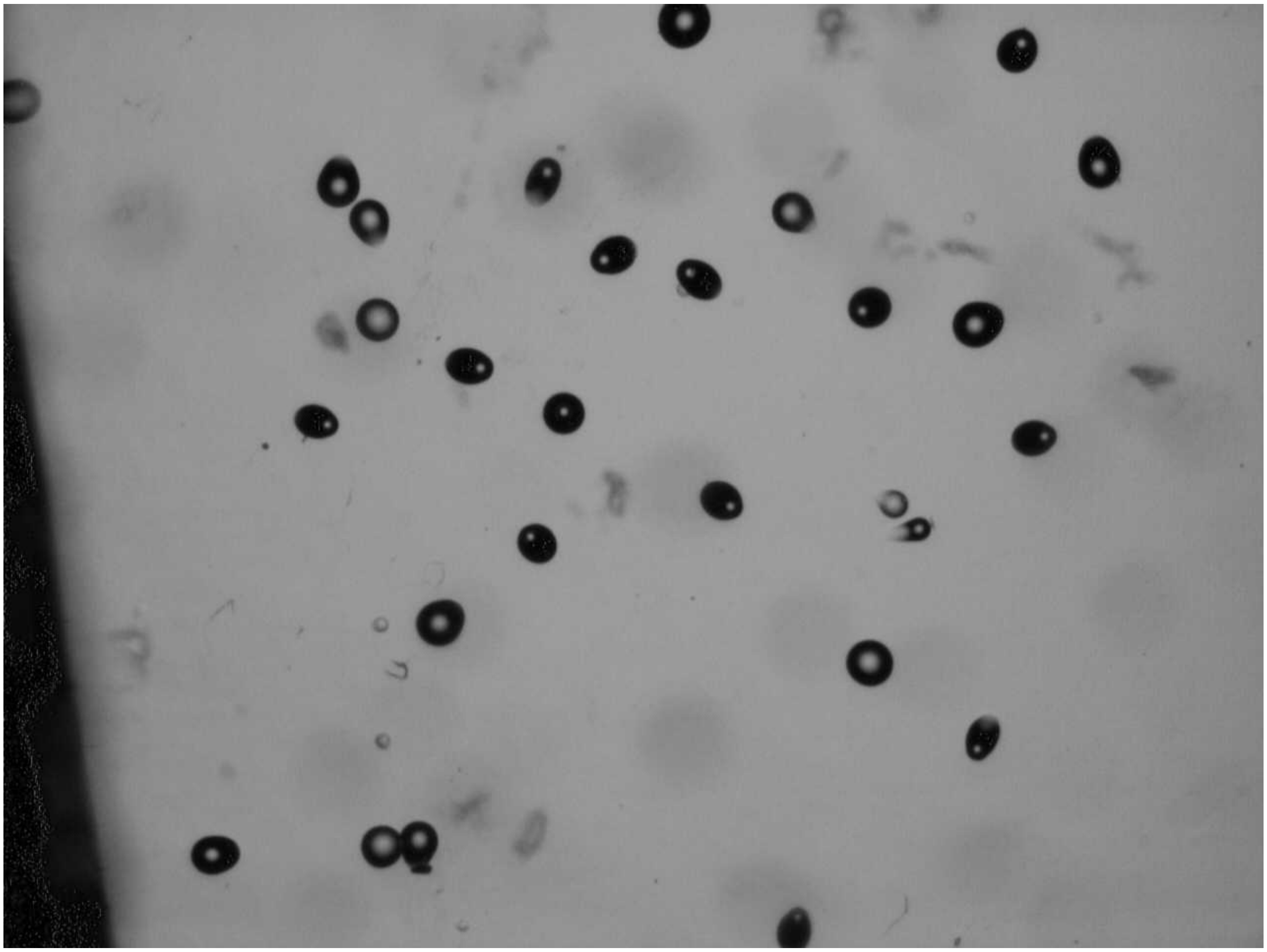

Figure 9
A cluster of very large pits. Very large pits were observed on the back side of the chip only (back side means facing away from the electrolyte).

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Please note that most of the 1.3 mm by 1.0 mm fields contain no pits at all. On the average a background pit can be found in one out of five fields.

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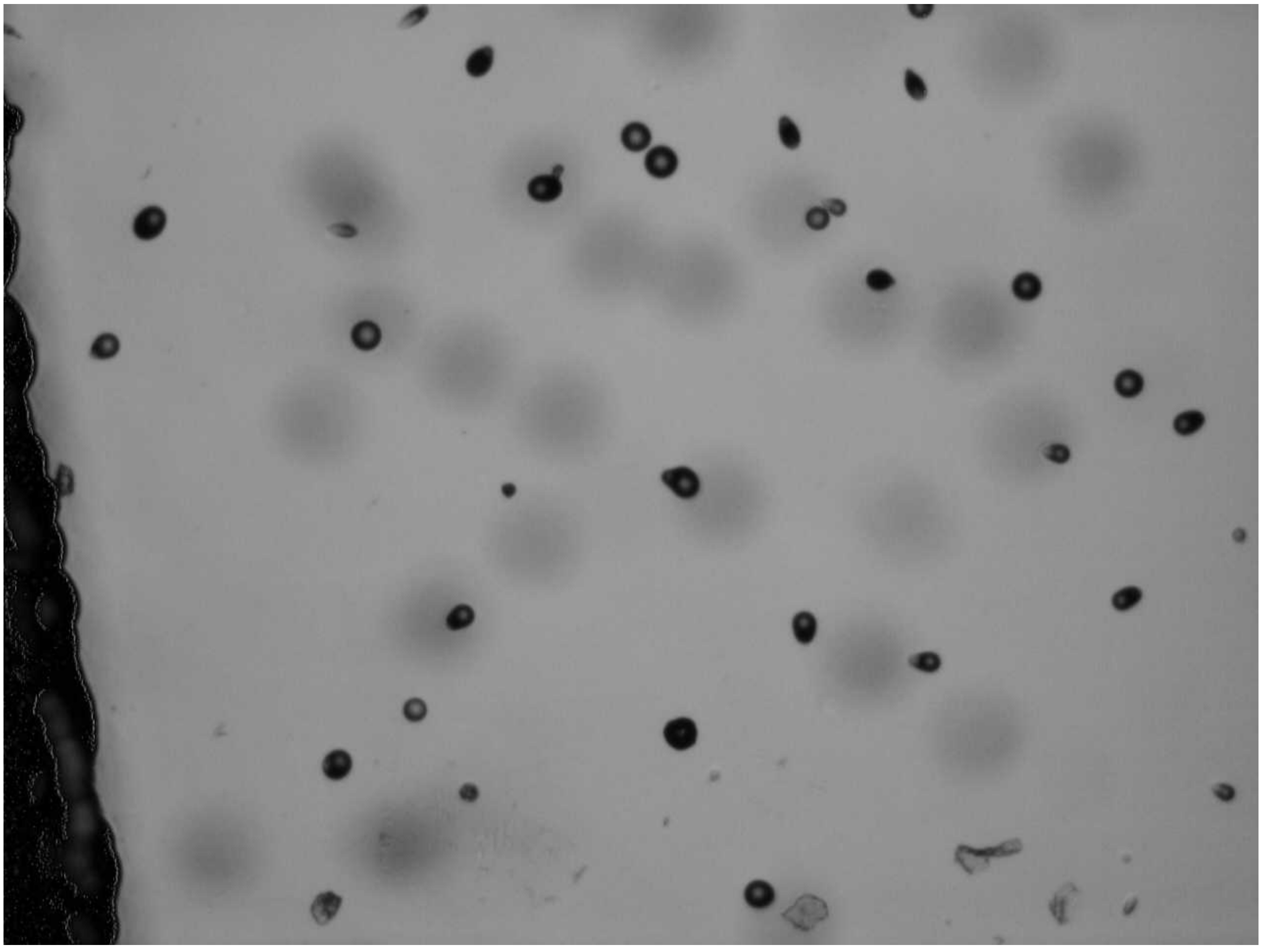

Figure 10
A cluster of medium-size pits on the top side of the chip (front side was facing the electrolyte). All pits, except for the background, are located near the margin of the chip. More than 99% of pits turn out to be located on less than 5 % of the total area.
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Large dark areas are due to pits on the oppsite side. Being out of focus these back side pits appear to be larger than in the previous figure.

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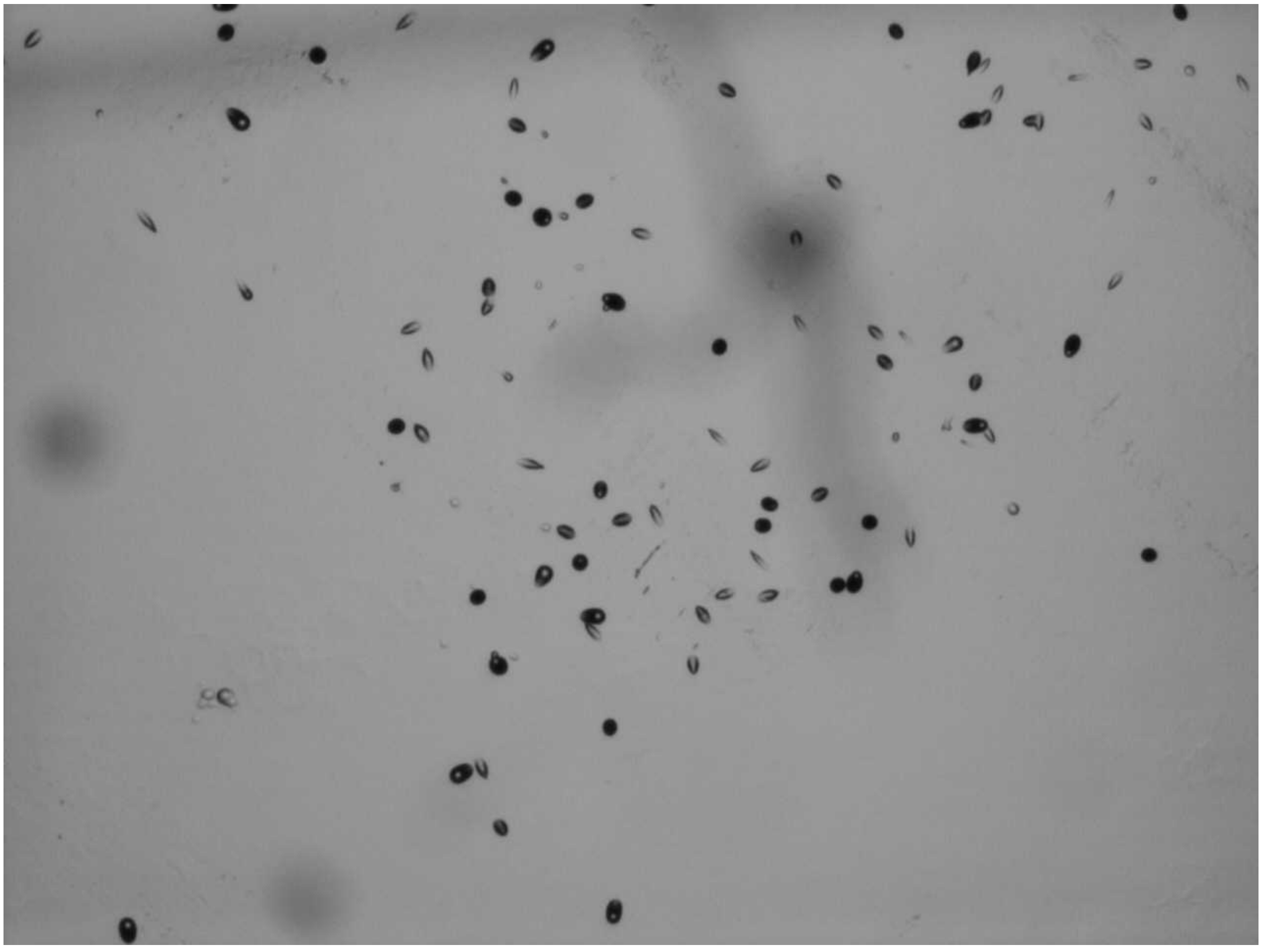

Figure 11
A cluster of small-size pits on the top side of the chip (top side was facing the electrolyte).
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Large dark areas are due to pits on the oppsite side. Being out of focus these back side pits appear to be larger than in the previous figure. - - - - - - - - - - - - - - -

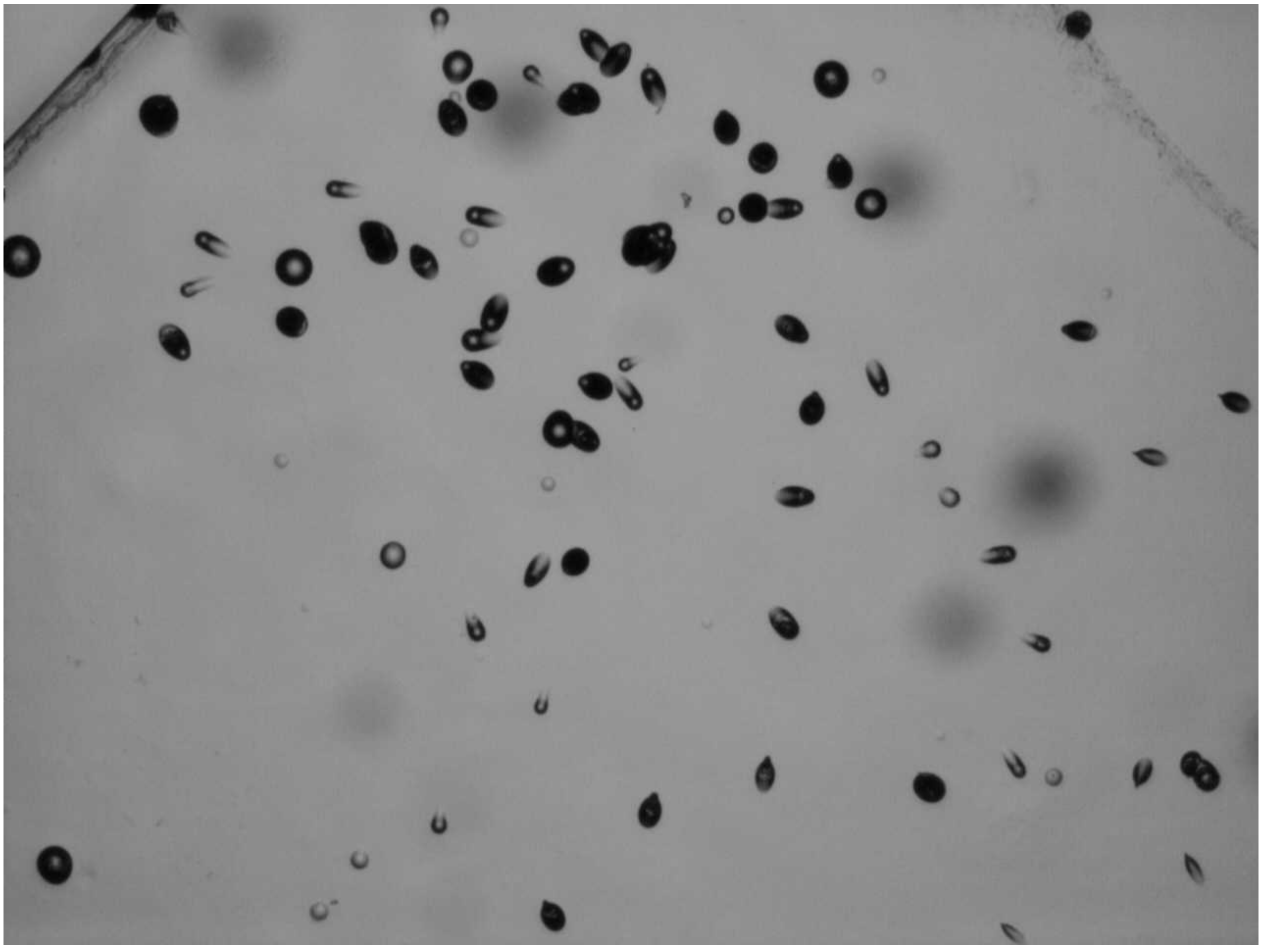

Figure 12
Another cluster of medium-size pits on the top side of the chip. -

* *

22) Time = 195 hours 21:15 5/15/07 (8 days +3 hrs):
I = 41 mA, V=5.55 V.

23) Time = 226 hours 4:15 5/17/07 (end of day 9 + 10 hrs);
I = 41 mA, V=5.40 V. Time to add water. After adding water potential became 6.35. Yes, the level is slightly above where I started.

24) A suggestion was made, on a private list for CMNS researchers, that excessive pits might be created by mylar. This prompted me to set up another control experiment. It started at 4:00 on 5/17/07.

a) A microscopic slide (glass) is resting on the box. About 6 mm of glass is actually in air, outside the box.
b) Protective films were removed from my CR-39 chip (~2 by 1 cm) and the chip was discharged (by touching it with salty-water-wet fingers.)
c) The chip is placed on the glass slide. About 6 mm of the chip is actually in air, outside the slide.
d) All is covered by the mylar film. About 6 mm of the film is actually in air, outside the CR-39 chip.
e) The small beaker (about 40 cc) is turned upside down and placed on mylar. That is my fake cell.
f) One rim of the cell is close to the edge of the box, actually, it is about 3 mm outside the box.
g) A scratch was made on the lower side of my CR-39 (to recognize it later).

Note that one end of the mylar film is totally in air, more of less as in my main experiments. The "null hypothesis" is that mylar becomes charged, creating an electrostatic field near the cell. That field attracts radioactive ions (always

present in air, due to cosmic rays, radon, etc.) into the region in which my CR-39 is in contact with air (at the lower side). Will this create excessive pits ? I will know in two or three weeks.

25) Time = 240 hours 18:15 5/17/07 (end of day 10)
I=41 mA, V=6.09 V

26) Time = 265 hours 19:15 5/18/07 (day 11 days + 1 hour)
I=41 mA, V=6.05 V Note: The current I=41 is the nominal value, as measured with a digital ammeter then the current stabilizer was built. I know that the actual current can fluctuate slightly due to fluctuations of temperature. In other words, the cell potential depends not only on how much water was lost but also on the temperature. In this experiment an old analog millimeter (full scale 500 mA) and vary accurate measurements are not possible. The instrument is used to indicate that the current is flowing. I plan to use higher currents (80 mA and 150 mA) before ending the experiment in June. Why? Because these currents were used in the experiment in which clusters were first observed. I do not know at what current the clusters were actually recorded.

27) Time = 293 hours 23:15 5/19/07 (12 days + 5hours)
I=41 mA, V=5.94 V

28) Time = 306 hours 12:15 5/20/07 (12 days + 18 hours)
I=41 mA, V=5.88 V

29) Time = 336 hours 18:15 5/21/07 (14 days)
I=41 mA, V=5.63 V

30) Time = 352 hours 10:15 5/22/07 (14 days + 16 hours)
I=41 mA, V=5.54 V
The level of the electrolyte is again about 1 cm above the anode. I will water now. After adding water V=6.57 V

31) Here is a message I posted on the CMNS list last night.
Scott Little wrote: “When examining the etched CR-39 under the microscope, each track you find always raises the question, "Is this a nuclear track or not"? The ones that are elliptical and have a distinctly conical shape have a high probability of being of nuclear origin. The ones that appear to be perfectly round but still have a conical shape are likely to be of nuclear origin. The ones that are perfectly round and relatively flat-bottomed are disturbingly ambiguous.”

**) One should expect a large fraction of tracks to be elliptical when the source of alpha particles is very close to a CR-39 chip. But nearly all alpha particle tracks are circular when the distance between the source and the detector is larger than 5 or 10 mm.

**) Looking at my three clusters, I see that the smallest one (Fig 5 and 6) has many elliptical pits. This could be interpreted as an indication that a nuclear process we want to replicate took place somewhere very close to the CR-39 chip. But pits in a larger clusters (Fig 1) are mostly round. This might be an indication that the process is taking place several millimeters away from the CR-39. The same can be said about the even larger cluster (Fig 3 and 4). In this case the distance might be even larger because the cluster is much larger, and because tracks diameters are widely dispersed. (Small diameters would be expected for particles which lose most of their kinetic energy before being intercepted by CR-39.)

**) In my opinion, it is too early for this kind of speculations, and for experiments suggested yesterday by X. In my opinion we must first agree that "seeded experiments" are reproducible on demand. As far as I know, each team that received seeds (recently-used Oriani's O-rings and mylar) observed clusters of pits. These pits are similar to tracks of charged nuclear particles.

**) What will happen when seeds are no longer available? In other words, what will happen when Richard Oriani stops performing experiments, one after another (to keep the NAE fire going)? I do not know how to answer this

question. For the time being as many researchers as possible should ask Richard for seeds (in order to replicate his results). The issue of possible artifacts, systematic investigation of parameters, and theoretical explanations, can wait till we agree on a high level of reproducibility. That would be a fantastic accomplishment (of turning protoscience into science?). Do not wait too long, please.

32) Tomorrow is day 16 of my second seeded experiment, as far as the current 41 mA is concerned. That will be four times longer than the duration of my first seeded experiment. Will I find more clusters than before? I hope so. But that remains to be seen. It is reasonable to expect 3*4=12 clusters, plus or minus 7. Why 7? Suppose I used a Geiger that registered 3 random particles from a radium source in 4 seconds. Then, the expected result, after 16 seconds, would be 12. For a mean value of 12 the distribution of possible outcomes is essentially bell-shaped and its standard deviation is close to 3.5. To be 95% confident that my expectation is correct, I would bet on the result that can differ from the mean value by two standard deviations. I expect to see at least 12-7=5 clusters because I assume that things are reproducible. (But my first experiment also contained 1 day of electrolysis with I=80 mA and 2 hours of electrolysis with I=160 mA. That is why I will run my second experiment for 4 additional days at 80 mA and for 8 additional hours at 160 mA. I have no idea at what currents were my clusters recorded.)

33) Time = 376 hours 10:15 5/23/07 (15 days + 16 hours)
I=41 mA, V=6.21 V

34) The statistical reasoning in 32 has a mistake. The 95% certainty would be valid if 12 clusters (or 12 Geiger counts) were really discovered in my first experiment. But the number 12 was an extrapolation. How to evaluate the level of confidence based on an extrapolation? I will ask a statistician. Meanwhile let me follow the intuitive approach. Three counts were recorded in a random sample drawn from a large Poisoning population. Thus the expected mean is 3 while the expected standard deviation is 1.73 (the square root of 3). Multiplying this by 4 one gets the mean of 12 and the standard deviation 6.97. The new standard deviationis nearly twice as large as the value I used above. Therefore the level of confidence, that the result will be between 5 and 19 is only 68% and not 95%. But even this is questionable; a Poisoning distribution with the mean of 3 is not bell shaped. Well, it is not difficult plot that distribution and to calculate probabilities of various intervals. But why should I waste time on this? Note that I totally ignored the fact that 21 consecutive experiments of Oriani, plus three consecutive experiments of Fisher, plus one to-be-confirmed (probably today or tomorrow) experiment of X, were successful.

35) Time = 386 hours 20:15 5/23/07 (16 days + 2 hours)
I=41 mA, V=6.07 V
Removing the current regulator and increasing the cell potential to 9.00 V. The current is 80 mA. I will use this current for 4 days (4 times longer that in the first experiment).

36) Time=386.25 hrs 20:30 5/23/07
I=82 mA V=9.02 V. I lowered the current back to 80 mA by lowering the potential difference to 8.73 V. Something is probably happened on the surface and lowered the overall cell resistance. About 15 minutes later the potential became 8.70 V; the current agin increased but very little. I should not pay attention to small voltage fluctuations. Next voltmeter reading will be tomorrow.

37) Last night the TGP beta team 2 (Marissa Little) reported that they also observed a cluster of about 70 tracks in the first seeded experiment, as illustrated in Figure I will say more about this in Figure 13 below.

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Figure 13
A closter reported by Marissa Little. Note that the cluster is surrounded by the area that has a negligible track density. The magnification was 40; the diameter of the displayed field is 2.5 mm.

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The etching temperature of the beta 2 team was 65 C. That is why sizes of pits are smaller than those shown in my Figure 1. Here is a brief message that Marissa just posted on the private list for CMNS researchers. "I've organized all of the experimental data for our PACA replication into an online logbook that I will keep up to date from now on. You can read about the first two experiments we completed and the two that are running now.

http://www.earthtech.org/PACA/logbook.html

Just a brief summary, our first experiment showed nothing anomalous. While waiting for additional o-rings from Richard, we ran a second experiment with what we thought were "dead" o-rings. The second experiment produced a cluster and a higher than background track count. Exp #3 and #4 are currently running (with fresh o-rings from Richard). Let me know if you have any questions of comments."

37) Time=387 hrs 21:15 5/23/07
I=81 mA V=8.76 V. I lowered the current back to 80 mA by lowering the potential difference to 8.24 V. Something is probably still happened on the surface and lowers the overall cell resistance.

38) Time=389 hrs 23:15 5/23/07
The current is about the same. V=8.40 V.

39) Time=396 hrs 6:15 5/24/07
The current is about the same. V=8.31 V.

40) Time=402 hrs 12:15 5/24/07

I =86 mA, V=8.22 V. To bring I back to 80 mA the V had to be lowered to 7.85 V. So I will continue with I=80 mA and V=7.85 V

41) Time=410 hrs 20:15 5/24/07
I=83 mA, V=7.84 V. To bring I back to 80 mA the V had to be lowered to 7.70 V. So I will continue with I=80 mA and V=7.70 V. How much of this is due to the progressive loss of water and how much to the instability of my simple d.c. power supply?

42) There was an interesting discussion, on a private list for CMNS researchers, about publishing our excitning results in a mainstream journal. Responding to someone, I wrote: " Yes, establishing repeatability should be the highest priority. I would not worry about possible artifacts, or about testing of the seeds hypothesis. Let us first reach the stage at which each of us, not only Oriani, can say, with confidence, 'do what I do and you will probably observe clusters, and excess tracks.' This can be accomplished in June, if we are lucky with the three ongoing experiments, my #2 and your #2 + #3. Also experiments of three other people on whose help Richard is counting. A paper Oriani suggested, with appendices from four or five teams, would be worth publishing, even if explorations of possible artifacts are still in progress. A protocol for a reproducible-on demand demo should be published as soon as each of us has additional replications. This will probably create a very favorable situation for new projects. But the seeding precondition should be part of the protocol, as I wrote yesterday."

Several hours later Marissa Little wrote: "I disagree. I think it is vital to investigate the possibility of artifacts before we publish. If the paper is published without evidence supporting the hypothesis that these tracks are indeed due to nuclear reactions occurring in the cell, it will be quickly dismissed by the scientific community at large. What if the effect turns out to be due to something mundane and not to nuclear particles? It would be very bad to have a published claim of a repeatable LENR experiment only for it to turn out to be an artifact. In order for LENR research to be accepted by mainstream science, researchers must be even more diligent with their investigations than those working in other fields. I don't think LENR scientists should publish early with preliminary results. I would not be comfortable publishing our results until a thorough investigation has been completed to rule out all artifacts and mundane causes for the effect. Only then will we be equipped to present a complete story that will speak for itself. At this juncture, I would have to 'argue' that the tracks were caused by LENR rather than the data "stating" that they were. I am not comfortable arguing any position in science. Let the facts speak for themselves."

43) Time=324 hrs 10:15 5/25/07
I =85 mA, V=7.61 V. To bring I back to 80 mA the V had to be lowered to 7.33 V. So I will continue with I=80 mA and V=7.33 V. I will have to add water this evening.

44) Time=435 hrs 21:15 5/25/07
I =89 mA, V=7.37 V. To bring I back to 80 mA the V had to be lowered to 7.08 V.
Add water. Current goes down to 54 mA at V=7.15 V. To bring I to 80 mA I had to increase the potential to 8.94 V. This 80 mA should be kept for 4 days. I started it at 20:15, on 5/23/07. So I have run it for 2 days and one hour. The end will be on Sunday evening (5/27/05). Then 8 hours at 160 mA and the end of this second experiment. Oriani is sending a chip to start another experiment. He thinks that another experiment will have more value that runing the current experiment for much longer.

45) Time=436 hrs 22:15 5/25/07
I =88 mA, V=8.89 V. To bring I back to 80 mA the V had to be lowered to 8.43 V.

46) Time=4446 hrs 8:15 5/26/07
I =80 mA, V=8.28 V.

47) Time=455 hrs 17:15 5/26/07
I =88 mA, V=8.27 V. To bring I back to 80 mA the V had to be lowered to 8.01 V.

48) Time=472 hrs 10:15 5/27/07
I =91mA, V=7.97 V. To bring I back to 80 mA the V had to be lowered to 7.51 V.

(ending 80 mA after 4 days)
Go from 80 mA to 160 mA
V=11.48 V before adding water and 13.30 V after adding water.

50) Time = 481.50 hrs; 20:45 5/27/07
I=200 mA, V=13.2 V. Lower V to 11.30 V to go back to 160 mA

51) Time = 482 hrs; 22:15 5/27/07
I=169 mA, V=11.49 V. Lower V to 11.03 V to go back to 160 mA

Use this current for 8 hrs (to match the 16 days at 40 mA) and then for 0.5 hrs (to match one extra day of 40 mA). Then go to 80 mA for 6 hours (also to match the extra day at 40 mA) and finally to 40 mA for 24 hours. End the experiment on Tuesday, 5/29/07, provided the CR-39 for the next experiment arrives. If not then keep running for another short 40, 80, 160 cycle.

52) Time = 506 hours; 22:15 5/28/07
I=41 mA, V=5.86 V. Experiment proceeds as planned; I am in the middle of the last 24 hours, unless the CR-39 chip for the next experiment does not arrive tomorrow. The mean background on that chip, as specified by Oriani, is 16.6 tr/cm^2; st.dev. 9.7. hat is about the same as he wrote about the previous chip. But that chip actually had about ten times lower background.

53) Time = 514 hours (21.5 days since the beginning); **6:15 5/29/07**
I=40 mA, V=5.89 V.

54) Time = 518 hours (the end of electrolysis is not far away) 10:15 5/29/07
I=40 mA, V=5.76 V

55) Time = 523 hours (ending the electrolysis at 15:15 5/29/07)

56) Summary
Experiment #1 lasted 5.08 days (~79% at 41 mA, ~20% at 80mA and 1.7% at 160 mA)
Experiment #2 lasted 21.8 days (523 hours, with the same percentages as above). I expect to see about 12 clusters, plus or minus several, depending on the fluctuating level of optimism.

57) Experiment #3 started at 16:50 on 5/29/07, less than an hour after the end of the experiment #2. The electrolyte was the same as used in experiment #1 (in which three clusters were found) and reused in experiment #2, which just ended. But new mylar is used in the experiment #3.

58) About exeriment #2:
Results from the experiment #2 will be posted below, as I did after the experiment #1. One thing should be mentioned. In taking apart the cell used in the experiment #2 I noticed some moisture on the CR-39 surface below the mylar. That surface was dry at the end of the experiment #1. I suspect some tiny holes in mylar (used by Oriani and then by me in two experiments) were responsible. It was not a leak; only a very thin layer of moisture. And only near the center. I do not think that the layer of moisture could create interpretational problems, especially if clusters are found outside the O-ring area. No visual sign of any kind could be seen on the surface after the moisture was wiped out. I will examine the surface under the microscope, before etching, as well. Tomorrow I will probably etch the chips and have the first look at them under the microscope.

What has been done today (5/30/07)? The chips from the experiment #2 were etched for 6 hrs (NaOH 6.25 N at 73 C). At the same time the already-etched chip from the experiment #1 (with three clusters) was etched again for additional 5 hours. I will start examining the chips from the experiment #2 tomorrow. Today I examined the chip from the experiment #1. Perhaps longer etching will tell us something important. What did 11 hours of etching did to tracks of alpha particles? Tracks became much larger, as shown in Figure 14 below.

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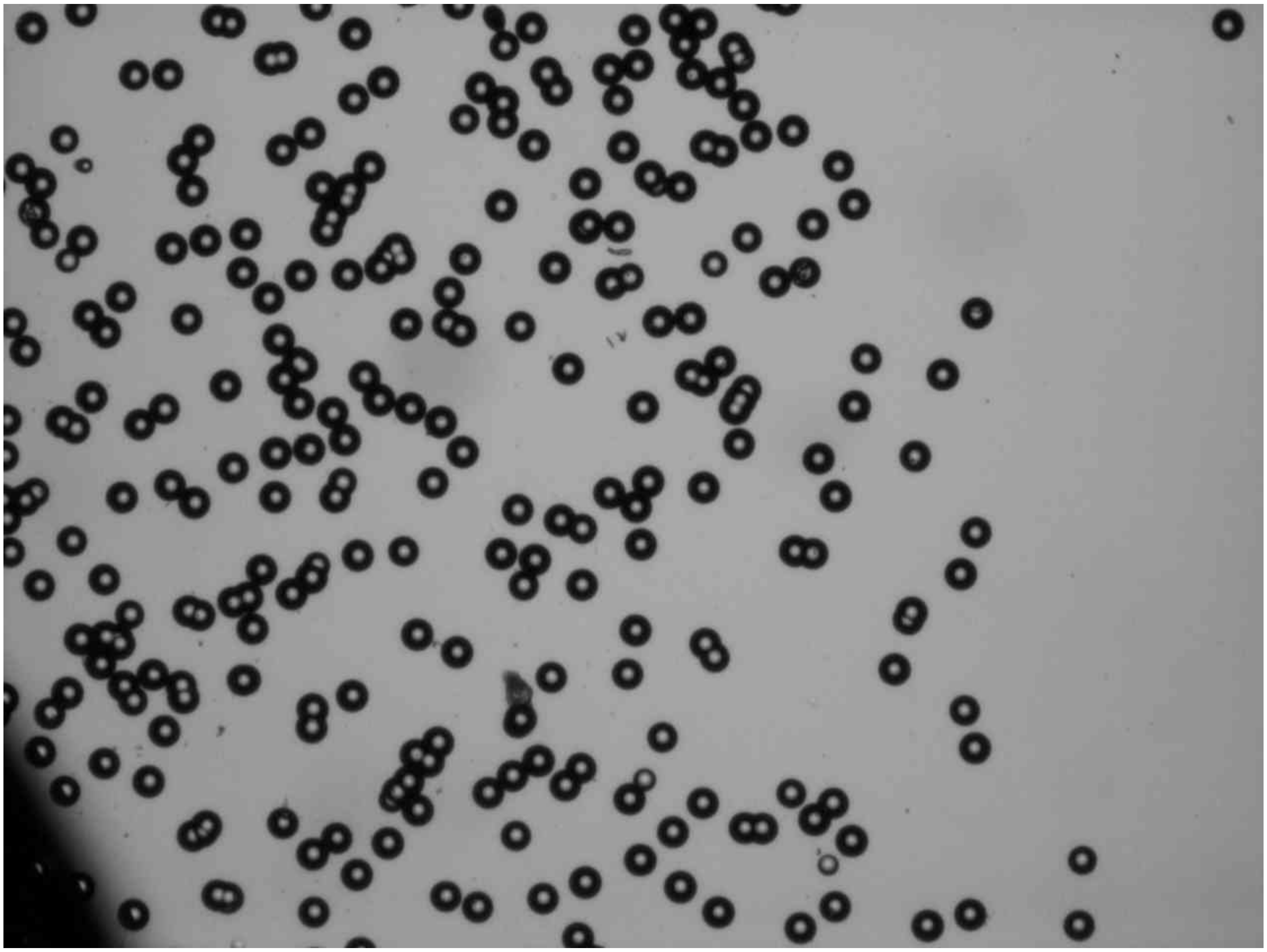

Figure 14

Tracks of alpha particles from Am-241 after 11 hours of etching. Compare them with tracks after 6 hours of etchin, in Figure 2. Magnification 40. The field shown is about 1.0 by 1.3 mm.

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The Figure 15 below shows a cluster which I though was the same as in Figure 1, except the tracks are larger. But now I am not sure. Perhaps it is a cluster I missed before. Or it can be the same cluster as in Figure 1 except that some tracks were washed away. Unfortunately, it is not easy to find exactly the same spot. Missing a cluster on this chip is easy because it became twisted.

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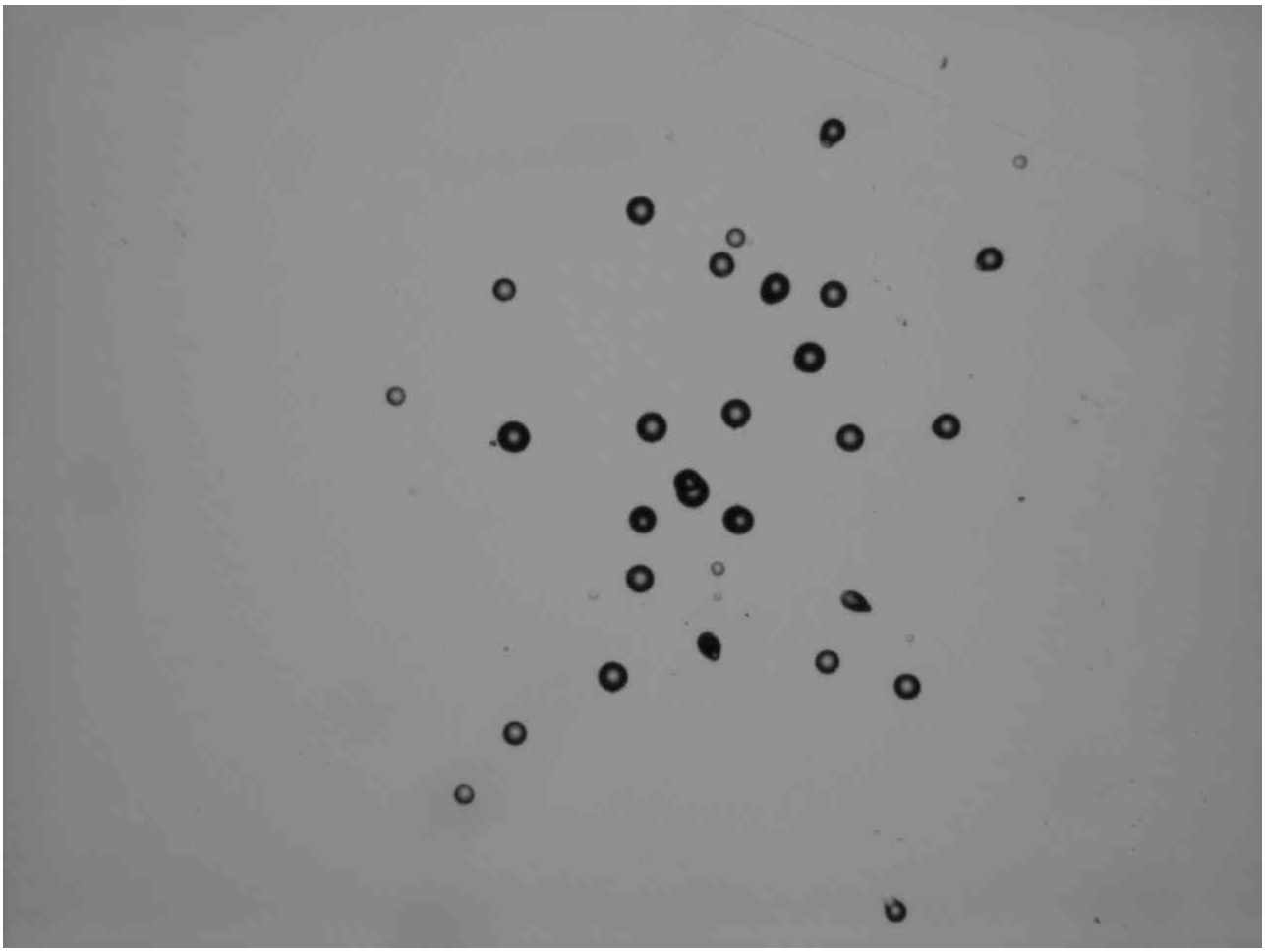

Figure 15
Is this a cluster I missed before or is it a cluster that lost some tracks due to additional etching? Magnification 40. The field shown is about 1.0 by 1.3 mm.
- -

The figure 16 below shows the same cluster as in Figure 5, except that tacks are larger. One thing becomes clear; many tracks that were elliptical before became round. That is good to keep in mind. Large tracks are easier to count but loosing elipticity might not be desirable. Another undesirable thing is that tracks are overlapped more often.
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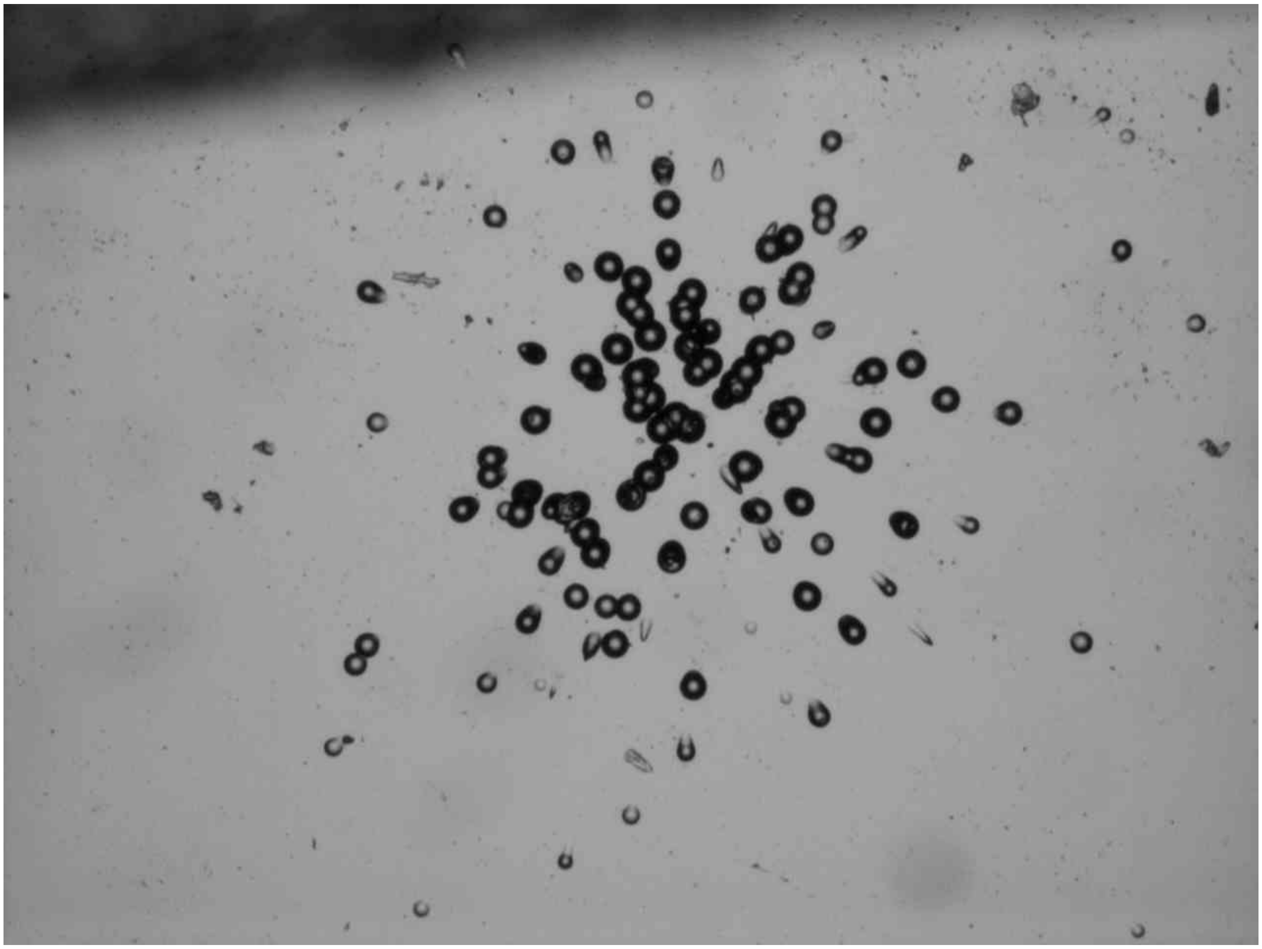

(BR> Figure 16
A cluster that was shown in Figure 5, after additional 5 hrs of etching. Magnification 40. The field shown is about 1.0 by 1.3 mm.

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The large cascade, shown in Figures 3 and 4, already had a smalled and shallower tracks. In Figure 17 one can see some tracks from that cluster, after additional 5 hours of etching. They seem to be even more shallow (less dark) than before. The figure 17 shows an island of pits which I did not see before. I would not count them as tracks. But what are they? I saw several little island of such “foam” today. In any case, this question belongs to speculations; our task, at this stage is to either agree or disagree on reproducibility. What is reproducible and what is not? It is a waste of time to speculate about things which might turn out not to be reproducible.

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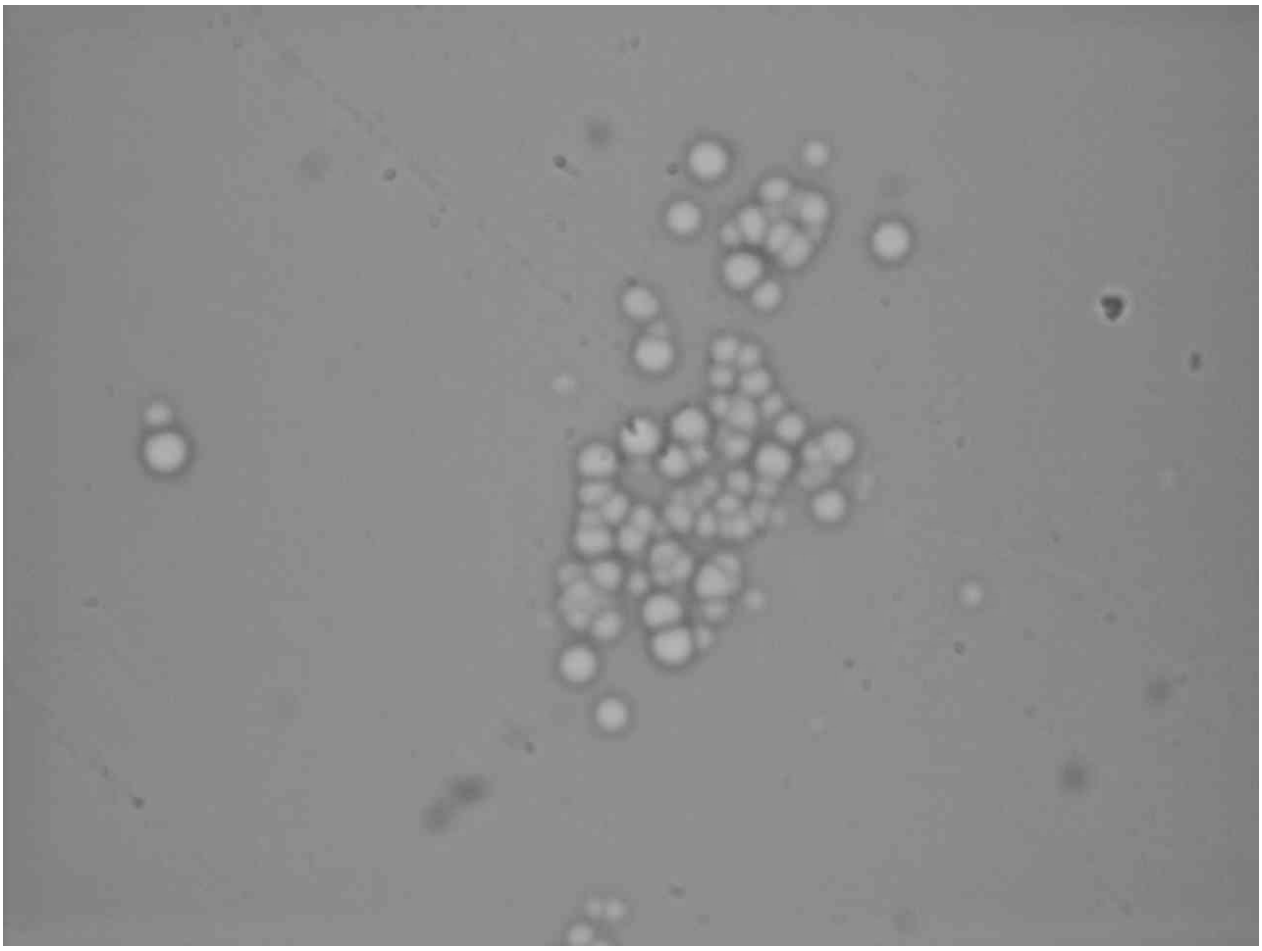

Figure 17
An island of very shallow pits that are very close to each other. Magnification 200. The field shown is about 0.2 by 0.26 mm.
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A smaller island of very shallow pits is shown in the Figure 18.
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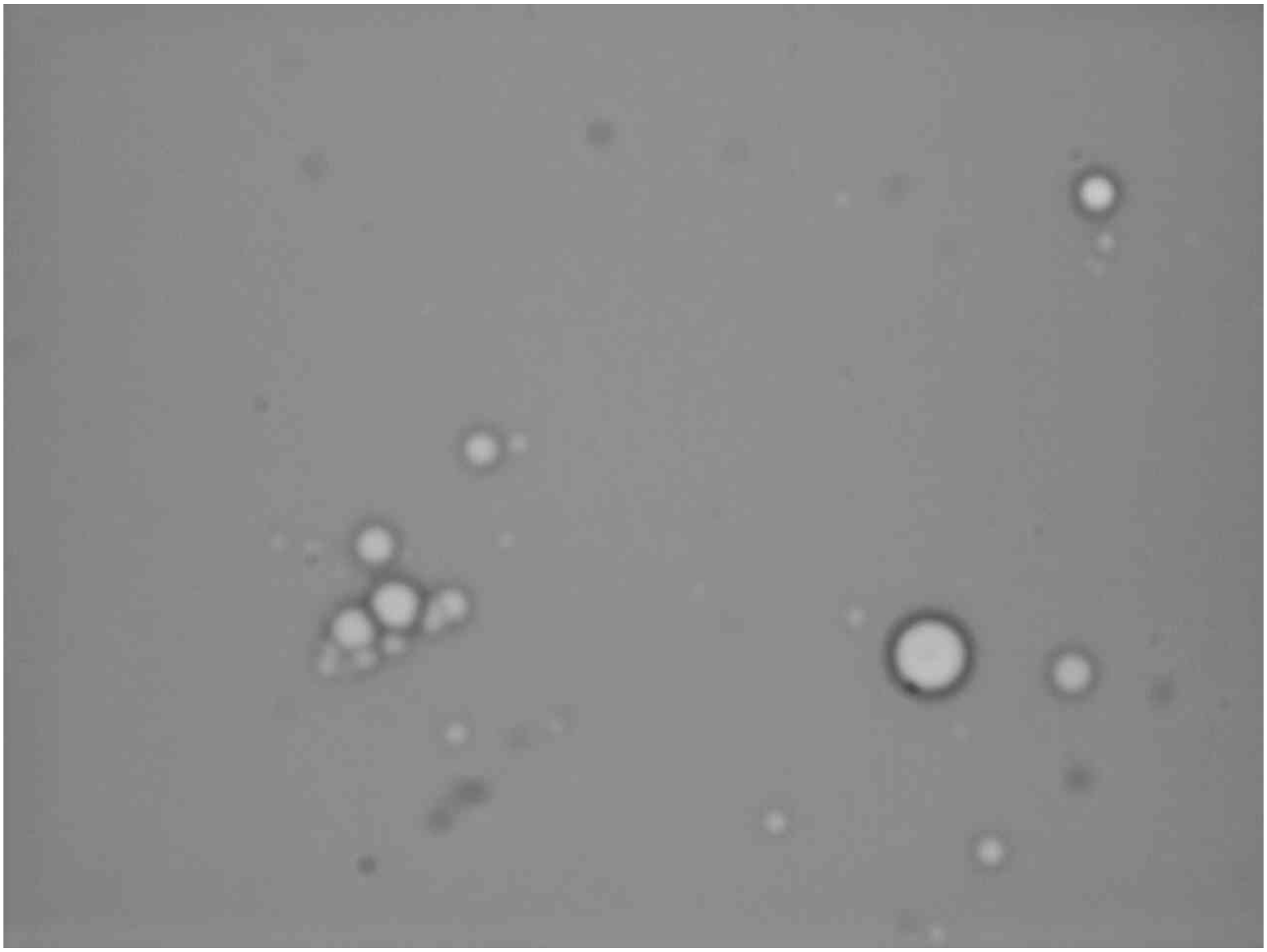

Figure 18
Another island of very shallow pits that are very close to each other. The field shown is about 0.2 by 0.26 mm. Magnification 200. At that magnification, pits due to alpha particles would be about two times larger than the largest pits below (toward the lower right corner). And they would have dark boundaries, as in Figure 14.
- -

Pits like those shown in the last two figures will not be counted when the CR-39 chips, from the experiment #2, are examined tomorrow. Keep in mind, in comparing pictures with each others, that the level of focusing and the level of illumination effect peak appearences. I am glad that Marissa found two large clusters. How many will I find tomorrow? The large chip will be easier to work with because it is not twisted. So I will not have to constantly refocus when the chip is scanned.

59) Other replications: Marissa Little also finished two experiments and clusters were observed in each of them. Preliminary results, and pictures, are already in her electronic logbook at:

http://www.earthtech.org/PACA/logbook.html

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60) My three experiments were #1 (5 days of electrolysis) #2 (21 days of electrolysis) and #3 (it has been running for over 2 days so far). The five chips from the experiment #2 (their locatios are shown in Figure 8 above) have been etched yesterday. Today I examined these chips. Figure 18 shows tracks of alpha particles, from the little corner of my large chip.
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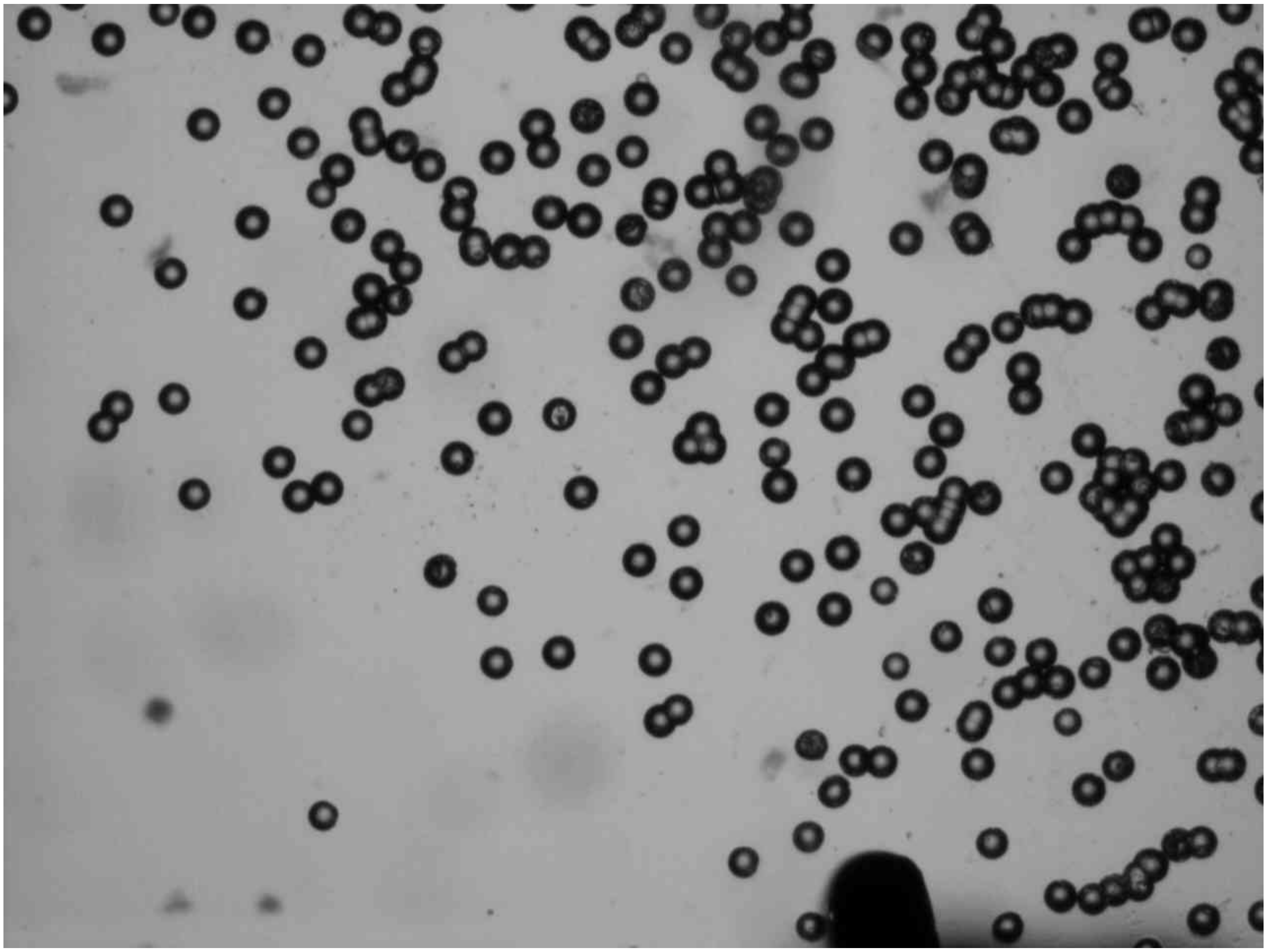

Figure 19
Tracks of alpha particles from Am-241 after 6 hours of etching. Magnification 40. The field shown is about 1.0 by 1.3 mm.
- -

Figure 20 shows the largest cluster I found. It has about 80 tracks. The average track density, over the field shown in this figure is 80/0.013= 6154 tracks per cm^2. The average background density is known to be close to 15 tr/cm^2.
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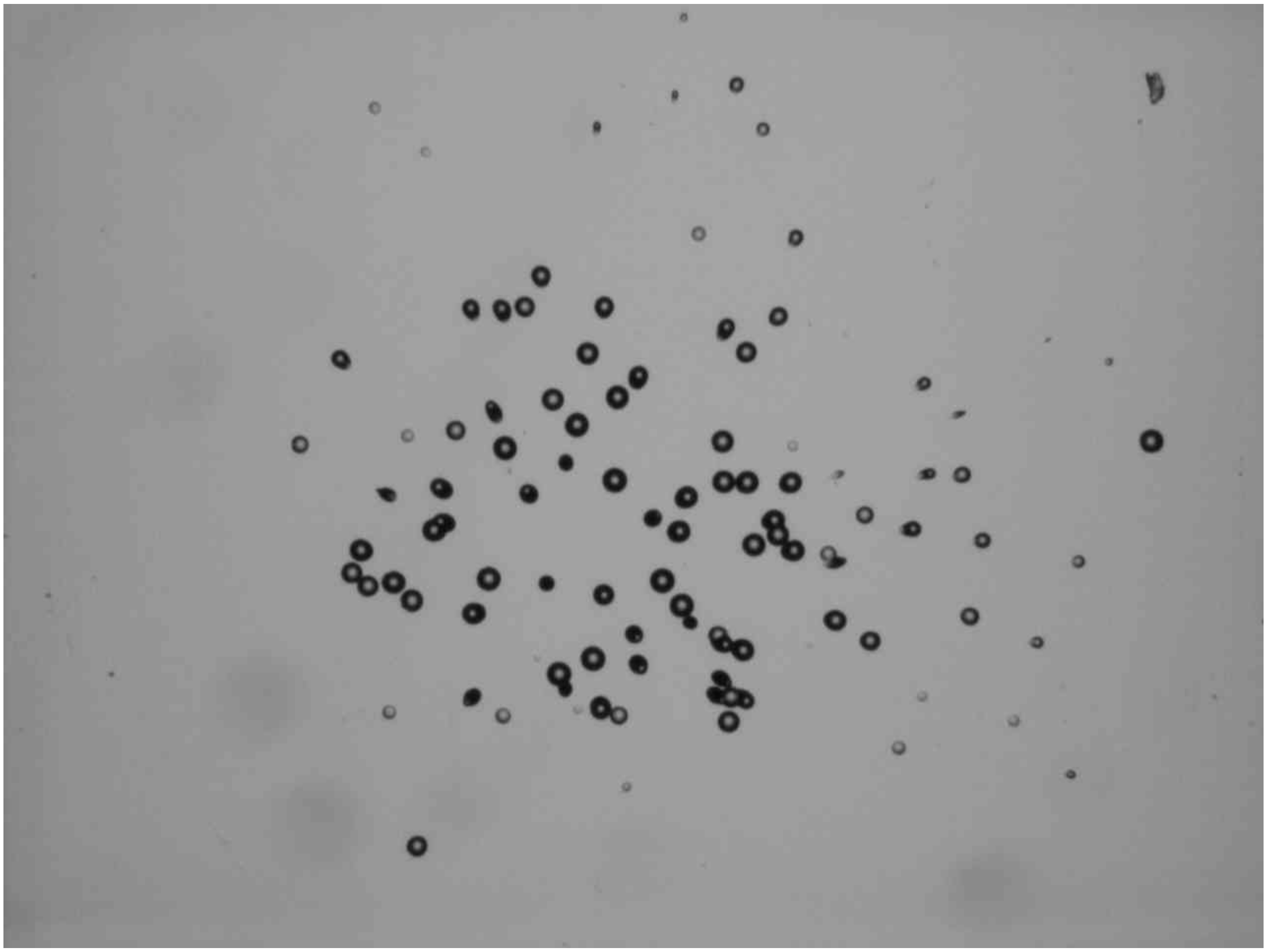

Figure 20
A cluster of about 80 tracks. Magnification 40. The field shown is about 1.0 by 1.3 mm. This cluster was found on the side of CR-39 chip that was facing down (away from the electrolyte). I looked at the corresponding circular field above it (the field looking up) and found only two tracks, on the field periffery. Visually seen circular fields have diameters close to 5mm, their areas are 3.7 times larger than areas of rectangular fields of the camera.
- -

Figure 21 shows a small cluster. Many more such clusters could be found. In this case one has 7 tracks on the area of 1.3 mm^2. This translates into 7/0.013= 538 tracks per cm^2. That is nearly 40 times higher than the mean background density.
- - - - - - - - - - - - - - -

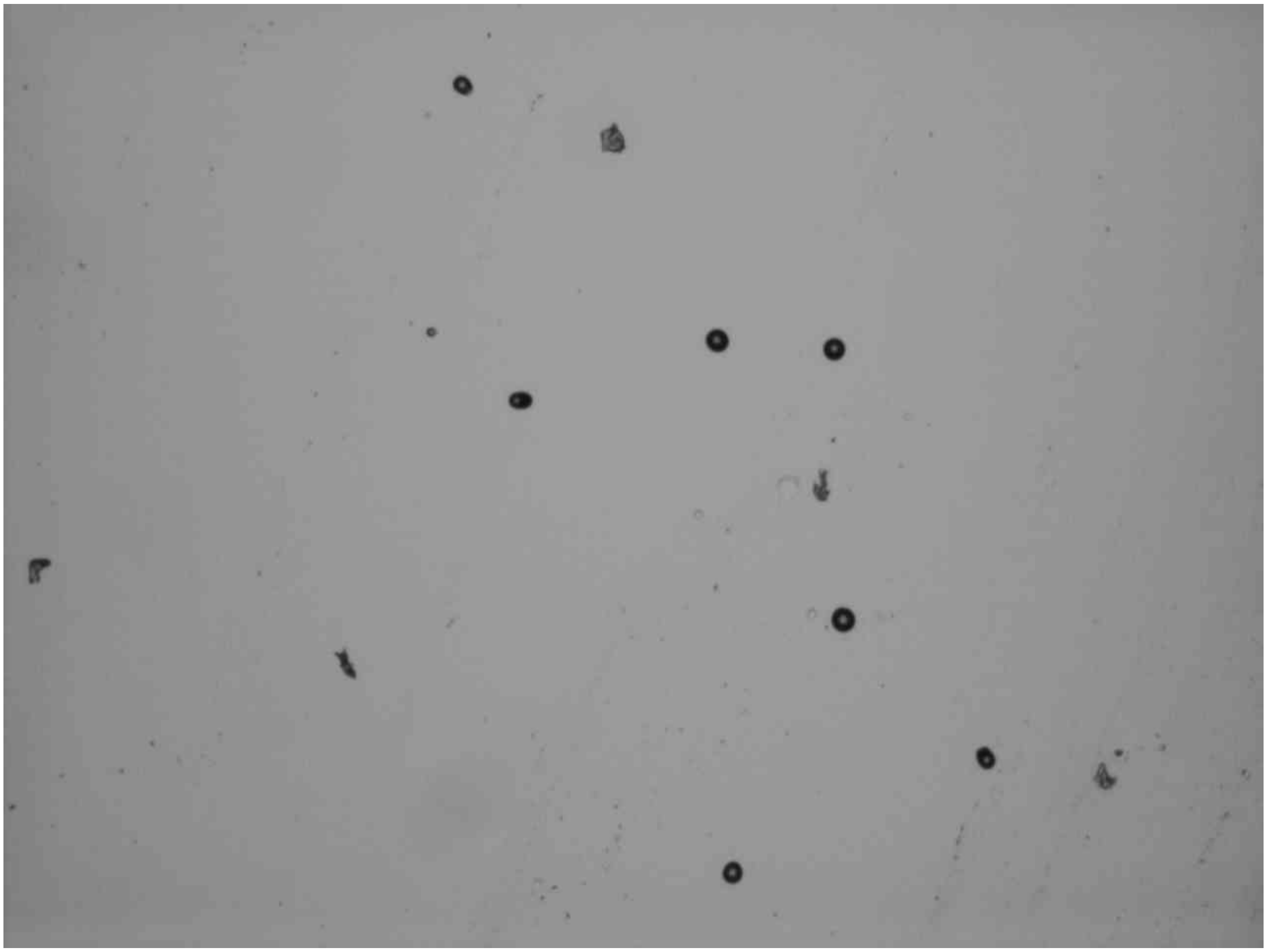

Figure 21
Tracks of a small cluster. Magnification 40. The field shown is about 1.0 by 1.3 mm. The local field density, 538 tr/cm^2 is still much higher than in the background (15 tr/cm^2). Clusters with small number of tracks, like the one above, could be found on each side of the chip.
- -

Note that diameters of pits in clusters are about two times smaller than the diameters at tracks due to alpha particles from Am-241. This is probably significant. I wish the exposure to alpha particles (at the corner of my large CR-39) had been done before the electrolysis and not after it. For the time being this issue should probably be ignored. Our goal is to establish reproducibility of clusters. Each experiment, so far, confirms Oriani's claim. In that sense, great progress was made in recent weeks. Figures 22 and 23 show a cluster that occupies two fields.
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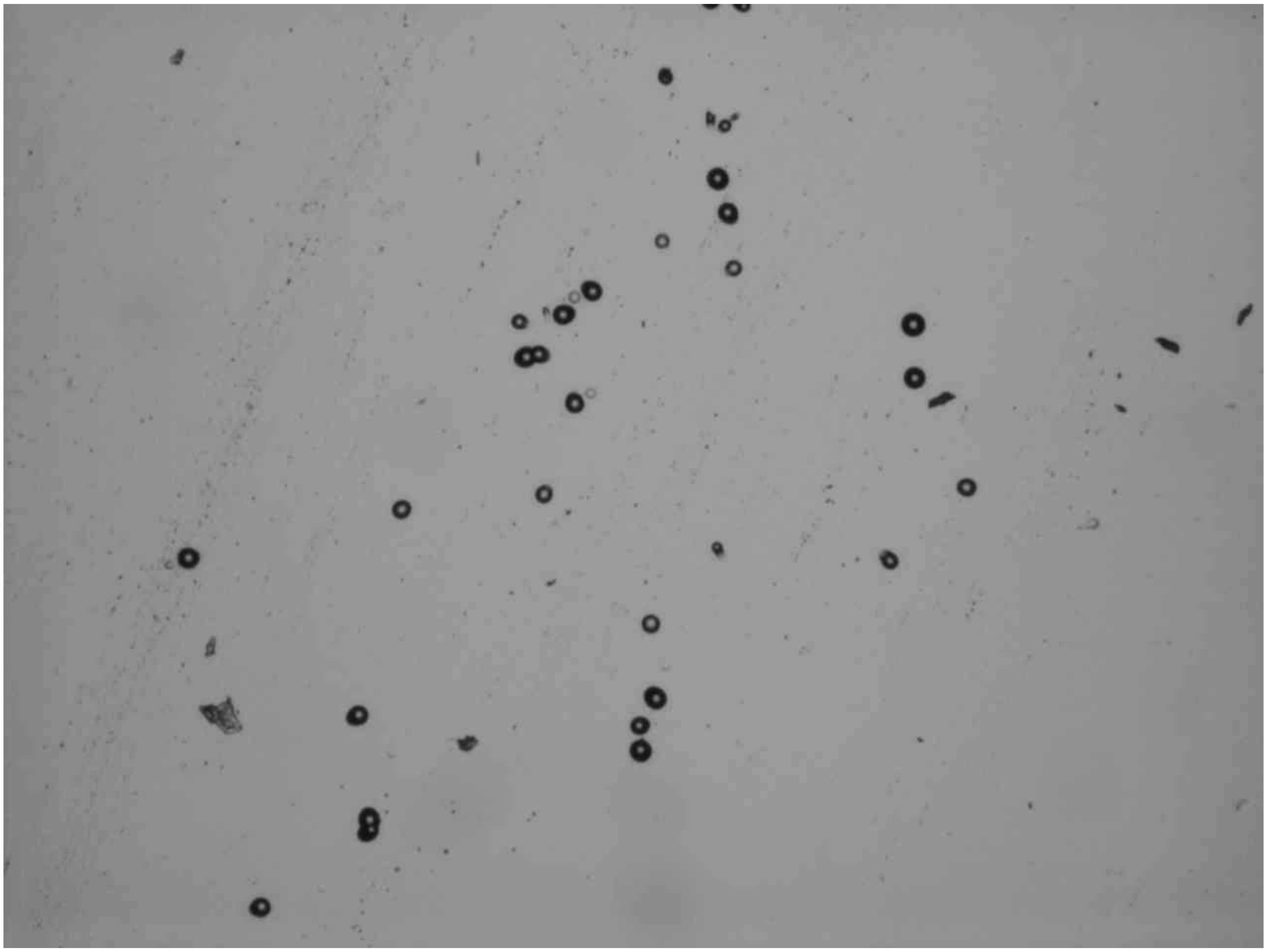

Figure 22
Lower part of a cluster (27 pits). Magnification 40. The field shown is about 1.0 by 1.3 mm. This cluster was found on the side facing the electrolyte. The circular field below it (the field facing away from the electrolyte) had zero tracks.
- -

Some tracks can be seen on both figures.
- - - - - - - - - - - - - - -

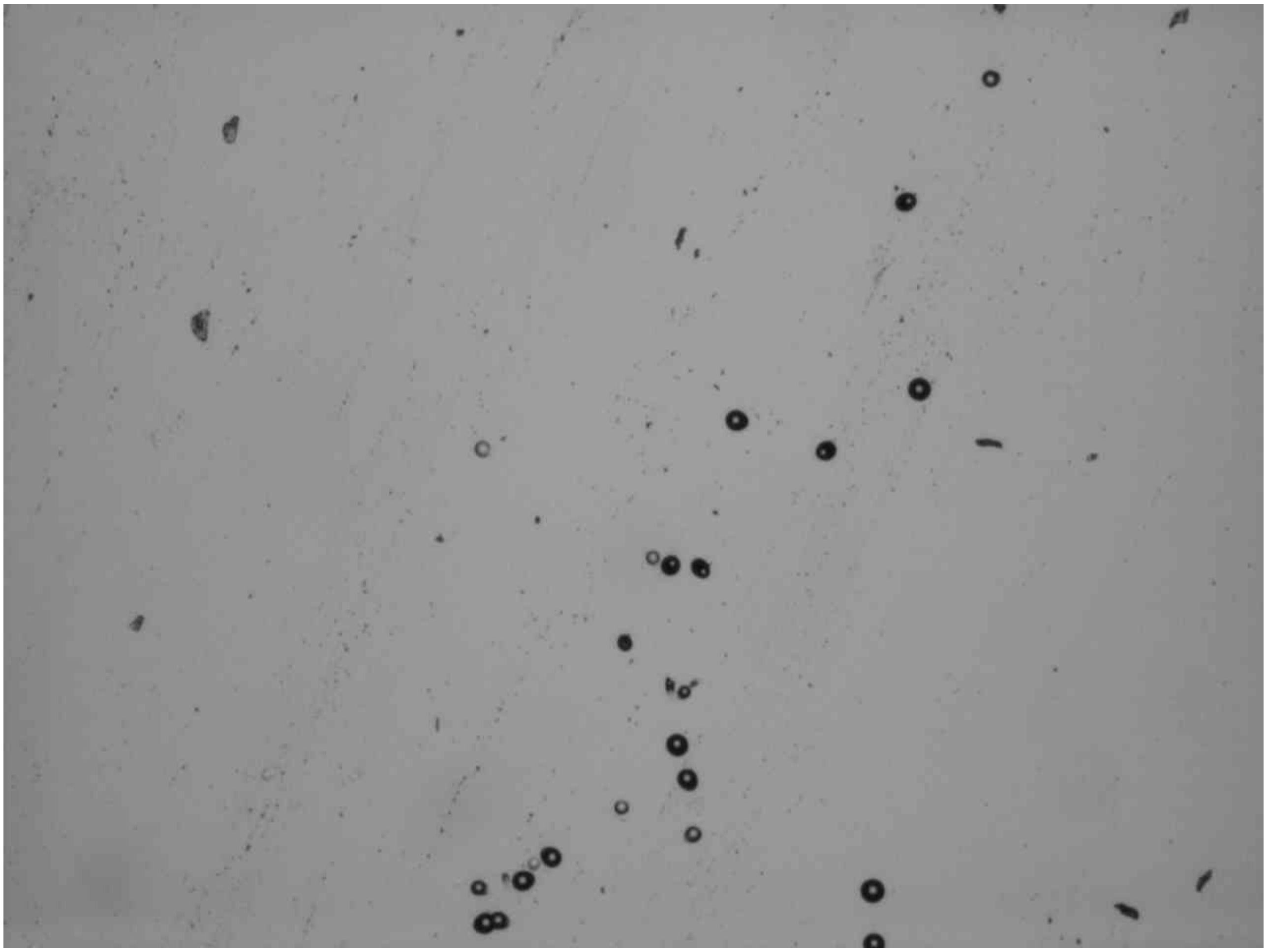

Figure 23
Seven additional pits of the cluster shown in Figure 22 can be seen in the upper part of this figure. Magnification 40. The field shown is about 1.0 by 1.3 mm. This cluster was found on the side facing the electrolyte. The circular field below it (the field facing away from the electrolyte) had one track.

- -

Figure 24 shows a mini-cluster, similar to that of Marissa.

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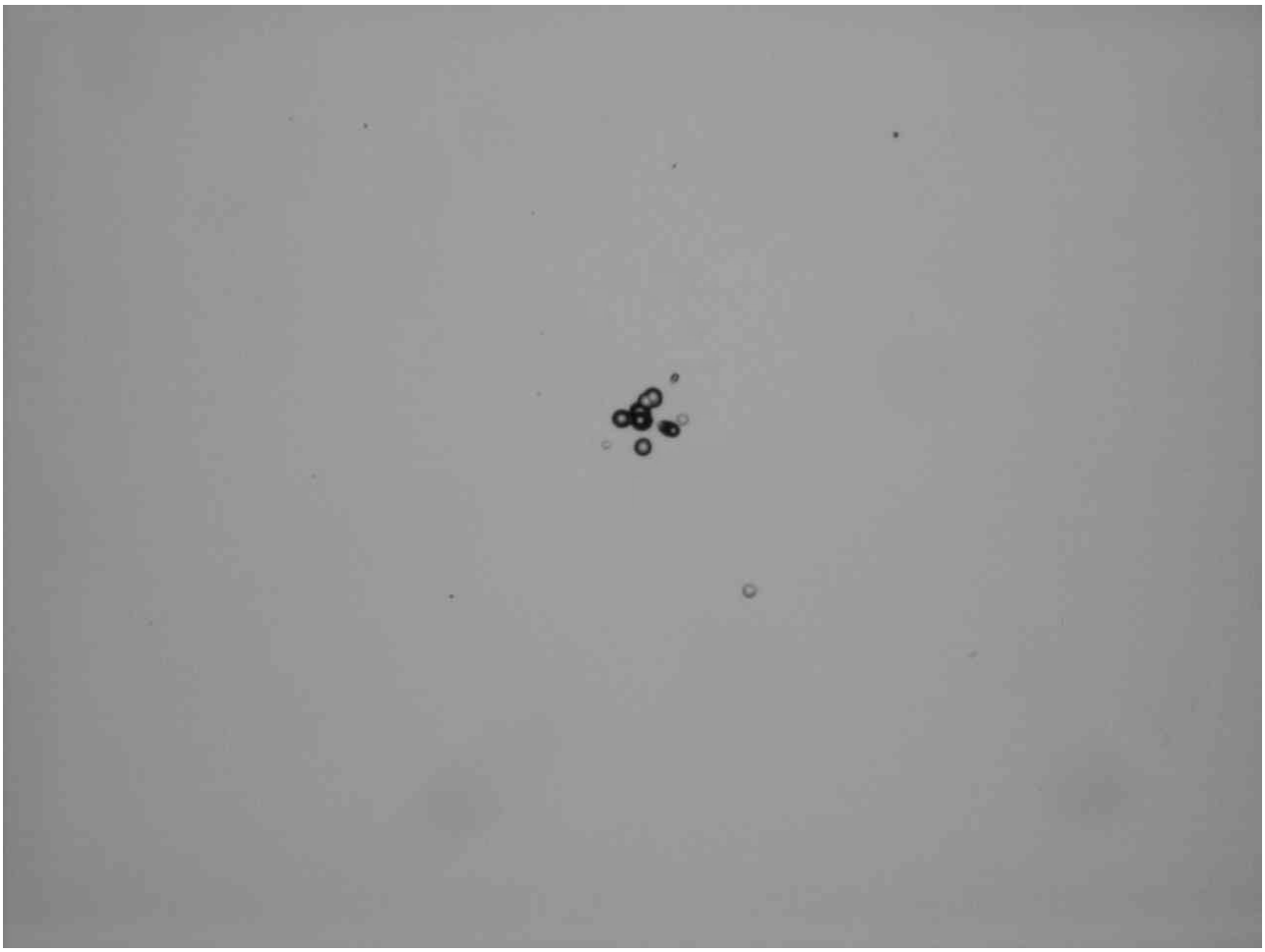

Figure 24
A tiny cluster. Magnification 40. The field shown is about 1.0 by 1.3 mm. This cluster was found on the side facing the electrolyte. The circular field below it (the field facing away from the electrolyte) had zero tracks.
- -

Figure 25 shows the same mini-cluser under a five times larger magnification.
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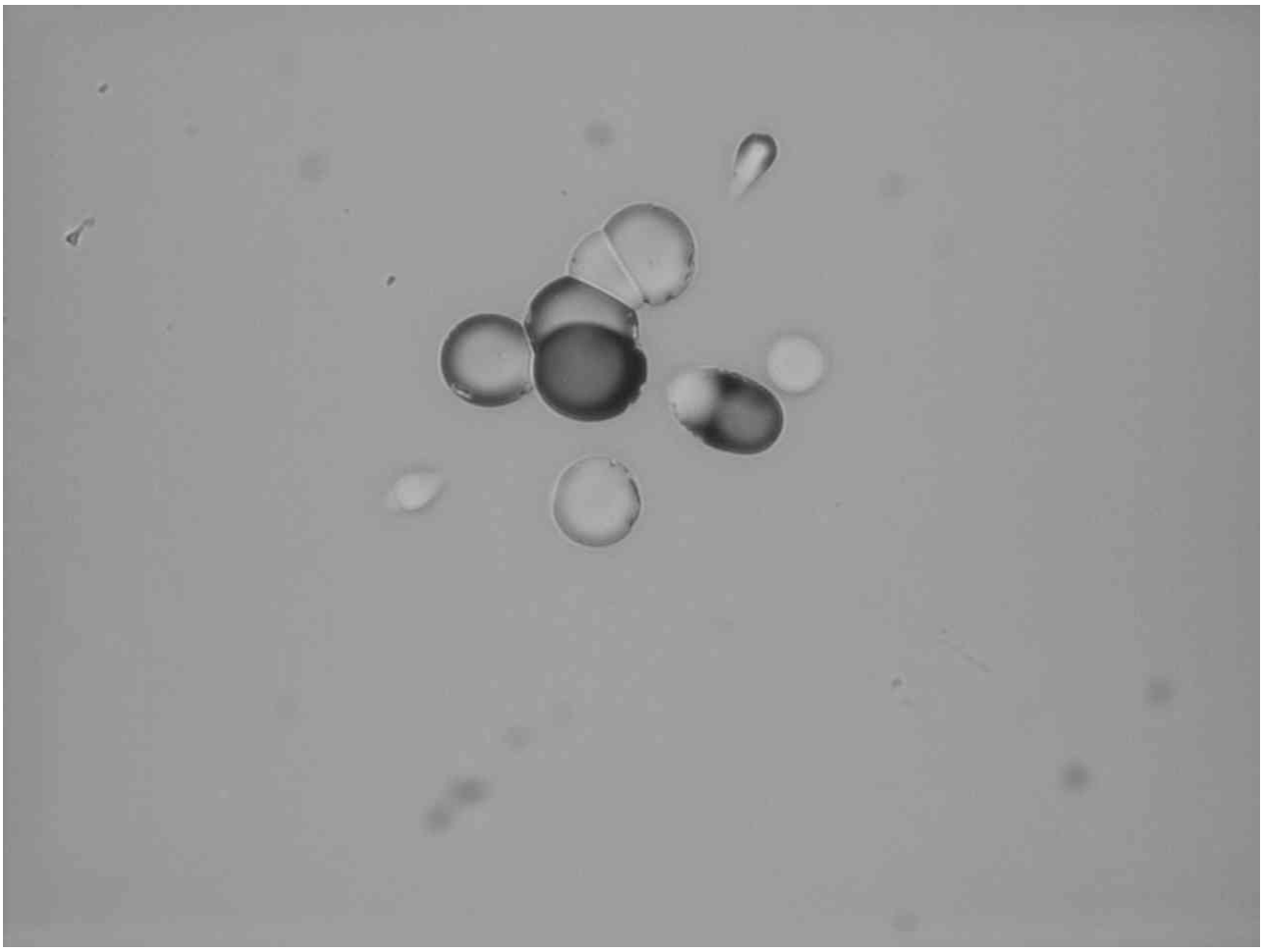

Figure 25
A tiny cluster. Magnification 200. The field shown is about 0.2 by 0.26 mm. This cluster was found on the side facing the electrolyte.
- -

No clusters were found on four smaller chips, placed as shown in Figure 8. The numbers of tracks on 8 surfaces of these chips (each 2 by 0.9 cm) are shown below.

41 tracks on Surface 1
22 tracks on Surface 2
36 tracks on Surface 3
34 tracks on Surface 4
35 tracks on Surface 5
32 tracks on Surface 6
33 tracks on Surface 7
26 tracks on Surface 8

These results are similar to the background, measured by Richard (who sent me the chips). The sum of tracks on both sides of the large chip, excluding those that belong to large clusters, turned out to be 396. The exclusion process was subjective. Dividing this sum by 2*9 cm^2 one gets 22 tacks per cm^2. This is not significantly larger than the expected background of 15 tracks per cm^2. Part of the difference might be due to small clusters (that were not excluded) but this cannot be taken for granted. Note that local track densities within some clusters, as in Figure 19, are orders of magnitude higher than in the background. For that reason we should emphasize clusters and not mean track densities (which include clusters). This reminds me of a famous saying attributed to E. Rutherford -- “if your experiment needs statistics, you ought to do a better experiment.” Clusters will be easier to sell than mean densities.

61) June 1, 2007

Experiment #2, that lasted 21 days, did not produce many more clusters that the experiment #1, that lasted only 5 days. Prompted by Richard and Marissa, I decided to end the experiment #3 after only four days of electrolysis, and to start experiment #4.

62) June 2, 2007

Experiment #3 was stoped at 15:50 today (after 95 hours of electrolysis at I=41 mA). That was only one hour less than 4 days. Once again some moisture was found on the CR-39 that was supposed to be protected from the electrolyte. The mylar had no rupture but the imprit of the spiral nickel cathode (the foot) was clearly visible. That is where, I suspect, was the leakage. Instead of starting experiment #4 at once I removed the O-rings and applied the CR-39 chips to them. Motivation for this was in what I know about Oriani's results (a lot of tracks below the rings), and in what was reported by Marissa yesterday (also excessive track below the ring).

Figure 26 is very crude schematic representation of my O-ring testing arrangement. The two removed O-rings are labeled as Q and P while four 2 by 1 cm chips are labeled as A, B, C, and D. In the cell from which the two O-rings were removed the ring P was below the ring Q, as illustrated. The chip A stayed below the ring Q for 24 hours, the chips C and D stayed above the ring P for 24 hours, while chip B stayed between the two rings for 24 hours. The chip E has three areas. During the first 8 hours first area was between the two rings, during the second 8 hours the second area was between the two rings, and diring the last 8 hours the third are was between the two rings. This was accomplished by sliding the chip E from one position to another.

- - - - - - - - - - - - - - -

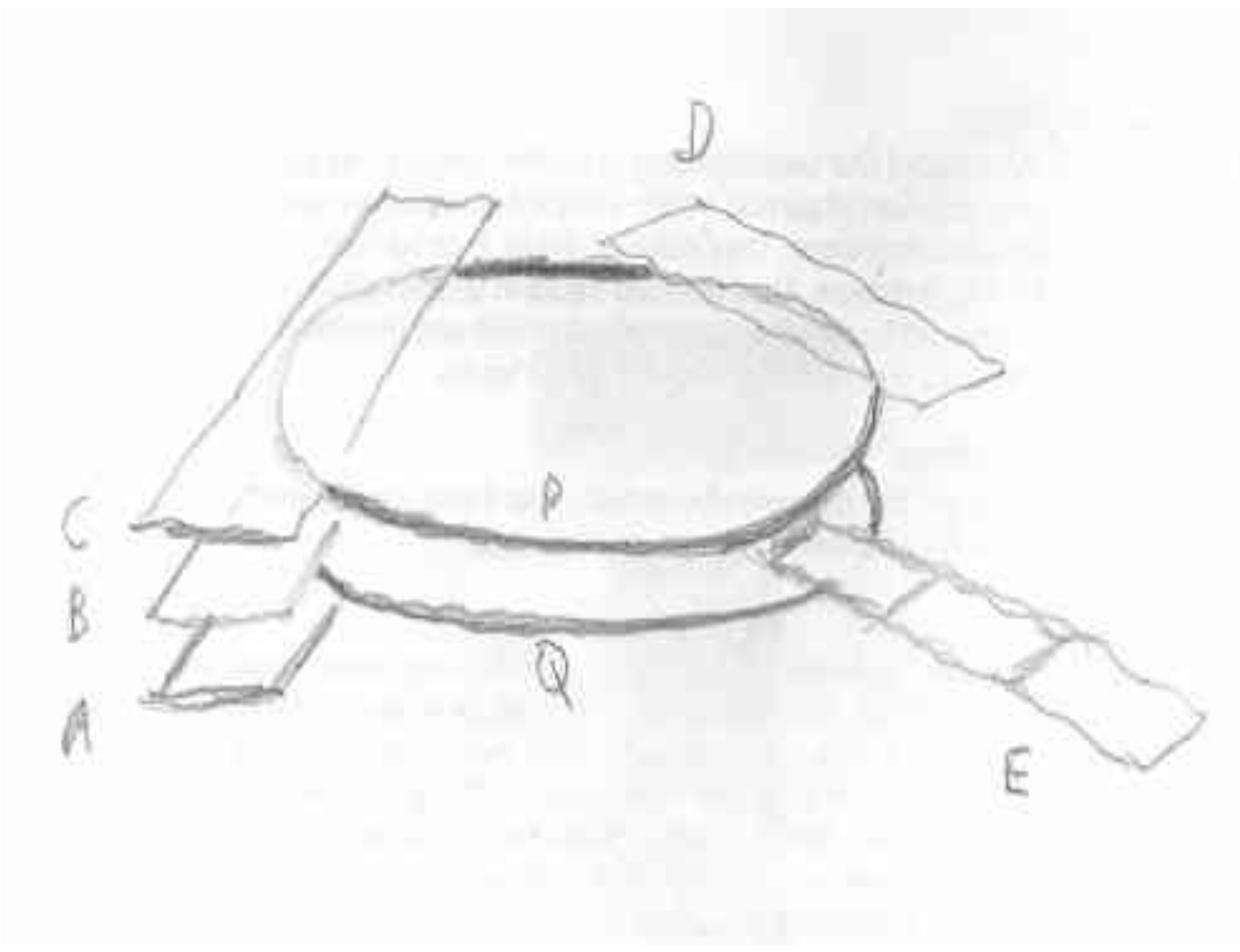

Figure 26

A tiny cluster. Magnification 200. The field shown is about 0.2 by 0.26 mm. This cluster was found on the side facing the electrolyte.

- -

Chips A, B, C and D are the same as those shown in Figure 8; they were used in experiment #2. I know that the average number of pits per surface, after the experiment #2, was 32. The chip E on the other hand was never used; it was the last one from a set sent to me by Pamela Boss. The expected background is 15 pits per surface.

63) Uranium dust?
Marissa's website:

http://www.earthtech.org/PACA/logbook.html

shows what happens when alpha-radioactive dust particles are deposited on the CR-39 surface. Each dust particle creates a set of tracks similar to our clusters. It might mean that the NAE created during the electrolysis is not deposited uniformly over the O-ring surfaces. But it can also mean that some kind of radioactive contamination, for example uranium or radium, is present on seeded rings sent to us by Oriani. If this were the case then a "seeded" ring would remain "seeded" for a very long time. Oriani wrote that seeded pieces loose their potency after several days. That seems to rule out uranium or radium. What about Rn-222 whose half-life is 3.8 days, or Po-218 which is constantly recreated from Rn-222 at the rate of 3.8 days? There are several such possibilities, such as Ra-224, Po-216, Bi-212 and Po-212. My ongoing test (see Figure 26 above) could be repeated but with time intervals longer than 8 hours. But a silicon detector would be a much better tool for this kind of investigations. It would show which isotopes are present and in what relative amounts (on the basis of energies and half-lives).

64) June 3, 2007
Chip E has just been moved to the third (last) position. The field that was on the right side in Figure 26 is now between the two O-rings. The test will end in 8 hours and the experiment #4 will be started. Meanwhile I was thinking about the radon-like contamination again. Which experimental facts is it consistent with and which facts are not consistent with it?

a) Experiment #2 lasted four times longer that experiment #1. But it did not produce about four times more large clusters. The might be interpreted as an indication of the radon-like effect. Clusters are due to dust particles whose alpha radioactivity goes down exponentially with the half-life of several days.

b) If that interpretation is valid then I should see even smaller number of large clusters in the experiment #4. After all, it will be started 27 days after the O-rings were received. Perhaps experiment #4 should also last several weeks. If clusters are due to original contamination with something that decays like radon then the experiment #5, stared two months after the experiment #1, should have no clusters at all. On the other hand, I should still observe large clusters, in the experiment #5, if they are due to something alpha-radioactive that is recreated in the cell during the electrolysis.

c) Oriani did observe large clusters in about twenty consecutive experiments, each lasting about 4 days. That would be a good argument that clusters are due to a nuclear process induced by electrolysis, provided one can be sure O-rings are not recontaminated with radon-like something, after each experiment. It would be interesting to know how many clusters were actually observed after each of his experiments. Fortunately, his CR-39 pieces contain this information. I am sure Richard keeps them well organized and labeled.

d) In a private message received today, someone wrote that an X-rays film could be used below the CR-39 chips during Oriani-type experiments. " If you see any pattern in the fogging that matches the damage (pits) in the CR39, it will be very difficult to explain the pits as chemical or mechanically induced artifacts." Logically this makes sense. But the X-ray film is not sensitive enough, unless one wants to detect millions of photons. Using thick nuclear emulsions, instead of CR-39 chips, could be useful. But this is not trivial. In any case, I have no experience in working with thick nuclear emulsions. Silicon detectors would be much more appropriate replacement for our chips.

65) The 24-hours-long test ended and the experiment #4 started at 16:30 (6/3/07).
Nothing is different in comparison with the experiment #3, except the cathode wire was slightly reshaped, to make sure the center column (in the heat shrink) is not the lowest point of the foot. I am concerned about the moisture. The nuts were turned a little tighter. I will now start etching all five chips used in this test, and three chips used in the experiment #3. The large chip (3 by 3 cm) used in the experiment #3 was squeezed between the O-rings while two small chips (2 by 0.9 cm), used in this experiment, were in the air, outside the cell. As in the experiment #2, they were slightly above the plane of the main chip (say 3 mm). One of them was 45 to 65 mm from the cell axis and another was 75 to 95 mm from the axis.

Only one large CR-39 is used in the experiment #4; it is my last CR-39 piece. New 6 micron mylar is used in experiment #4. And the exposure to Am-241, at the little corner, took place before the electrolysis. Before etching I will expose another corner as well. This might become important in comparing sizes of post-electrolysis pits with sizes of pits due to alpha particles from my Am-241. Starting voltages 12.1 V on the power supply and 6.4 V on the cell (because I am using 41 mA current regulator, as in experiment #3).

66) Time = 5 hours (Sunday, 6/3/07, 21:30)
The experiment #4 has been running nicely but I just discovered a mistake. The mylar film has been positioned below the CR-39 chip, rather than above it. In other words, the chip is not protected from the electrolyte, the cathode foot is resting on the chip. Should I stop the experiment and restart it after placing mylar into the desired position? That is what I would do if it was my first experiment. But it is my third one and I already believe that experiments are reproducible on demand. Why should absence of mylar prevent me from seeing clusters, even near the center? I will certainly be able to see clusters outside the O-rings, and between the O-rings. And I might learn something interesting from this mistake. The experiment #4 is not as important as the experiment #3; its main purpose was "to keep going' so that kindling will be available when I decide to do something else.

67) Time = 30 hours (Monday, 6/4/07, 22:30)
Chips from the experiment #3 Not a single large cluster (above 5 tracks) was found. The total number of tracks was also found to be much smaller that in the experiment #1. Each of the two small chips, that were positioned outside the cell, showed only 7 tracks on two surfaces. That is much less than the expected background. Note that the background is known to fluctuate a lot from one chip to another.

The total number of tracks on two surfaces of the main CR-39 chip was found to be 188. That corresponds to the mean density of about 10 tracks/cm^2. The expected background density is 15 tracks/cm^2. The corresponding number of tracks in the experiment #1, excluding about 100 in clusters, was 636 (mean density 35 tracks/cm^2. The test described in Figure 26 above also did not show excess tracks, in comparison with expected background. These results, and the fact that experiment #2 that lasted 21 days did not produce more clusters than the experiment #1, that lasted only 5 days, are rather significant. They seem to indicate that Oriani's seeded O-rings, responsible for creation of cascades in the first experiment, lost their potency shortly after the experiment #2 was started. The etching was for 6 hours at 74 C, as before, and alphas from Am-241 produced usual pits, But I will etch the big CR-39 for another 6 hours, together with the chip from the experiment #4 (that started yesterday). I do not expect any excess tracks from experiment #4. But I will look for them.

68) An overview of the situation:
a) Are Oriani-type experiments reproducible on demand? Ignoring my experiment #3 one should say yes. But there is vexing problem; the first step of the protocol calls for obtaining a set of seeded O-rings from Richard Oriani. We cannot say "start with commonly available components, follow our footsteps and you will observe clusters of tracks." The demonstration of an electrolytically-induced nuclear process cannot be said to be independently replicable unless preparation of seeded O-rings is also replicable.

b) Seeded O-rings are produced from virgin O-rings in Oriani's electrolytic cell. There is something peculiar in that cell; an attempt to make seeded O-rings in another cell has a very low probability of success. Richard thinks that something rare happened to the cell during one of many experiments conducted by him in the last five years. But he does not know what it was. Investigations of Richard's cell, and of his process of seeding the O-rings should be given high priority.

c) Numerous tests conducted by Richard demonstrated that virgin O-rings, placed on CR-39 chips for several days do not produce excessive tracks of nuclear particles. But seeded O-rings, under identical tests, show presence of excessive tracks. Oriani says that seeded O-rings acquired "residual radioactivity." What kind of track-forming particles are emitted? Is the residual activity permanent or does it decrease with time? Dedicated experiments to answer these questions must be designed. I take it for granted that nothing was alpha radioactive in Richard's cell before it acquired the ability to produce seeded O-rings on demand.

d) Suppose a replication experiment is conducted by a researcher who received a set of seeded O-rings from Richard, as I did. The total number of tracks, after the electrolysis is N(tot). Let N(electr) be the number of tracks due to the electrolysis in the replication experiment. Then

N(electr) = N(tot) - N(res) - N(b)

where N(res) refers to tracks due to residual activity and N(b) refers to tracks due to the background. To show that electrically induced emission of nuclear particles is real one must show that N(tot) is much larger than the N(res) + N(b). Showing that N(tot) is much larger than N(b) is not sufficient. But that is what I was doing so far. That is why investigations of residual activity must be conducted before investigations of electrically induced radioactivity. None of the replicators was able to succeed in a replication experiment without receiving a seeded cell from Oriani. But each replicator can study residual activity.

69) My new results and etc.:
a) Experiment #4 was terminated this morning, after 64 hours of electrolysis. Then the CR-39 chip was etched for 6 hours, as usual. As expected, nothing was found but very low background; only 24 tracks on one side and 20 tracks on the other side. That translates to the average track density of about 2 tracks/cm^2.

b) The already etched main chip from the experiment #3 was etched for additional 6 hours, as suggested by Oriani. The tracks became larger but the total number of tracks, on both surfaces, was found to be 222. This is not significantly different from 188, after the first etching.

c) Today I ordered a CR-39 sheet for a next sequence of experiments. These experiments will probably be started next week, after a fresh set of seeded O-rings is received from Oriani. Marissa and Scott Little also forging ahead with new experiments, as described at their logbook-webpage at:

http://www.earthtech.org/PACA/logbook.html

70) Continuation of this electronic logbook will be in unit #329.
Let me end this unit by describing what happened yesterday. I was etching CR-39 chips in the chemistry lab. A colleague said “you are wasting time on this project.” My reply was that trying to resolve a scientific controversy is not a waste of time. He looked at me and said: “this controversy has already been solved.” I asked: “Do you know what I am studying?” He said “yes, this is another study of cold fusion.” I said that what I am doing is not cold fusion. Then I told him about tracks of nuclear particles. Nothing of that kind was investigated by Fleischmann and Pons. After that he stared listening and admitted that this is not a waste of time. This little episode confirms that many mainstream scientists are preconditioned to reject anything that they associate with cold fusion. Our investigation of Oriani effect has nothing to do with excess heat, with fusion of deuterium nuclei, or other effects that are, unfortunately, labeled as cold fusion. Each effect should be studied on its own merit, till connections with other effects are demonstrated. Being labeled a cold fusion researcher is highly undesirable.

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328) Recent observations and comments

Ludwik Kowalski; 5/14/2007
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

1) My second replication experiment, described in the unit #327, is in progress. It will run for many weeks. It is remarkable how reproducible everything is, as far the experiment is concerned. The current remains stable, as it should, but the cell potential decreases slowly as more and more water is lost by the electrolyte. Relicability of that kind is expect from all experiments in physical sciences -- one does the same things and results are the same. Some fluctuations are unavoidable because of the limited precision of our instruments. But will I see at least as many clusters in my second experiment as I saw in the first? That remains to be seen.

2) Here is what Scott Little wrote, about ten days ago, about excess heat on a private list for CMNS researchers. "[Someone asked how confident are we that the high rates of excess heat are real and nuclear?] In our case, not confident at all. We have never seen a real excess heat signal in our lab. But we have seen plenty of calorimeter artifacts that could easily be mistaken for excess heat signals. In a depressingly small number of cases, we have had the opportunity to perform our measurements side-by-side with the original investigator. In these cases alone we were usually able to pinpoint the error that led to the original excess heat claim. In a few instances, "the effect" would not appear while we were waiting to measure it. In the rest of the cases, all we can really say is that the experiment didn't work when we tried it. Our website http://www.earthtech.org contains a number of reports of these null results. Some people have accused us of trying to disprove cold fusion. That's ridiculous. Cold fusion does not need any help from us to continue being largely ignored by mainstream science.

What cold fusion really needs is a reliable demonstration experiment. That's what Ludwik Kowalski has been trying to find for several years now. We worked closely with him to see if Mizuno's incandescent W experiment (a la Clauzon et al) would do the trick. It didn't. That's what the Galileo Project was trying to establish. It didn't...or hasn't yet. That's what we are still pursuing right now in cooperation with Richard Oriani who continues to see relatively low level positive results from his PACA CR-39 experiments. That's why we continue to maintain our high performance calorimeter, MOAC, in good working condition so that cold fusion researchers can take advantage of our standing offer to test promising cold fusion cells free of charge.

Why go to all this trouble if we've never seen any real signs of cold fusion? Simple. If cold fusion is real, it will be of enormous importance to mankind. Lots of discoveries in science have come only after years of searching. Despite all our null results we still have some hope. But I also think that there is a finite chance that all of the apparently positive results that have been observed in cold fusion experiments are erroneous. i.e. the result of various artifacts. I know, it's hard to imagine how so many intelligent researchers could all be making such measurement errors. But it would at least explain the notorious difficulty of replication that cold fusion suffers. "

3) In message, posted a little earlier, another CMNS researcher, Jacques Dufour, observed: "……..It is very difficult to make a table containing ALL the result, because researchers have a natural tendency not to take into account unexpected results, just because they think that their experiment has to be improved. This is apparently perfectly honest, but we cannot exclude that the unexpected results are in fact true. ……" I find this observation very interesting. Not reporting results which cannot be trusted due to mistakes is natural and desirable. But this should be true for all results, not only for results conflicting with expectations. Otherwise the "perfectly honest" is indeed only

apparent. It is very difficult to be perfectly objective. That is why all important claims should be checked in different laboratories.

4) At the end of his message John Fisher wrote " I certainly believe that Oriani's O-rings contained radioactivity. It is this radioactivity, occasionally releasing polyneutrons, that ignites a reaction in a new setup." Hmm, I have two comments about this. Here what I posted on the private discussion list for CMNS researchers:

(a) The term radioactivity is well defined. In the context of our task, it stands for the emission of alpha particles from substances such as radium, radon, uranium, etc. John is using the same term for something different. I have seen a lot of unnecessary debates due to the fact that people assign different meanings to the same word. Perhaps John's process (releasing of polyneutrons) deserves a different term, at least for the time being. My suggestion is to use the noncommittal term NAE (Nuclear Active Environment), invented by Ed Storms.

(b) What is the best strategy to convince mainstream scientists that CANR are real? I think that the issue is worth discussing. My advice would be to ask all theoreticians -- including John, whose theory inspired the protocol based on starters -- not to inject theoretical interpretations until facts are recognized as real. Remember what happened in 1989. Instead of focusing on real experimental facts (generation of excess heat) discussion quickly shifted to theoretical considerations, such as coulomb barrier, expectations based on wrong models, etc. It would be much better if the F&P phenomenon was called UEH (unexplained excess heat) rather than CF (cold fusion), until the reality of UEH were recognized by all scientists.

If it were up to me I would recommend focusing on our new experimental facts. Let us agree that clusters of tracks are not due to artifacts, such as radioactivity or cosmic rays. Let us agree that clusters are likely to be due to unexplained nuclear projectiles (UNP). Then let us try to convince others that UNP are real. Trying to mix experimental facts with theories might backfire again. We want people to look at our experimental data; we want them to perform experiments; we do not want the debate to shift toward the as-yet-unaccepted ideas, such as polyneutrons, etc. I know it is a touchy issue. Theoreticians do not want to be told what to do, what to publish and how long to wait. And we all believe that pure empiricism is not science. Theoretical debates are essential. But, like other powerful tools, theories can have both positive and negative effects. I am afraid that premature theoretical considerations can produce more harm than good at this delicate stage. What do other people think?

5) In another message I wrote: I am not suggesting another acronym but in the future CANR might be replaced by CEMANA (chemically, electrically and mechanically assisted nuclear activities). In my mind forced diffusion of deuterium, like in Iwamura's experiments, is a mechanical process. And the term "activity" is less restrictive than the term "reaction." Alpha radioactivity of uranium, for example, is not a reaction. I suspect that clusters are produced in bursts of activity. A silicon detector, connected to a scalar (or to a MCA) would be an ideal tool for checking this hypothesis.
P.S.
I my opinion elementary events should be clusters of tracks, not individual tracks. What kind of artifact can possibly be responsible for a cluster? Natural alpha radioactivity from point-like sources. Control experiments, to test for absence radioactivity, are not difficult and we make them routinely. As suggested by someone else, cosmic rays do not produce clusters at the rate of several per week. Such clusters would have been discovered by CR-39 dosimetrists long time ago. If it was not for the widespread prejudice, and for significance of our claim, our data would be sufficient to convince all honest skeptics.

6) Here is how John Fisher responded to my suggestion about short-term strategy: " The idea that neutral particles of a novel type play a role in CANR reactions has proved fruitful. It encouraged Oriani to look for and find charged particles generated in the vapor over the electrolyte in an electrolysis cell (ICCF11). It encouraged Oriani to look for and find charged particles generated in the air beyond the cell wall (ICCF11). It prompted the suggestion that a bit of material exposed to the reaction in one laboratory might serve as a starter for igniting a reaction in another laboratory. Whether or not polyneutron theory is correct, it has proven to be useful by suggesting these procedures. Progress is faster when theory and experiment go hand in hand. They learn from each other and they teach each other. It would be a mistake for theoreticians to remain silent."

7) Replying to the above, X wrote: “Well said John. Scientific method although surprisingly rarely employed in any field has pretty good parentage and has scored some notable successes. As I understand it one should operate with a tight and closed loop connecting experiment and theory; it is a symbiotic process. The only reason to perform an experiment is to test a(n) hypothesis*. I suppose that means that the only reason to formulate a(n) hypothesis is to define conditions of an experiment.”

8) And here is what I wrote about the above comment: In the final analysis, X is correct. The long-term goal is to know what happens, and to understand it in terms of what is already known and accepted. My suggestion had to do with strategy. It is better to first offer what is easier to defend. Experimental data are easier to defend than polyneutrons. But I am only an observer. Let us hope that the 1989 situation does not repeat itself. I am afraid that people will start discussing polyneutrons instead of performing and discussing experiments. I will be easier to defend polyneutrons after existence of clusters, predicted by John Fisher, is accepted by mainstream scientists.

9) Responding to my comment William Collis posted this message: “Yes it will indeed be easier to defend poly-neutrons. As someone who has defended that point of view since I first heard John Fisher's presentation on the subject at ICCF4 in 1993, that will be a relief! In those days, there were very few explanations other than variations on d-d fusion. I'll confess I was subject a certain amount of ridicule when I made a presentation on it in 1994 (at Siena, Italy)! Having said that, it may well be that the poly-neutron theory is wrong, almost certainly in detail, quite possibly in the basics. The theoretical question we need to ask is, "What are the minimum characteristics necessary for these neutral particles in order to explain clusters, hot-spots, seeding, transmutations, helium, tritium, neutrons and few penetrating Xrays / gammas? That's quite a tall order!

I've taken you up on your challenge to write a tutorial on another class of Exotic Neutral Particles (ENP) - Bazhutov's Erzions and I shall include in my ICCF13 paper. I dare say that come the Catania Workshop in October I shall make some drastic changes to the Bazhutov's parameters which will satisfactorily explain the lack of detectable beta decay (no Bremstrahlung, no gammas) which is currently a problem. No model is perfect. It may be that a fusion of multiple ideas will be closer to the truth. Or it may be that magnetic monopoles are the elusive ENPs we are searching for. What we need, is theoretical guidance on how to distinguish these models experimentally. Both Erzion and poly-neutron theory suggest that beryllium should be an excellent fuel and generate energetic alphas. (Why not place a Be foil on the CR39?). You can distinguish the 2 models by examining the Be with a geiger counter afterwards (I won't tell you what to expect!)”

10) Replying to a suggestion, made by another CMNS researcher, to modify our experiment, I wrote: “I think that this is a good idea, as far as trying to understand what is going on is concerned. But that is not what our primary goal should be at this time, in my opinion. The short-term goal should be to convince ourselves, and then to convince others, that what has already been discovered is a new kind of nuclear activity induced by a chemical process, rather than an artifact of some kind. In that context, attempts to replicate identical experiments by eight TGP teams, and by other CMNS researchers, seem to be more important. My suggestion is to focus on potential artifacts in existing setups. Knowing what is going on seems to be more important, in the present situation, than trying to understand it. The coordinated assault should continue. Do you agree?

11) Responding to the above, Andrew Muelenberg wrtote: “Yes and no! While confirming an effect is high priority in the present context, understanding what is going on may make a major impact of establishing the critical conditions for reproducibility. It may make a difference as to how the work is received. It is easy for us back seat drivers to make suggestions on how the experimenters should use their time. If we can usefully contribute time / money / moral support / theories, then we can make suggestions. The extra chips are cheap, easy to place, and would not change the present operations. However, they do require documentation and "reading." Both of those items cost the experimenter dearly. Are there areas in which the experimenters would like or could use assistance? “

12) Yesterday I suggested: let us produce a list of potential artifacts, as far as clusters are concerned. Then we can discussed experiments that will help us to demonstrate that what is observed is not an artifact. Clusters are "easier to sell" than uniform distributions (that are not much more intense than the background). . . .

13) I already commented on what William Collis wrote. Let me add to this. It is always more desirable to explain facts

in terms of what has already been accepted. Explaining facts in terms of unexplained ideas seems to be counterproductive. But this is not something unheard of. I am thinking about the famous paradox of missing energy in beta decay. Calorimetric measurements of mean energies per beta particle, conducted in 1930s, were not consistent with the law of conservation of energy. To explain these experimental results, Pauli invented neutrino, a particle of negligible mass that carries the missing energy. I suppose that many people had reservations about this, just like many of us resist explanations based on polyneutrons, erzions and magnetic monopoles. But Pauli's hypothesis was eventually shown to be correct by Cowan and Reins (1950's).

In my opinion, people who are not fluent in advanced physics, like myself, are not in good position to validate exotic theoretical speculations. But most of us are interested in what high caliber experts think about explanations based on ad hoc assumptions. Explanations will be validated not only on the basis of their logical correctness but also, and mostly, on the basis of experimental facts. But first reality and reproducibility of facts should be recognized by mainstream scientists.

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329) Continuation: Oriani's effect in Phase 2

Ludwik Kowalski; 6/14/2007
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1) Get ready for more experiments:
This is my electronic logbook for data reporting, and for related comments. It is the continuation of my electronic logbook from the unit #327.

2) Below is a message that I just posted on a private list for CMNS researchers. It describes a plan to continue replications of Oriani's experiments. Hopefully, some progress is going to be made to learn about two topics; (a) what is the time dependence of residual activity is seeded O-rings sent to me by Oriani, and (b) are clusters of tracks due to electrolysis or are they due to what Richard calls residual activity.

Richard is going to send me two seeded O-rings, each between two large CR-39 detectors. As soon as detectors arrive (next Tuesday, probably ~24 hours after they were applied to seeded chips) I will remove the chips, and start the experiment #5, with a fresh detector. At the same time two fresh detectors will be applied to the O-ring #2, for the duration of the electrolysis (4 days). During the electrolysis the O-ring #2 will be kept between two fresh detectors. After the electrolysis all seven CR-39 chips will be etched in the same bath for 7 hours.

Suppose I see clusters on CR-39 after the electrolysis and no clusters on CR-39 chips that were applied to the O-ring #2, at the same time. Then I would be able to say that clusters were electrolytically-induced. The other extreme would be "no significant difference between the three chips." This would mean that the electric current in the cell is not responsible for the formation tracks; I would conclude that all excess tracks are due to what Richard calls "residual activity." Information about time-dependence of residual activity (NAE?) will also become available. Does this logic make sense?

Comments, criticism and suggestions will be appreciated, as always. Details will be reported in the on-line logbook (unit #329), as they were when my first four experiments were in progress (unit #327). I am going to follow Richard protocol, as before. I consider this protocol to be preliminary because its first step consists of asking Richard for seeded O-rings. What is needed is a protocol which tells people how to produce seeded rings on demand.

3) Terminology and expectations:
Clusters of tracks on a post-electrolysis chip might be due to three kinds of processes: (a) natural background (cosmic rays and alpha-radioactve contamination), (b) residual activity discovered by Richard Oriani, and (c) electrolysis in a replication experiment (in a cell with seeded O-rings). Contribution of natural background was shown to be negligible in numerous experiments conducted by Oriani (see Table 1 in unit #327). Are alpha-like particles emitted during the electrolysis due to residual activity or are they due to a different process? Assuming that two different processes are involved, I am going to write

$$N(tot)-N(backg) = N(res)+N(electr)$$

where N(res) is the number of particles, or number of clusters of particles, due to residual activity and N(electr) is their number due to electrolysis. On the left side N(tot) represents the total number particles recorded during an experiment while N(electtr) represents the number of particles due to the electric current only. My experiment #5 is expected to

provide information about the N(res) and N(electr). Three distinct outcomes are possible:

(a) N(tot) is not significantly different from N(backg).
(b) N(electr) is negligible in comparison with N(res)
(c) N(electr) is not negligible comparison with N(res)

Note that (a) would exclude anything unusual while (b) or (c) would confirm that at least one CMNS process was taking place during my replication experiment. The outcome (c) would be a clear indication that two CMNS processes were contributing to emission of unexpected alpha-like projectiles.

What does the term "seeded O-ring" refers to? Oriani was studying emission of alpha-like particles, during the electrolysis, for many years. Then he discovered that such particles are also emitted from the O-rings removed from a cell, after electrolysis. That emission was named "residual activity." Why is an O-ring exhibiting residual activity is said to be "seeded"? Because it can be used to replicate Oriani's results in another laboratory, as recently demonstrated. Note that the word "seed" is used to describe an unknown prerequisite for a replication success. In that sense "seed" is not necessarily the same thing as "residual activity," or vice versa. A large number of reproducible-on-demand experiments would have to be performed before things become clear. Inventing names is not sufficient.

4) Replying to Scott Little; 6/17/07:
In a private message Scott Little wrote: "In my search for the sensitivity of CR-39 to radon, I saw several mentions of the problem of radon progeny (decay products) sticking to the CR-39 surface and influencing the track count. It is mentioned in this
paper: www.cityu.edu.hk/ap/nru/pub_j20.pdf . This raises the following question: Is it possible that active o-rings are o-rings that are contaminated with radon progeny? Here is my reply; the CC was sent to R. Oriani and J. Fisher.

"It is true that residual activity, that appears when a virgin O-ring is used in Richard;s cell, is likely to be the central issue raised by referees. Richard's task will be to convince them that what he named "residual activity" is a new CMNS process. They will probably ask "how do you know that it is not an alpa contamination activity?" Here is what I plan to do to address this question, in the next week or two.

a) The two seeded O-rings will probably arrive on Tuesday, each between two CR-39 detectors. Let me name these detectors D1, D2, D3 and D4. I will remove them from the O-rings after 36 hours of exposure (most of it during the delivery).

b) At that time I will start the electrolysis experiment, with a fresh chip, using one seeded O-ring. This will give me the detector D5, after 4 days of electrolysis. Hopefully, D5 will show several clusters, as in my first experiment .

c) During the electrolysis I will study residual activity of the second seeded O-ring. That O-ring will be kept between two fresh chips for two days, and then with two other fresh chips for the next two days. This will give me chips D6, D7, D8 and D9.

d) After four days I will start the second electrolysis experiment, also lasting four days. This will give me the D10 chip, after eight days. Hopefully clusters will again be seen in D10.

e) I will continue studying residual activity during the second electrolysis experiment (applying two fresh chips to the second seeded O-ring every two days). This will give me chips D11, D12, D13 and D14.

f) All chips will be etched at the same time, and in the same bath, after eight days. Will the rate of excessive track formation (tr/cm^2 per day) remain the same or will it be decreasing exponentially with time? To rule out a possibility that Rn-222 is responsible one must show that experimental data are not consistent with the 3.8 days half-life.

g) Unless nothing but the background is observed after the second electrolysis, I plan to continue. Details will depend on results. For example, another 4 days of electrolysis with D15 and two additional data points on residual activity, with D16, D17, D18 and D19, during the same time. The goal will be to learn about the time dependence of residual

activity, using the O-ring #2, and to collect data on clusters during consecutive electrolysis experiments. What else should I do, or how should the plan be modified, to maximize the amount of useful information? P.S. Scott, you asked about Rn-222 atoms sticking to CR-39. That could produce a star with four nearly overlapping tracks (from Rn-222, Po-218, Po-214 and Po-210). But large clusters of separated tracks cannot be explained in that way, unless sticking atoms are clustered, for example, on electrically charged islands. ”

5) The seeded O-rings will arrive today (6/19/07):
a) I will be studying two things at the same time: residual activity (outside the electrolytic cell) and electrically induced activity (in the electrolytic cell). There will be many chips and I must be able to correlate relative positions of chips with respect to O-rings. A system to accomplish this has been invented. This is particularly important in studying of residual activity (because some parts of O-rings might be more active than others.

b)Another important thing is to minimize contributions of radon. This should not be a problem on 27th floor, unless cement walls contain uranium. According to a 2005 Korean paper -- “Construction of an environmental radon monitoring system using CR-39 nuclear track detectors;” by Gil Hoon Ahn and Jai-Ki Lee -- a CR-39 chip exposed to air gains 4 tr/cm^2 each day, when the radon concentration is 1 pCi/L. At a typical 3 pCi/L concentration, the excess would be 60 tr/cm^2, after five days. In my experiment, the accumulation of tracks (both in air and in the electrolytic cell) will be near the open window, in a ventilated room, at the 27th floor. This is the best I can do to minimize the exposure to radon. I will measure the preexisting background in the newly purchased CR-39. Suppose it is 2 tr/cm^2. Suppose the control chip, exposed to air for five days, shows 10 tr/cm^2. That would tell me that the mean radon concentration is close to 0.4 pCi/L. I expect it to be lower than this. But that remains to be seen.

c) After removing a blue plastic, protecting a chip, I will discharge it in jar with salty distilled water. Then the chip will be rinsed in pure water and used in an experiment. And then what? Ideally, a chip should be etched as soon as possible, to prevent accumulation of post-experiment tracks. On the other hand, one may prefer to wait and etch all chips together. What is better? I think that etching as soon as possible is more important. That is what I am going to do.

6) Tuesday, 6/19/07 13:00
The experiment #5 was started. One hour later experiment #6 started. I am performing two experiments at the same time. To avoid confusion, measuring time dependence of residual activity, in the seeded O-ring, will be experiment #5. It is performed outside the electrolytic cell. Looking for clusters of tracks, in the PACA detector below the cell will be experiment #6.

The seeded O-ring #2, received today, had been in contact with two large CR-39 chips for 24 hours. I removed these D1 and D2 chips, and applied fresh D6 and D7 chips to it. These chips will be collecting tracks during the next two days. The seeded O-ring #1 was also in contact with two CR-39 chips, D3 and D4, for 24 hours. These chips were removed; they should give me information about the initial residual activity of the O-ring #1. That O-ring has already been mounted into electrolytic cell, above the mylar and the fresh D5 chip, to start the experiment #6. The current is 41 mA. That is where I am now. The first etching of removed chips will take place as soon as the hot plate, which I ordered today, arrives, probably on Friday. Chips ready for etching are suspended in air, near the open window.

How were the seeded O-rings created in Richard’s cell? They were created during 4 days of electrolysis, in his cell, at 40 mA. A small container with heavy water was in the tube below the CR-39. The lower side of the CR-39 (facing heavy water) was covered by the protective plastic. But a large cluster was found at that protected side of the chip, after the electrolysis. The track density on the other side (facing the mylar and the electrolyte) was only two times higher that the expected background. That is not significant to me, when the mean background and standard deviation are 16 and 9 tr/cm^2, respectively. But the large cluster is an indication that the unexpected process occurred and that the O-rings were seeded.

7) Wednesday, 6/20/07:
The hot plate was delivered today. But its temperature control thermostat is not working. I have to return it. Accumulations of tracks in two experiments are in progress. Nothing abnormal so far.

8) Thursday:, 6/21/07
At 10:00 the D6 and D7 chips were removed from the seeded O-ring #2, after 45 hours of exposure. Fresh D8 and D9 chips were applied to this O-ring at once. They will be exposed to the O-ring for 51 hours. The level of the electrolyte in the cell is still high (~1 cm above the anode). But I am adding distilled water, to replace loses, because I will be away till tomorrow evening. A chip measuring the background was unwrapped today and suspended in air, near the open window.

9) Friday, 7/29/07 (new experimental results):
The chips were etched (in 6.25 NaOH) two times for 6 hour; first without stirring and then with the stirrer. The etching temperature fluctuated between 69 and 79 C.

a) My chip D0' was exposed to the air near my window for 51 hours: The total area of 6 cm^2 (3 cm^2 on each side) had 43+32=75 tracks . This gives the mean density of 12.5 tr/cm^2.

b) My chip D0" was exposed to air for less than 2 minutes (between peeling off the protective plastic and beginning of etching). The total area (of 2*8=16 cm^2) had 104 tracks. This gives the mean density of 6.6 tr/cm^2. But why does one side has 86 tracks while another side, of the same chip, has only 18 tracks? In any case, even 86/8 is consistent with the mean density on the D0' chip. It is also consistent Oriani's background (mean 15 tr/cm^2 and standard deviation of 9 tr/cm^2. Using his standard deviation, I will assume that any mean density below 33 tr/cm^2 is background. That is the threshold of significance, for my chips.

c) Oriani's chips D1, D2 (exposed to the seeded O-ring #2 for the first 24 hours) revealed tracks whose diameters were larger than those due to my alpha particles. This was discovered after my first etching. The chips he applied to O-rings were probably pre-etched for background. Richard confirmed that he often reuses already-etched chips, expecting much stronger signals from residual effects. The first etching for my Am-241 tracks was actually the second etching for his old tracks. How else could the difference in diameters be explained? The chip D1 had 105 large tracks on the side facing the O-ring #2 and 66 on the opposite side. The chip D2 had 90 large chips on the side facing the same O-ring and 79 on the opposite side. This translates into the mean density of 19 tr/cm^2; it is consistent with the background. Smaller tracks, much less numerous, were not counted because I decided to count them after another etching. But distinguishing old tracks from new tracks became practically impossible after my second etching (two partially overlapping distributions).

g) My chip D5 was used in the electrolytic cell. As in the experiment #1 (see item #327), the electrolysis started 24 hours after the seeded O-ring was sent to me by Richard. The electric current was 41 mA and the duration of the electrolysis was 96 hours. No clusters were found on the D5 chip. The area of 9 cm^2, facing the electrolyte, had 107 tracks. The area of 9 cm^2, not facing the electrolyte, had 70 tracks. The mean track density, about 10 tr/cm^2, was not significantly different from the expected background. Absence of clusters on chips used in experiments #2, #3 and #4 was explained (see unit #327) in terms of decreasing "potency of seeds." But this explanation does not apply to the D5 chip; the timing of electrolysis, during which this chip was used, was not very different from that in the experiment #1. Let me add that the post-electrolysis chip D10 was also examined but no clusters were found, only randomly distributed tracks, consistent with the background. The D10 chip, like the D5 chips, was exposed to electrolysis for 4 days (at the constant current of 41 mA). Thus I no longer believe that creation of clusters is reproducible on demand, even when seeded O-rings are used.

h) My chips D6 and D7 were exposed to the seeded O-ring #2, as illustrated in Figure 1 below.

= = = = = = = = = = = = = = =

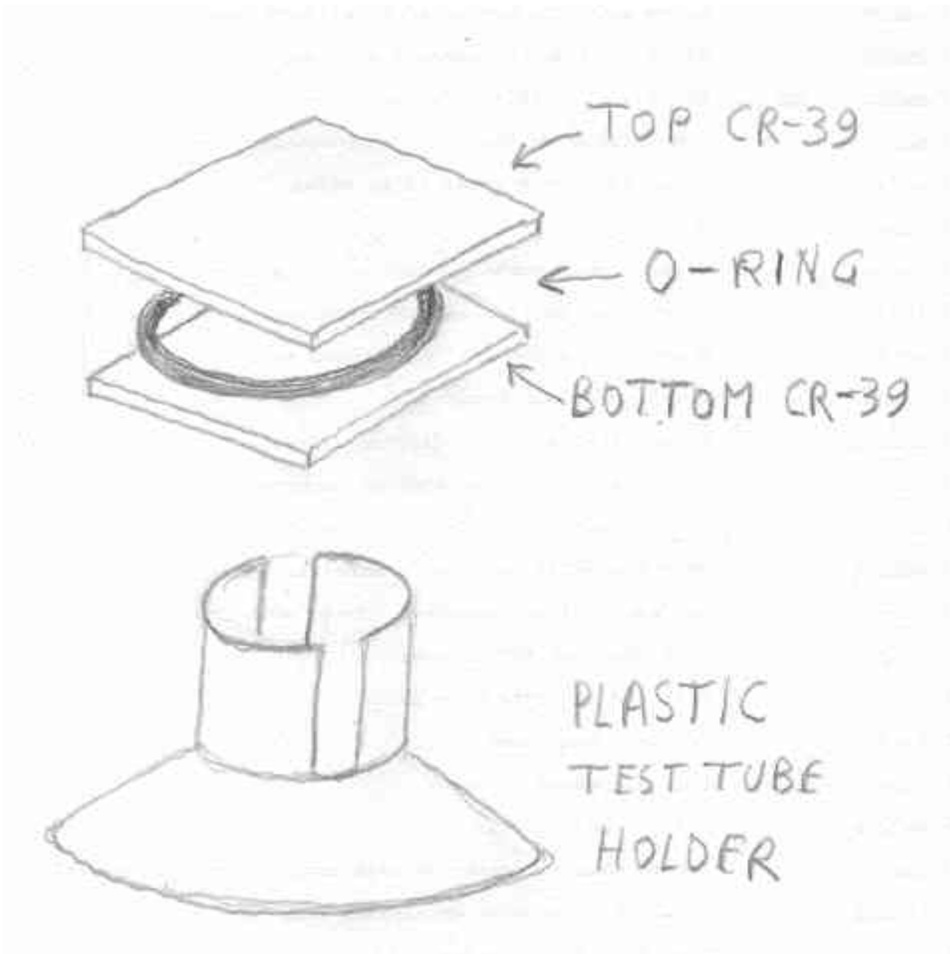

Figure 1
A simple setup to study residual activity in air (without any electrolytic cell).

= =

The D7 chip, supported by a plastic holder, was supporting the O-ring while the D6 was resting on that O-ring. The entire setup was about 15 cm from an open window in a ventilated room. The two vertical slots, in the plastic tube holder, were on the path of fresh air entering the room. This allows me to say that sides of the D7 and D6 chips, that were not in contact with the O-ring, were exposed to fresh air. This test started 24 hours after the seeded O-ring was sent to me by Richard. And it ended 45 hours later. The seeding operation was presumably successful; Richard observed a large cluster on the lower side of the post-electrolysis chip. His mean track density, however, “were in the range of 35 per cm^2.”

The D6 chip produced the following number of tracks: (a) The side in contact with the O-ring had 175 tracks on the surface close to 9 cm^2. This amounts to about 19 tr/cm^2. In other words, nothing significant in comparison with the background, was found. But the other side had many more tracks; I am certain that no mistake was made to identify which side was facing the O-ring. The side that was not in contact with the O-ring had 627 tracks. This translates into 70 tr/cm^2; well above the mean background. To map the distribution of tracks, the entire D6 surface was subdivided into 81 cells (9 rows and 9 columns). The area of each cell was 3.3 by 3.3 mm. The distribution of tracks was not uniform, as shown below.

```
...2...5...4...4...5...5...1...4    4
...5...8...9...3...3...9...4...3...3
..11...7..14...9..13..15...4...3...1
...7...5...4..12..10..33..17...3...1
...8...7...1..12...8..11...6...3...3
...7...2...2...6..11..16...8...1...4
...1...2...1...8..44..47..30...6...5
...2...2...2...1..36..41...5...2    2
```

```
...4...1...3...1...5...2...3...3...0
```

The chips D7, that was applied to the other side of the O-ring #2, had 149 tracks on the side that was in contact with the O-ring. This translates into about 149/8.1=18.4 tr/cm^2; it is not significantly higher than the background. The other side of the D7 chip had 355 tracks over the area of about 8.1 cm^2. This translates into the mean density of 44 tr/cm^2. Once again, it is above my threshold of significance. The distribution of tracks, this time in the form 8*8=64 cells, is shown below.

```
...5...4...6...3...1...5...1...0
...4...6...6..17  12   6   2   6
...5...2...4...6...6...2...5...4
...6...6...5...0...8...1...3...1
...7...2...3..20...4...3...1...2
...6...6...7...3...6...4...5...5
...8..14..30..10...6...4...7...0
...4...8...7...4...4...2...1...1
```

My chips D8 and D9.
These two chips were applied to the O-ring #2 during the next 48 hours (after chips D6 and D7 were removed from the O-ring). The face of the D8 chip that was facing the O-ring had 95 tracks. This translates into the mean density of 95/9=11 tr/cm^2. The other side of that chip had 159 tracks; This translates into the mean density 158/9=17 tr/cm^2. Both D8 mean densities are likely to be due to the background. The chip D9 also had nothing above the background (114 tracks on the side in contact with the O-ring and 163 on the other side).

My chips D11 and D12.
These two chips were applied to the O-ring #2 during the next 46 hours (after chips D8 and D9 were removed from the O-ring). The mean density, from four surfaces, was 9 tr/cm^2. This is not surprising, considering absence of excess tracks on the D8 and D9 chips. The only conclusion one can make is that the half-life of residual activity is not very different from three or four days. This seems to confirm that the "potency of seeds" decreases, as suspected after the failure to observe clusters in the 21-day-long experiment #2.

10) Summary:
The overall situation, however, is far from being clear. My results seem to conflict with what Marissa Little posted in her electronic logbook yesterday.

http://www.earthtech.org/PACA/logbook.html

She thinks that Po-210 is the most likely candidate for tracks they observed. The half-life of that nuclide is 138 days. My track densities, on the D8 and D9 chips, would be about the same as on the D6 and D7 chips, if Po-210, whose half-life is 138 days, were responsible. Another mystery is that my excessive tracks appeared on CR-39 surfaces that were not in contact with the O-ring. Marissa wrote: "Additionally, we have shown that there is no significant change in the activity of the o-rings using the following sequence: run in a cell for two days, place on CR-39 for two days, repeatedly. This sequence was run for 5 iterations. The o-ring activity appears to be about the same for all 10 experiments." Why did my experiments, conducted in the air, yielded the results conflicting with what was observed in electrolytic cells? Additional experiments are needed to answer such questions.

11) Appended on 7/3/07:
Chip D15 was etched and examined yesterday, after 5 days of exposure to the cathode in my PACA electrolytic cell. The third exposure stared 8 days after the seeded O-ring was received from Richard. Neither clusters nor excessive tracks were found. This was expected because first two exposures (chip D5 and and D10) also produced negative results. The number of tracks on the side facing mylar was 106 while the number of tracks on the side facing down was 186. The mean track density, 292/19=16 tr/cm^2 is in good agreement with the background.

Why is my background so high? A proportional relation between the concentration of alpha radioactivity in air and track density has been studied. If the concentration is 1 pCi/L then each day of exposure to air increases the track density by about 4 tr/cm^2. My experiment was performed in presumably radon-free air (near an open window in a ventilated room at the 27th floor). The radon concentration was probably less than 0.1 pCi/L. The exposure to air was

no longer than several minutes (after the protective layer of plastic was removed and the electrolysis started). The radon in air could thus not be responsible for 16 tr/cm^2. The electrolyte I am using, the glass of my cell, and the wires from which the electrodes are made, were tested by me long time ago. Their contributions to the background, after several days of exposure, were found to be negligible. The only explanation I have is that the CR-39, received from Landauer, has higher than expected background. Perhaps airline companies randomly expose their cargo to neutrons, searching for explosives. The CR-39 chips received from Pamela, and then from Steven (to replicate SPAWAR experiments), had about ten times lower bacground.

Appended om 7/4/07)
1) The recent June 8 and June 26 entries in Marissa's logbook are probably not totally clear to everyone who read them at:

http://www.earthtech.org/PACA/logbook.html

What follows is my recent private messages to Marissa and Scott. It shows my thinking about Po-210. But first let me summarize their point of view. Scott and Marissa, please correct me, if necessary.

(2) Suspecting radon related contamination, and knowing that Rn-222 (and its daughters) can be identified by gamma rays, they sent a seeded O-ring to an expert on low-energy gamma rays detection, Dr. S. Landsberger, at the PRC laboratory. The ring was examined with a large high resolution gamma rays spectrometer. The “clean as a whistle” result indicated that gamma rays intensities were negligible in comparison with what would be consistent with the rate at which alpha particles are emitted from the O-ring. That was first interpreted as a very strong argument against the suspected Rn-related contamination. But then Marissa and Scott found out that one of the daughters, Po-210, nearly always emits alpha particles without accompanying gamma rays (actually, gamma rays of 0.80 MeV are emitted but only in 0.0012% of decays). Thus the “clean as a whistle” result cannot yet be used as an argument against a possibility of contamination. The alpha particles emitted by the O-ring, might be due to Po-210, the last radioactive element in the Rn-222 chain. That is the essence of what was posted by Marissa. I do not think that Po-210 is responsible for alpha particles from their O-ring. Here are extracts from my two private messages about this:

3) On Jun 30, 2007, at 8:26 PM, Scott Little wrote:
"The following calculations show that it is entirely possible that the activity of Richard's "hot" o-rings comes from casual contact with bench surfaces in his lab.....Please show me where I am wrong. I don't like this result any more than you do. But Occam's razor tells us that simple contamination with a known-to-be-present radioisotope is a more likely explanation for these tracks than nuclear reactions caused by ordinary electrolysis." Yes, the volume of a column of air whose base is 1 cm^2, and whose height is 250 cm, is 0.25 liters. Yes, if the activity of radon is 1 pCi/L then the column's activity, due to Rn-222, is 0.25 pCi. Each decay of Rn-222 actually leads to emission of three alpha particles (from Rn-222, Po-218, and Po-214) emitted shortly one after another. Then the Pb-210 is formed. The half-life of that isotope is 22 years. That brings a lot of complications.

The assumption that all single atoms of Pb-210 fall on the floor is questionable. I think that they also "fall" on other surfaces, including walls, ceiling, lungs of people, skin and cloth of people, etc. But that is not my main point. I question validity of Scott's idealization that the activity of Pb-210 is the same as that of Rn-222. I do not know what fraction of Pb-210 atoms, created in a room, actually decays into Po-210 in that room. Many things can happen to a lead atom during 22 years. Here is a hypothetical scenario. Suppose the probability that a lead atom reacts (chemically) with human hair is much higher than that it reacts with other surfaces in a room. In that case most of Pb-210 will be washed away when we take showers. The inert radon, on the other hand, does not react with other surfaces and remains in air. In that scenario the activity on Po-210, in a room, will be much lower than the activity of Rn-222. . . . My suggestion was that we focus on the half-life of residual activity, outside electrolytic cells. Is it consistent with that of Rn-222 (as my single result seems to indicate) or is it consistent with Po-210 (as described by Marissa and Scott)? If the residual activity does not change significantly during a week or two than their hypothesis (which might also be a referee's hypothesis) should be taken seriously. Speculations about what might happen to single lead atoms, created in air, will not help us in this context.

4) On Jul 1, 2007, at 12:45 AM, Scott Little wrote: At 10:44 PM 6/30/2007, Ludwik Kowalski wrote: "My suggestion

was that we focus on the half-life of residual activity, outside electrolytic cells." That seems like a very good direction to investigate, Ludwik. But we are going to have to be very careful not to physically alter the surface of the o-rings during this testing. Ideally, the o-ring should be held somehow and the CR-39 pieces placed very close but not touching. That way we would ensure that casual contact with the o-ring was not removing....or adding...radioactive material. The present method of handling o-rings with our fingers and placing them directly onto the CR-39 chips does not seem sufficiently well controlled for this half-life study. Don't you agree?

And we also need to protect the CR-39 from whatever is causing ~ 100 tracks/cm^2 in our lab on all 4 day background tests. Perhaps the o-ring should be placed in a very shallow, small cup so that, when the CR-39 square was placed on top of the cup, it would almost touch the o-ring and it would also completely cover the cup so that a small, static volume of room air would be exposed to the CR-39. We need one of Richard's super-hot o-rings that produces too many tracks to count in several days. That would allow us to make rather short exposures to the CR-39 and still get a statistically significant track count.

Adding 25 tr/cm^2 each day, especially in your low-radon environment (~ 0.6 pCi/L0), is an indication of something very unusual, as you already indicated. At your concentration only about ~3 tracks/cm^2 should be added each day. Therefore, "whatever is causing ~100 tr/cm^2 in our lab on all 4 day background tests" should be taken very seriously. This "whatever" may indeed be the UNA (unexpected nuclear activity) discovered by Oriani and Fisher. . . .

Appended on 8/2/07
Richard sent me a seeded O-ring with two CR-39 applied to it. I removed the CR-39 chips after 5.5 days and applied fresh CR-39 chips to the same O-ring. The second set was removed 6.5 days later. Then all four chips were etched at the same time (6.25 NaOH, 6 hrs, ~73 C)

1) Richard's CR-39 (labeled with one scratch):
a) side that was facing the O-ring had 49 tracks on 9 cm^2; 5.4 tr/cm^2, no clusters
b) opposite side had 42 tracks on 9 cm^2; 4.7 tr/cm^2, 4.7 tr/cm^2, no clusters.

2) Richard's CR-39 (labeled with two scratches):
a) side that was facing the O-ring had 32 tracks on 9 cm^2; 3.5 tr/cm^2, no clusters.
b) opposite side had 56 tracks on 9 cm^2; 6.2 tr/cm^2, no clusters.

3) Ludwik's CR-39 (labeled with a double cross):
a) side that was facing the O-ring had 32 tracks on 9 cm^2; 3.6 tr/cm^2, no clusters.
b) opposite side had 24 tracks on 9 cm^2; 2.7 tr/cm^2, no clusters.

4) Ludwik's CR-39 (labeled with a single cross):
a) side that was facing the O-ring had 21 tracks on 9 cm^2; 2.3 tr/cm^2, no clusters.
b) opposite side had 186 tracks on 9 cm^2; 21 tr/cm^2, no clusters.

Yes, 186 tracks! Should this be taken as a sign of an unusually large background or as a sign that some kind of a nuclear process that took place during the second exposure? The average from my first 3 surfaces is (32+24+21)/9 = 2.8 tr/cm^2. This is about 8 times less than on the last surface. But I did see large background fluctuations in the past. That is why I tend to attribute 21 tr/cm^2 to the background. Why would a seeded O-ring produce particles during the second exposure (on one side of the CR-39 chip only) and not during the first exposure?

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330) Validations and relying on authority

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1) Trying to educate myself on the subject of "burden of proof," I went to Google and found <http://en.wikipedia.org/wiki/Burden_of_proof>. Scrolling to "Science and other uses, I found this short paragraph: "The less reasonable a statement seems, the more proof it requires. The scientific consensus on cold fusion is a good example. The majority believes this can not really work, because believing that it would do so would force the alteration of a great many other beliefs about thermodynamics." The author is wrong; validity of the laws of thermodynamics is not being challenged by cold fusion claims; it is a matter of using a new form of energy. In that respect cold fusion does not differ from fission already used to generate electric energy. The word 'thermodynamics" should be replaced by "nuclear physics."

2) A scientist who claims to discover something is expected to provide evidence for the validity of the claim. Other scientists evaluate the evidence and often challenge it in one way or another. That is how mistakes, when they exist, are discovered. A claim might be withdrawn, or additional evidence is provided before it is accepted. That is how things are supposed to work. Mother nature is not capricious; similar results are obtained when the same protocol is used in different laboratories. Suppose a controversy develops. Experimentalists from the camp A believe that pits are due to nuclear particles while experimentalists from the camp B believe that pits are due to a non-nuclear artifact, for example, a chemical effect. On whose shoulders is the burden of proof in this situation? We have two conflicting claims. The side A is expected to validate the nuclear claim and the side B is expected to validate the non-nuclear claim. The burden of proof is not on one side only.

Now consider a situation in which the team A claims to discover something potentially important and valuable while the rest of scientists, team B, prefers not to be involved. Those in team B say "it is not our claim and therefore it is not our obligation to show that the claim is not valid. The burden of proof is on A." I tend to disagree. Scientists are serving society; most often society supports them in one way or another. Evaluation of claims made by recognized experts is expected from all qualified scientists, not only from those who announced a discovery.

3) Another issue, related to the controversies in science has to do with faith in what experts say and write. The ongoing controversy about global warming triggered a debate, among physics teachers, about reliance on experts. In my opinion, one has no choice but to rely on experts, especially in dealing with topics with which one is not sufficiently familiar. A good example is Colorado2 experiment, described in unit #300. A clear explanation with references was needed to convince readers that our measured excess heat could not be attributed to a chemical reaction. I was not able to produce such demonstration. That is why help from an electrochemist was sought. The paper was not submitted because the expected help did not materialize.

The issue of relying on authority should not be confused with the issue of who is the authority. Some people think that newspaper articles and market analysis brochures are written by knowledgeable people. But this is not always true. What follows is an example of a nonsensical report about a scientific claim made by an ignorant writer. (How do I know that the writer is ignorant? Because s/he did not realize that the iESi technology would violate the law of conservation of energy. If s/he did s/he would address the issue. And also because s/he writes kW/hr. At first I thought it was a typing error. But the same "typo" appeared twice. The unit implies that the quantity described is the rate of the rate of the use of energy. This would probably be noticed by many high school students. But the author is obviously

not aware of this.)

Norwood Foundry, Innovative Energy Solutions agree to power up for clean energy facility.

Publication: Modern Casting
Date: Dec 2004
Subject: Internal combustion engine industry (Alliances and partnerships), Metal castings industry (Alliances and partnerships)
Location: United States

Norwood Foundry Ltd., Nisku, Alberta, Canada, and Innovative Energy Solutions Inc. (iESi), Las Vegas, entered into an agreement to begin the engineering, development and operation of iESi's first power generation plant utilizing iESi's clean energy technology. The new clean energy plant will enable Norwood Foundry, which employs 75, to generate six times (12 MW) more electricity than it consumes (2 MW).

iESi's technology will eliminate the need for fossil fuel-fired equipment, such as boilers, to generate super-heated steam required to drive turbines and generators to produce electricity. This will benefit Norwood, which currently uses induction furnaces to cast both ferrous and nonferrous green sand and nobake components.

The finished plant will apply iESi's clean energy technology to generate a power capacity of 12 MW/hr. for Norwood, which expects to market approximately 10 MW/hr. to external entities. The revenue created through the joint venture project is expected to exceed $6 million annually.

Under the joint venture, iESi will be responsible for the implementation of its technologies, while Norwood will finance the project. ACS Engineering, Calgary, Alberta, Canada, has been retained to provide engineering, procurement and construction services to the project. The plant is slated to be fully operational by the third quarter of 2005.

"As one of the largest consumers of electricity in the Nisku area, the implementation of iESi technologies will allow us to save money and continue our leadership position in the metal casting industry," said Norwood Foundry Director Bart Dornan. "iESi technologies will help us realize incremental income on the surplus power we generate, while being environmentally responsible, since no greenhouse gases will be emitted."

As far as In know, it was a fraudulent claim. The company no longer exists and millions of dollars were lost by naive investors.

Appended on 7/27/07:
What follows is a piece, probably more than two years old, that I found quoted over the Internet. The author is a Canadian nuclear scientist who was working for iESi.

Whitepaper On Plasma Heat and Hydrogen Generator

By Dr. Norman L. Arrison

iESi has acquired the most significant technology of the 21st century through Dr. Hyunik Yang and his team from around the world. Their technology draws on the energy of the atom and converts that energy into useful energy in the form of heat in one device and the splitting of the water molecule into hydrogen and oxygen in another device. Dr. Yang's team has varied in size and composition over a ten year period of research. The consistent aspect of the team has been their international stature and dedication to hard work under the leadership of Dr. Hyunik Yang. Dr. Hyunik Yang is 47 years of age and has a distinguished record beginning with his B.Sc. in Engineering from Korea followed by his Ph.D. and post doctorate degrees at Columbia University in New York. He then had a successful career with Hyundai where he was contracted out to NASA and where he won the Eastman Kodak Award for the best paper and an ASME conference. Dr. Yang then went to Russia where he became a member of the Russian Academy of Science.

With the distinguished scientists Dr. Yang had worked with, they decided to build a unit to produce power for mankind based on the energy in the atom.

The approach they used was brilliant. They used resonate harmonic frequencies to expose the nuclei of atoms so they could put the nuclei together to obtain the energy from the fused product. Their system is inexpensive, safe, and easy to operate and construct. The first plasma device will produce heat by taking water and converting it to steam. This device is expected to be working by late 2004 and an early prototype is already functioning. the early prototype produces 14 times the energy put into it and the final product is expected to produce 200 times the energy going into the unit. The second plasma device is expected in early 2005 and it will use it's energy to split the water molecule into hydrogen and oxygen. This device is already working in an old prototype which produces the hydrogen and oxygen and immediately recombines the two in a hot hydrogen and oxygen flame. The old hydrogen-oxygen device was the first proof that the team had successfully tapped the energy of the atom. It only produced 50% more energy out than went into the device but showed that the energy of the atom was being drawn upon.

iESi got control of this techology through the special relationsip which exists between Dr. [removed] and Mr. [removed] the founder of iESi. Mr. [removed] is married to a Korean attorney and through her got to know of Dr. [removed]'s work. Mr. [removed] was so fascinated with the techology that he helped Dr. Yang with his funding which up to that time had been carried by Dr. [removed] and his immediate family. Because of Mr. [removed]'s assistance for Dr. Yang's work, a close relationship blossomed which has resulted in the formation of iESi as it exists today. We at iESi feel very proud that we are the ones bringing this historical changing technology to the world. Plasma heat generation alone guarantees that the cost of electricity will be stable for all mankind. The hydrogen and oxygen producing technology guarantees a clean planet for humanity. the result is that iESi should be the most significant company of the 21st century.

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An illustration of calibration

Ludwik Kowalski (August 5, 2007)
Montclair State University, Montclair, N.J. 07055

It often happens that a physical quantity in which we are interested depends on several quantities, x, y, z, which we measure. We say that the quantity is a function of (x,y,z). Sometimes this function is known. For example, the density of a cylinder can be calculated from D = mass/volume. In that case the formula is

D =4*m/(3.145*d^2*h)

where m is the mass, d is the diameter and h is the height. The m, d and h are measured and D is calculated. The well known rules of propagation of errors (1) can then be used to calculate the uncertainty (random error) associated with the calculated quantity. If the errors in m, d and h are, 2%, 3% and 1%, for example, then the uncertainty associated with the calculated density is close to 4.8%. A situation is not as simple when a formula to calculate the value of the quantity of interest is not known. In such case a researcher must depend on calibration. What follows describes an example of a calibration for a simple case in which the quantity of interest is a function of only one variable. I will ignore procedures used to deal with propagation of errors (via calibration) when the quantity depends on more than one variable.

Suppose we are studying an alloy of copper and zinc. We know that the resistivity, r, depends on the percentage of zinc, p. We want to be able to calculate p from r, after r is measured, But the formula for p(r) relation is not known. We also want to be able to calculate the uncertainty associated with the calculated p. To accomplish this we prepare six cylindrical samples of the alloys of known p. The values of p, for these standards of reference, are 5, 10, 15, 20, 25 and 30 percent. We measure r for each of these these samples and obtain the following set of data:

p(% of Zn)	5.0	10	15	20	25	30
r(nΩ*m)	31.2	32.7	38.1	53.1	58.4	52.4

For pure copper, at room temperature, r would probably be close to $1.68*10^{-8}$ Ω*m (16.8 nΩ*m) as reported in most textbooks. The linear regression line, for these six calibration points, happens to be:

p = 0.721*r - 14.5

as illustrated in Figure 1a. That is the end of calibration; the linear relation above becomes our formula to calculate p after r is measured. Details about the method used to measue r are not important in this context. The only important thing is the limited precision of measurements.

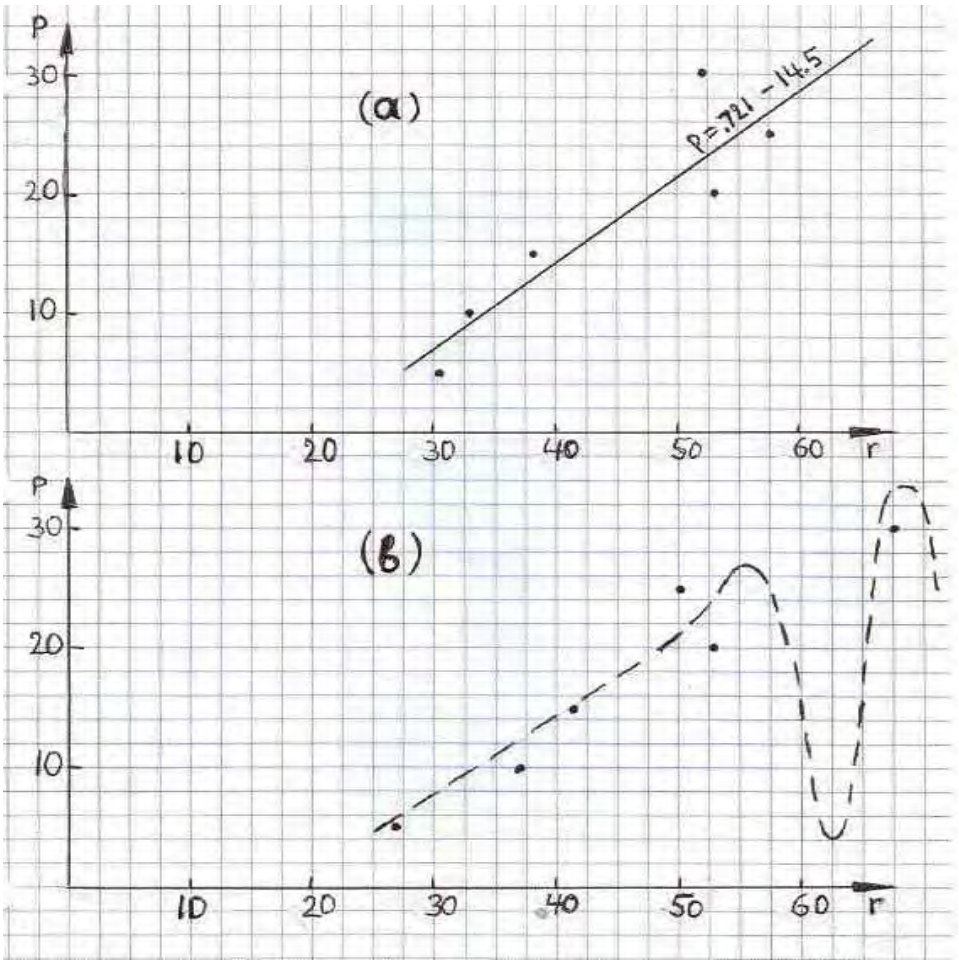

FIGURE 1ab
(a) Calibration scatter plot, p versus r, and the linear regression line.
(b) Another possible scatter plot, after remeasuring r for the same set of six standard alloys. The dashed line shows a possible nonlinear relation between p and r.

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For example, suppose a sample of unknown composition is given to us. We measure its resistivity and the result is 62.0 nΩ*m. What is the percentage of zinc in that sample? Using the above correlation line we find that p is 30.2 percent. And what uncertainty should be assigned to this results? Looking at the Figure 1a, we see that some data points are above the regression line while others are below it. This is due to random errors associated with measurements of r. The six numbers below are vertical distances between data points and the calibration line (see Figure 1a).

dp (% Zn): -3.04 +0.881 +1.98 -3.83 -2.66 +6.67

Such differences are often called residuals. Note that the mean value of residuals is essentially zero, as is should be according to the definition of the correlation line. Positive numbers refer to data points above the line while negative numbers refer to points below the line. It should be intuitively clear that the uncertainty in p would be larger if random fluctuations of data points, with respect to the correlation line, were larger. Smaller fluctuations, on the other hand, would indicate that the random error in p is smaller. The standard deviation, s, of the above six residuals is 4.00. The uncertainty in p, proportional to s, is thus 5.5, as illustrated in Figure 2.

The above method of inference, however, is not universal. Suppose the true relation between p and r is as shown by the dashed line in Figure 1b. That hypothetical relation has a deep minimum near r=0.62. Not being aware of the minimum we would say that p is 30, plus or minus 4 percent. But real p would be much smaller. The method described above should be used only when one is reasonably sure, on the basis of additional information, that the true relation between the variables is not very different from its linear approximation. (Any non linear function can be

approximated by a line segment for a sufficiently narrow range of independent variable.)

The six data points in Figure 1b is the result of another calibration, based on the same six standards as in Figure 1a. The new numerical data are show below.

p(% of Zn)	5.0	10	15	20	25	30
r(nΩ*m)	26.1	35.7	41.1	52.8	50.2	67.6

This time the calibration line turns out to be

$p = 0.622*r - 10.9$

It coincides with the dashed line in Figure 2b only when $r<4.5$. Why is this line slightly different from the one we obtained before? This is due to the limited precision of measuring r. Note that we are assuming that errors in p, for standards of reference, were negligible. According to the new calibration line, the value of p, assigned to 62 nW*m, should be 27.7 percent (plus or minus of approximately 1,5%Zn), rather than 32 percent (plus or minus approximately 4%Zn), as before. Ambiguities of that kind are unavoidable because results of measurements fluctuate around true values. Two kinds of related ambiguities can be recognized in our situation; one about the exact location of the calibration line, and another about values of p based on single measurements of r.

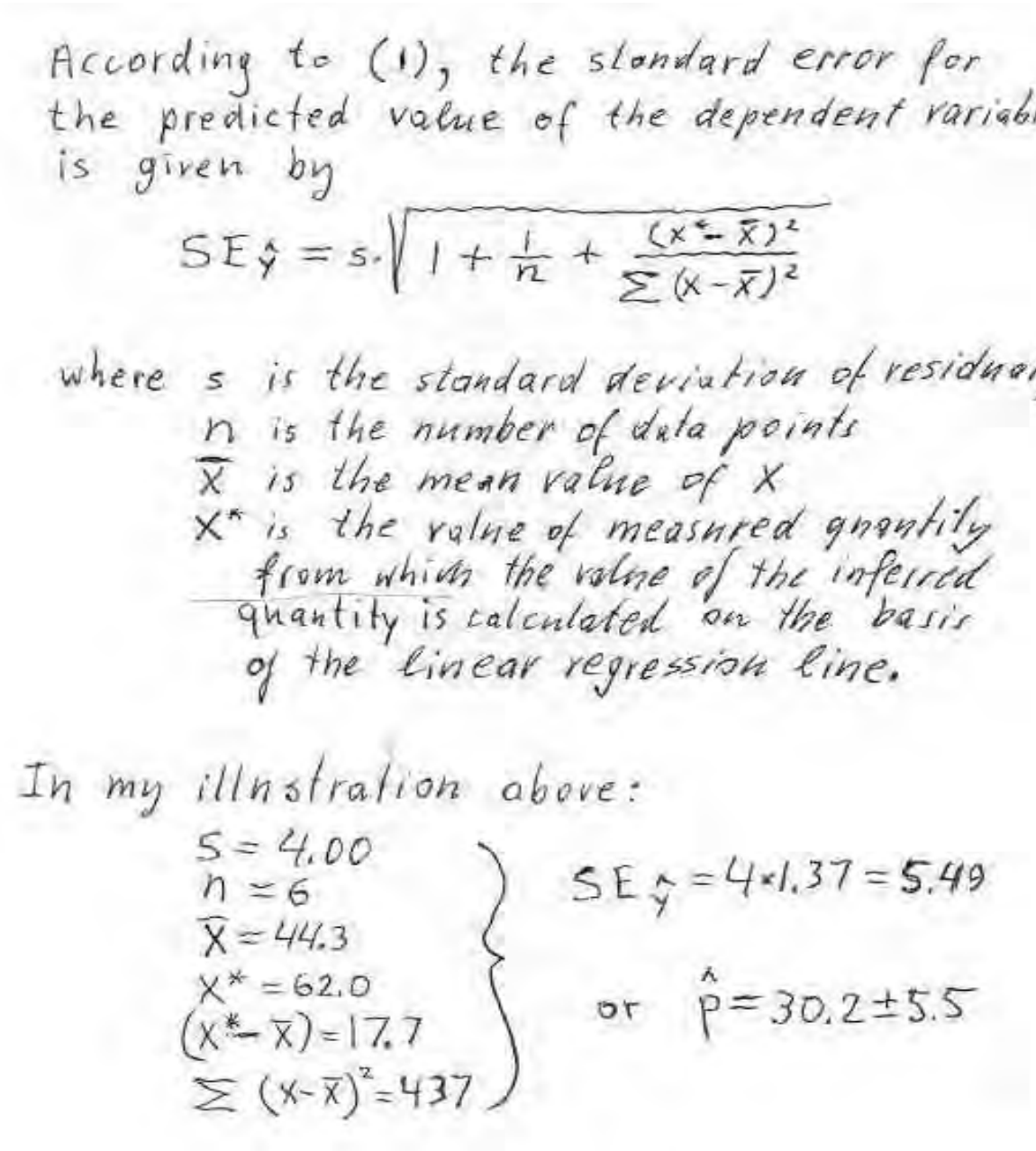

Figure 2
The term (x* - x_bar), under the square root, is the horizontal component of the distance of a particular value of x* from the central region. The uncertainty assigned to the inferred value is at a minimum when the measured value x* is closer to the center. Note that x*=62 is located near the right-hand margin of the scatter plot. For an x* close to 44.3, the third term under the square root would become negligible. Furthermore, the second term becomes negligible when n>20. Thus, for a large number of calibration points, the uncertainty in the inferred value approaches the standard deviation of residuals when x* approaches the mean x. This is intuitively acceptable; try to totate the regression line about the centroid of data points, (x_bar,y_bar). The range of calibration data points should not be limited to the range of expected values of x*, the wider the calibration range the smaller the uncertainty assigned to p, for any given n.

P.S.
This note was inspired by correspondence with Scott Little, a calorimetrist who uses linear regression calibrations. The correspondence, in turn, was triggered by Scott's paper (2), and by comments made by several CMNS researchers (3). Scott also critically examined my original draft and helped to improve it. But his method of calculating uncertainties is based on commercial software.

References:
1) David Moore, “The Basic Principles of Statistics,” 2nd edition, Freeman, 2000, page 544.

Keep in mind that I am not a statistician. I will be happy to add a clearly written appendix with results based on alternative formulas, and on commonly available software.

2) Scott R. Little “Null Tests of Breakthrough Energy Claims,” Presented at “42nd AIAA/ASME/SAE/ASEE Joint Propulsion Conference & Exhibit , 9 - 12 July 2006, Sacramento, California.”
Link to the paper http://www.earthtech.org/publications/index.html (click on item #6).

3) Comments on Scott's paper, made by several CMNS researchers, including Scott himself, were published by S. Krivit, in the 23th issue of New Enenrgy Times, July 10, 2007
Link to Krivit's publication: http://newenergytimes.com/news/2007/NET23.html

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332) Krivit's Sonofusion Report

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Introduction:
Nonscientific factors, such as political and economic considerations, negative emotions, such as jalousie and vindictively, or desire to dominate, often influence scientific debates. Opposition to Galileo was ideological rather than scientific. The same can be said about opposition to the theory of relativity (Germany in late 1930s), and about opposition to genetics (Soviet Union in late 1940s). These are examples of well known, and highly organized, campaigns. Less known are contemporary feuds in various disciplines and sub-disciplines. A friend, Jed Rothwell, who created a wonderful Internet-based library of CMNS papers, wrote to me: "There is a lot of jealousy, rivalry and irrationality in this [CMNS] field, which is one of the reasons it has made so little progress. It is unfortunate." I believe Jed; he was following evolution of the CMNS field for much longer than I did. Jed's e-mail message came several days before the 23rd issue of The New Energy Times was published:

http://newenergytimes.com/news/2007/NET23.htm

Item 8 of that issue contains a link to an interesting report of Steven W. Krivit. The report is devoted to an ongoing feud in another field. Krivit is a professional journalist specializing in scientific reporting, and the editor of The New Energy Times. In the Introduction he wrote: "Society's general understanding is that scientists are objective, dispassionate, dedicated to serving the greater good of society, and held to a high standard of ethics." But then he describes situations which are quite different from what is generally expected. The feud he describes involves a nuclear engineer Rusi Taleyarkhan, Purdue University (where he is a tenured professor), his scientific opponents and some journalists.

In an overall comment about this ongoing feud (item 2 in the above URL), Christopher Purvis wrote "Purdue failed in that duty of care by leaving Taleyarkhan to represent himself among some of the most aggressive and manipulative international journalists, where, through no fault of his own, he quickly got out of his depth and walked into the trap they had prepared for him with the hired help of rival researchers." I have seen references to that episode but I did not read publications of involved scientists. I plan to read them; this unit is a place holder for future entries about sonofusion. For the time being I just want to share one observation.

Sonoluminscence and cold fusion: a common denominator
Taleyarkhan's claim is about sonofusion -- hot fusion taking place inside collapsing bubbles. In that sense the topic is very different from cold fusion claims. What do these two claims have in common? Both Fleischmann and Taleyarkhan made the same deplorable mistake. That mistake is partially responsible for subsequent rejections of what has been published. It would be better if claims were formulated in terms of what was actually discovered and not in terms of speculative implications. Fleischmann discovered non-chemical excess heat. It was a mistake to speculate that a nuclear process is responsible for excess heat, it was a mistake to accept the term cold fusion. The consequences are well known; instead of focusing on replications people started to discuss speculations, such as tunneling effect and emission of neutrons. Fleischmann should have refused to discuss nuclear physics concepts until reality of excess heat were recognized by most scientists.

Likewise, Taleyarkhan's claim should have been limited to what he actually discovered, generation of neutrons and production of tritium. He should have refused to discuss sonofusion until reality of nuclear particles was accepted by

most scientists. If he did this then subsequent debate would focus on detection of nuclear particles and not on wheather or not they are emitted from collapsing bubbles. Emission of nuclear particles would be interesting even if they were not emitted from collapsing bubbles. Putterman's emphasis on short time coincidences (between light and neutrons) was perfectly logical in the context of sonofusion but it would not be prematurely debated outside of that conext. Likewise, emphasis on coulomb barrier was perfectly logical in the context of cold fusion but it would not be prematurely debated in the context of excess heat. Decoupling facts from speculations would be initially beneficial in each of these fields.

Yes, tendencies to interpret experimental facts in terms of accepted theories is natural and desirable. Science is much more than factology. But waiting for general acceptance of new experimental facts, and refusing to speculate about theories, would likely be more effective, in a long run. Speculations about models should be based on will established facts while predictions about facts should be based on well established theories. And what to do in situations in which nothing is well established? In such cases it is probably better to address unexplained facts before addressing possible explanations. The issue of Fleischmann's excess heat, and the issue of Taleyarkhan excess neutrons, would probably be resolved long time ago if debates about explanations were postponed.

Appended on 7/12/07:
The first step toward sonofusion was discovery of sonoluminescence -- production of heat and light in tiny bubbles. Generation of heat in a rapidly compressed gas has been studied for more that a century; it led to many practical applications, such as ignition in a Diesel engine. In the case of small bubbles the temperature increases are often so high that substances are ionized and light is emitted. This was discovered by two German scientists in 1930's. But can the temperature be high enough to allow thermonuclear reactions? This question was asked in 1982, by an American scientist Hugh Flynn. The answer, according to Rusi Taleyarkhan (R.T.), is positive, as demonstrated by him in 2002, and in more recent experiments. But his claims have been challenged by several scientists. R.T. came to US from India, obtained his Ph.D. degree in nuclear engineering, and worked at Oak Ridge National Laboratory for may years. Then he became a tenured professor at Pardue University. Here is how his first sonofusion report was described in CERN Courrier (Apr 23, 2002):

". . . This is the latest apparent manifestation of sonoluminescence, whereby light is emitted by bubbles collapsing in a liquid excited by sound. Observations of the light suggest that implosions provoked by high-frequency sound could create extremely high temperatures and pressures - high enough, perhaps, to lead to conditions that could fuse two atomic nuclei. The researchers used neutrons to induce the formation of bubbles in liquid acetone in which the hydrogen atoms had been replaced by deuterium. Sonoluminescence resulted in the observation of light accompanied by neutrons. Tiny quantities of tritium were also detected. The researchers hypothesized that nuclear fusion had occurred, since the neutrons had different energies from those used to induce bubble formation, and tritium is an expected byproduct. When the experiments were repeated with hydrogenous acetone no tritium or neutrons were observed."

It is important to emphasize the basic difference between sonofusion and the so-called "cold fusion." The former is not different from the well known thermonuclear reactions, as in hydrogen bombs, except for the method by which bursts of high temperatures are generated in tiny bubbles. The later, on the other hand, are said to be due to unknown nuclear processes. The controversy about sonofusion has to do with generation of stellar-like temperature in collapsing bubbles; the controversy about cold fusion has to do uncharted areas of basic physics. The 2002 claim, for reality of sonofusion was based on production of tritium and on emission of neutrons, above the expected background level. These are two basic signatures of known thermonuclear reactions. But, as recognized by the first opponents of the sonofusion claim, D. Shapira and D. Saltmarsh, the neutron to tritium ratio in sonofusion was very different from what it is in known thermonuclear reactions. If sonofusion were real, they wrote, then it would be a totally new nuclear process. But these scientists from Oak Ridge National Laboratory did not believe that sonofusion was real. They, and other opponents, suspect that neither neutrons nor tritons, observed by R.T., were produced in compressed bubbles. An auxiliary neutron source, and tritium contamination, were said to be responsible for the reported results.

This morning I wrote the first two papers of R.T. and his coworkers:

(1) " Evidence for Nuclear Emissions during Acoustic Cavitation," Science , Vol. 295 , No. 5561, pp. 1868-1873 ,

March 8 , 2002.
(2) “Additional Evidence for Nuclear Emissions during Acoustic Cavitation”, Phys.Rev.E , Vol. 69 ,No. 036109 , March , 2004.

They are full of interesting details. Unfortunately, I am not sufficiently qualified to comment on everything. But I do understand some things. How was generation of tritium established? By counting electric pulses due to decay of tritium in a pre-calibrated liquid scintillation counter. One cubic centimeter of deuterated acetone could be removed from their cell and added to the liquid scintillator, ecolyte. Here are their results:

a) before sonofusion experiment (this is the background) -- about 53 counts/min.
b) after 7 hours of sonofusion --about 60 counts/min.
c)after 12 hours of sonofusion --about 69 counts/min.

Standard deviation associated with expected random errors were close to 2.5 counts/min. Yes, the amount of tritium does seem to increase with duration of an experiment but the rate at which accumulation of tritium increases is not impressively high. What is impressive, however, is the fact that no accumulation of tritium was observed when deuterated acetone was replaced by common acetone, or when deuterated acetone was exposed to seeding neutrons without acoustical waves. Numbers alone would be much more impressive if the rate of tritium production were several times larger.

According to R.T., sonofusion is greatly enhanced when acetone is constantly ionized by an external source of 14 MeV neutrons. These neutrons are delivered in pulses of short duration (about 10 microsecond). Each pulse arrives at a moment at which the applied acoustical wave is at the minimum. The bubbles collapse about 27 microseconds and that is when sonofusion takes place. Neutrons of 2.45 MeV are expected from the D-D fusion and that what said to be discovered by R.T. It is important not to confuse neutrons from sonofusion with neutrons from the 14 MeV source (a linear accelerator). The 27 microsecond delay seems to be sufficient for this. But this was not taken for granted by some critics. Statistical evidence for emission of neutrons is more convincing than evidence for emission of tritons, as illustrated in Figures 4a and 4b (see the second paper above). It is about 300 counts, when the acoustical wave is on, versus about 5 counts when the acoustical wave is off. In both cases the cell was exposed to the pulsating beam of 14 MeV neutrons. The hot fusion neutrons were detected with a scintillator counter that was blocking gamma rays. This was possible because shapes of electric pulses due to gamma rays were different from shapes due to neutrons. Actually, separation of gamma rays from neutron was not necessary to demonstrate presence of a nuclear effect. In a more recent experiment of R.T., described in

(3) “Nuclear emissions during self-nucleated acoustic cavitation,” Phys. Rev. Letters, Vol. 96, 034301, Jan. 27, 2006,

two additional kinds of detectors (BF3 and CR-39) were used to confirm presence of neutrons. But the most important innovation of the 2006 experiment was elimination of 14 MeV neutrons. This was done to show that 14 MeV neutrons could not possibly be responsible for hot fusion neutrons. Ionization of acetone, in the new experiment, was performed by alpha particles. To accomplish this a small amount of natural uranium salt was dissolved in acetone.

Inserted 7/14/07):
I also read two critical comments of this new experiment, one by B. Narango (a research associate of S. Putterman) and by a Russian Scientist, A. Lipson. Both comments, and rebuttals by R.T. can be found in

(4) “Comment on ‘Nuclear Emissions During Self-Nucleated Acoustic Cavitation,’“ Physical Review Letters, Vol. 97, p. 149403, (Oct. 6, 2006)

Narango analyzed the energy spectrum of neutrons, under assumed experimental configuration, and arrived to a conclusion that it was practically the same as that of Cf-252. Was he thinking about a possibility that a Cf-252 was present somewhere in the laboratory? If so then experiments with ordinary acetone would show about as many neutrons as experiments with deuterated acetone. But 2.45 MeV neutrons, according to R.T., were present only when deuterated acetone was used. Responding to Narango, R.T. showed that energy spectra of detected neutrons from his Cf-252 source and from his sonofusion cell were not identical. Even more convincing argument, against presence of a

Cf-252 source, was the comparison of energy spectra of gamma rays.

In Chapter 5 of his report, S. Krivit, tells us how the Cf-252 issue emerged, about half a year before Norango‘s paper was published, and how it was interpreted. It is important to be aware that neutron spectroscopy is a complicated business. Suppose that mono-energetic neutrons of 3 MeV are emitted. A plastic scintillation detector, exposed to these neutrons, will show energies of protons that neutrons create in plastic. These protons will have all energies from zero to 3 MeV. The shape of the recorded energy spectrum will depend on the size of the detector, on its shape, on its relative location with respect to the source, etc. etc. That is why neither the spectrum due to Cf-252 neutrons, nor the spectrum of neutrons attributed to sonofusion resemble the spectra of original spectra of neutrons from these sources.

Comments made by A. Lipson were about possible mistakes made in estimation of neutron detection efficiencies. Such mistakes, if real, would lower the neutron emission rate significantly. Was the rebuttal provided by R.T. (about e.m. noise) convincing? I think it was. But I am not sure.

Most people, including R.T.., seem to believe that sonofusion in deuterated acetone is the well known thermonuclear D-D reaction. But the R.T..’s data are not consistent with that interpretation. As noticed by D. Shapira and M. Saltmarsh, in

“Nuclear fusion in collapsing bubbles -- Is it there? . . . ” Phys. Rev. Lett., vol 89, p 104302 (2002),

the reported neutrons to tritons ratio in sonofusion and in thermonuclear reactions differ by four orders of magnitude. In other words, R.T.. observed about 10,000 less neutrons than would be expected for the reported amount of tritium. I do not remember this issue being addressed in subsequent discussion. That would not be important if the claim was limited to unexplained emission of neutrons and tritons. But the issue becomes essential when these particles are said to be produced in thermonuclear D-D reactions. In my opinion, arguments for reality of a nuclear activity are more convincing than arguments for the sonofusion nature of that activity. Let me add that according to Y.T. Didenko and K.S. Suslick, in

“Energy efficiency of formation of photons, radicals and ions during single-bubble cavitation,” Nature, vol 418, p. 394, July, 2002

Collapsing bubbles containing polyatomic molecules cannot possibly be heated to stellar temperatures. They wrote: “The temperatures reached during cavitation will be substantially limited by the endothermic chemical reactions of the polyatomic molecules inside the collapsing bubble. We therefore expect that the extraordinary conditions necessary to initiate nuclear fusion will be exceedingly difficult to obtain in any liquid with a significant vapor pressure. However, the possibility of such events in very low volatility liquids (for example, some polar organic liquids, molten salts or liquid metals) cannot be ruled out. The present results show that endothermic sonochemical reactions within a collapsing bubble are a major limitation on the conditions produced during cavitation.” This argument would also be totally irrelevant if the claim made by R.T. was limited to emission of nuclear particles.

The same observation can be made about very fast coincidences, between emission of light and emission of nuclear particles, requested by S. Putterman (see page 36 of Krivit’s report). Confirming such coincidences would indeed be a very convincing argument for sonofusion. Experiment of that nature, however, is not simple, and probably very difficult to interpret, especially if no dominant coincidence peak is found in the time spectrum. But such experiment is totally unnecessary in the context of a claim that does not include sonofusion. It would be better if attempts to confirm a nuclear activity were decoupled from attempts to confirm specific mechanisms, such as sonofusion or cold fusion.

Another overview of the sonofusion situation can be found in:
http://en.wikipedia.org/wiki/Bubble_fusion

This website contains other cold fusion items.
Click to see the list of links

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This website contains other cold fusion items.
Click to see the list of links

333) Richard's paper has been submitted to Physical Review C

Ludwik Kowalski (11/6/07)

When I was working on unit #335 (a paper describing my clusters) Richard Oriani submitted a paper to Physical Review C. This unit is a replacement of the draft of Oriani's paper that was posted here for several months. Unfortunately, the manuscript was rejected out of hand by the Editor without its being submitted to reviewers.

Note that figures and tables are below the main text.

Repeatable technique for the generation of a nuclear reaction during electrolysis

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Abstract
A relatively simple technique using CR39 particle detectors has been developed that in 25 consecutive electrolyses has reproducibly produced charged particle tracks, showing unambiguous evidence of a nuclear reaction during electrolysis of heavy or light water solutions. Nuclear tracks can be produced upon the surface and beyond the 1 mm thickness of the CR39 detectors. Nuclear activity of some sort can persist in the Viton o-rings used in the electrolysis cell so that charged energetic particles can be generated subsequently without electrolysis.

I. INTRODUCTION

Since the ill-fated announcement by Fleischmann and Pons [1] that a nuclear reaction can take place during electrolysis of a solution of lithium sulfate in heavy water much work has been done in attempts to confirm the claim. Papers have been published that show the production of tritium, of ^{4}He, of the production of thermal energy far in excess of the inputted electrical energy, and the transmutation of some elements; see [2] for a review. These efforts have been met by assertions from the nuclear community of experimental error or naiveté, or accusations of fraud and deception. There are two main reasons for this repudiation: the claimed phenomena can not be understood in terms of currently accepted nuclear theory, and the lack of reproducibility of the phenomena. Any one experimenter cannot produce the claimed phenomenon on demand, and another experimenter often can not replicate the results.

Recognition of the latter difficulty has motivated the search for an experimental technique that is as transparent, simple, and irrefutable as possible to try to prove that electrolysis can be accompanied by a nuclear reaction, despite current understanding in nuclear physics. Energetic charged particles and very energetic neutrons can be detected and recorded by a high polymeric material called CR39, and its use is well known to the nuclear community [3]. A nuclear particle upon entering the plastic leaves a trail of disrupted chemical bonds which are more easily chemically attacked

than undamaged material. The pit that results from etching by concentrated alkali solution is unambiguous evidence that a nuclear reaction had occurred, since only particles of energies of 0.5 MeV or more can damage the bonds sufficiently to leave an etchable trail. Such energies are much larger than can be provided by chemical reactions.

The integrating character and the relative simplicity of this technique have led the writer to apply it to the problem. Control, or blank, experiments are necessary because radon in the air and the occasional cosmic ray produce etchable damage trails in CR39 detectors. Past work with this method in this laboratory has included immersion of CR39 chips in electrolyte solution, suspension of the chips in the vapor above the solution, placement of chips in air just below the metal plate that serves both as cathode and as the bottom of the electrolyte compartment, and placement of chips in air just outside of the glass cell, with each of these configurations maintained during electrolysis. From the numbers of nuclear tracks on the detector chips used in the electrolyses and those on chips used as controls can be calculated the probability, P, that the two sets of numbers belong to the same population, ie., that electrolysis has no effect on the number of pits. The results of prior research [4,5] are as follows. For detector chips immersed in H_2O/Li_2SO_4 electrolyte and Pd as cathode $P = 1.2*10^{-6}$, and with Ni as cathode $P = 5.8*10^{-4}$. With D_2O/Li_2SO_4 and Pd as cathode $P = 2.5*10^{-5}$. With detector chips suspended in the vapor over H_2O/Li_2SO_4 electrolyte and Ni or Pd as cathode material $P = 3.0*10^{-10}$. Although these results reasonably demonstrate that electrolysis can indeed generate energetic nuclear particles, it cannot be claimed that every electrolysis experiment will produce the nuclear reaction since the data sets for the electrolysis chips and the control chips partially overlap. Consistent reproducibility has not been achieved despite having kept constant all the controllable parameters. Therefore a different experimental approach has been adopted.

II. EXPERIMENTAL DETAILS

Investigators [6] at the SPAWAR Systems Center, San Diego, California, initiated the strategy of having the cathode and the CR39 chip in very close proximity. The metal wire serving as cathode was wound tightly around the detector chip. Unfortunately, this configuration produced copious chemical attacks on the detector by ions generated at the cathode. The huge number of chemical pits produced made it very difficult to ascertain the generation of nuclear pits. To maintain the desirable nearness of cathode to detector while avoiding the chemical attack, the obvious modification is the interposition of a thin Mylar film between the electrolyte and the detector chip. Preliminary experiments showed that 6 _ Mylar film permits the passage of nuclear particles emitted by pitchblende, energies of 4.1 to 5.8 MeV.

The cell design adopted is shown in Fig. 1. Squares, roughly 3cm x 3cm, of CR39 cut from a sheet supplied by the Landauer Corp. are overlaid by 6 microns Mylar foils obtained from Cemplex Industries, Inc., and the combination is pressed between Viton o-rings that fit into grooves in the glass o-ring joints purchased from Fisher Scientific Corp. The joint is held together by a pinch clamp. The anode is a platinum wire whose lower end forms a loop parallel to the plane of the detector chip. The cathode is usually a nickel wire whose lower end is bent into a shape like a W the plane of which is parallel to the plane of the detector square. The vertical portion of the nickel wire is sheathed in heat-shrinkable plastic tubing and spot-welded to a titanium rod inserted into a glass tube. The electrode assembly is held together by a rubber stopper through which a hole permits the escape of the gases produced by the electrolysis. This describes the electrolysis cell designated by S. Another cell designated by B differs from the S-cell only in that the platinum anode wire ends in a crude spiral the plane of which is perpendicular to the detector chip.

The current to cell-S is supplied and controlled by a potentiostat, and that to cell-B by a constant-current power supply. To begin an experiment a square of CR39 is cut from the sheet from the manufacturer, a small hole is drilled at a corner of the detector square to accommodate a wire for the subsequent suspension of the chip, and an identification symbol is inscribed at another corner. The surface on which the identifier is scratched is referred to as the front surface, and is the surface upon which the Mylar film is laid after the manufacturer-supplied blue film that protects the detector is removed. After the detector with its overlying Mylar film is clamped in the o-ring joint about 10 ml of electrolyte solution are poured into the cell and the electrode assembly is put into place. The cathode is then lowered carefully by sliding the supporting titanium rod within the supporting glass tube until the W-shaped foot of the cathode rests upon the Mylar film. Electrolysis is then started and the current is either kept constant for the entire duration of the electrolysis, or is increased once per day.

The electrolyte employed was usually of distilled H_2O with 0.022 g Li_2SO_4 per ml. To provide a comparison with the results of ref. 6 the first two experiments were done with silver cathodes in electrolytes of D_2O with LiCl plus $PdCl_2$. Water was not added during electrolysis to compensate for its loss by dissociation. Four days of electrolysis were the usual duration after which the cell was disassembled and the detector chip removed and washed. There was never any trace of pitting or abrasion on the detector plastic before etching. Stirred 6.5 M sodium hydroxide was the etching solution, held at about 70 C for about six hours. This was followed by examination at 100X and 500X, applying experience with pits produced by exposure of CR39 to ^{241}Am and to pitchblende to distinguish nuclear pits from artifacts caused by manufacturing defects in the detector plastic. The nuclear pits were counted on the front and rear surfaces of the detector chips within the outer perimeter of the o-rings, amounting to 4.5 cm^2. This procedure eliminated the counting of tracks that may have been produced by air-borne radon during the duration of electrolysis.

Control experiments of four kinds were carried out to account for the nuclear tracks already present in the detector as received from the supplier, as well as those produced during the entire experimental process caused by radon in the air, in the electrolyte, in the etching solution, and in the wash water. The control detector chips were handled in exactly the same way as were the experimental chips except that instead of being used in electrolysis they were either wrapped in Mylar film, immersed in stock electrolyte solution, pressed against as-received o-rings, or mounted in a newly constructed cell fitted with unused electrodes, electrolyte, Mylar film and stoppers, but in the absence of electrolysis. In each instance the same length of time as the duration of the electrolysis experiment was applied. The number density of tracks in the as-received detector sheet varies from one shipment to another, and is not a constant over any one sheet. Therefore it is appropriate to compare an experimental value of track number density to the mean value for the controls. The results for the controls are presented in Table I.

Consideration has been given to the possibility that the features seen after etching might have been caused by processes other than the alleged nuclear reaction. For example, radioactive dust particles floating in the laboratory air may adventitiously settle upon a detector chip and produce nuclear tracks. This possible problem has been examined by placing fine particles of pitchblende upon detector chips. Examination after etching has shown that such particles produce “rosettes” of tracks (Fig.2). These track configurations, reputedly also produced by cosmic rays, have very occasionally appeared on detector chips during our research. They were not included in the counting of nuclear tracks. One may suspect that electrostatic charges produced by peeling off the manufacturer-supplied blue protective plastic film from the CR39 might cause etch pits. This possibility was explored by adhering Scotch tape to a detector chip then peeling it off. After etching nothing was visible that could be attributed to static charges. However, it is recognized that if, after peeling off an adhering film the bare chip is allowed to remain exposed to dry air for many hours, the electrostatic charges on the chip will attract daughter products of air-borne radon so that nuclear tracks would be generated. This was prevented in our work because always immediately after peeling off the protective film the detector chip was mounted in the electrolysis cell.

Manufacturing defects can make it difficult to discriminate between nuclear pits and artifacts. The polymerization process can leave poorly polymerized regions. As etching proceeds the receding surface of the detector chip intersects with such regions which etch more rapidly than well polymerized material so that pits result. These pits are usually small, circular, shallow, and most of them appear in groups. They can be distinguished from nuclear pits by the much darker appearance of the latter in the microscope using illumination from above. Features whose identity remains ambiguous are not counted as nuclear pits. Scratches and other mechanical insults to the detector chips can also produce ambiguities. This problem was examined by a series of experiments in which various mechanical forces were applied to chips, followed by etching and examination. Light scratching produces linear arrays of pits usually of uniform diameter. For this reason pits in linear arrays are never counted as nuclear pits. Pits are not produced by hard pushes against the chip with the point of metal tweezers or with the handle of an Exacta knife. On the other hand a push with the point of an Exacta blade produces a very dark pit. Rubbing with a plastic rod has no effect, but rubbing with a metal spatula leaves a faint trace, and rubbing with the point of tweezers produces an etchable trail. But grasping a detector chip with metal tweezers using considerable force does not leave any effects. This is fortunate because the chips are manipulated with tweezers.

One other type of experiment was in some instances carried out along with the primary experiments. Prompted by the results of previous research [7] that the apparatus employed for electrolysis appears to retain some kind of nuclear

activity, it was decided to investigate whether the o-rings demonstrate this phenomenon. After the termination of an electrolysis the o-rings were removed and after drying were placed upon a CR39 square, and the whole was wrapped tightly in aluminum foil and stored for four days. Then the detector was etched and the nuclear tracks counted.

III. RESULTS AND DISCUSSION

Table II displays the results of consecutive electrolysis experiments the primary purpose of which was to validate the technique. The counts of nuclear tracks, either on the front or on the rear surfaces, (Table II) are always considerably larger than the counts per unit area for the controls (Table I). In every experiment the average number density of nuclear tracks found after etching is greater than the mean value for the controls. Hence it can be concluded that the present technique has consistently produced evidence that a nuclear reaction of some sort has been generated in the course of electrolysis. The present research does not identify the nature of the nuclear particles that produce the observed tracks, but Lipson et al, [8] claim that the particles are protons and alphas. Also, the present work does not attempt to establish the connection between the nuclear reaction here manifested and the excess thermal energy reliably measured calorimetrically by several investigators [2].

A second observation from the results listed in Table II is that tracks are found on both sides of the detector squares, and that often the average track number density on the surface facing away from the electrolyte is larger than at the surface immediately below the Mylar film. Charged particles that caused the observed tracks on the rear surfaces could not have been generated at the site of the electrolysis. The 1 mm thickness of the detector square is much too large to be traversed by charged nuclear particles with the energies established by Lipson et al. This leaves either neutrons of very high energies as the damage trail maker, or some other neutral particle that subsequently reacts or decays to produce alphas and protons.

It is significant that the nuclear pits are distributed bimodally, both as individual randomly located pits and as dense groups or clusters that are bounded by areas relatively devoid of nuclear pits. The number densities in clusters are much greater than the mean number densities listed in Table II. The clusters are of two sorts: one within which the pits are essentially randomly distributed as to position and shape (Fig.3), and one in which the pits of elliptical or conical shape are radially distributed (Fig.4). Pits of these shapes result when the path of an impinging nuclear particle is other than perpendicular to the surface of the detector. By focusing the microscope up and down one can determine the direction and sense of the path [3]. Doing this for all of the elliptical and conical pits one can determine that the nuclear particles that produced those pits emanated from a common origin away from the surface of the detector. In fact, by a careful analysis [9] one can estimate the distance above the detector surface where the shower of nuclear particles originated. This consideration leads to the surmise that zero-charge precursors, that themselves originated in the region of electrolysis, reacted with something in the air near the detector or in the Mylar film to generate charged nuclear particles that produced the nuclear tracks.

Early in the present work it became clear that the number density of nuclear tracks found along the circle of the o-ring was often considerably larger than elsewhere on the detector chip. Past work [7] had developed weak evidence that nuclear activity can persist after the termination of electrolysis. These two items combined to motivate exploration of what happens when an o-ring that has been used in an electrolysis experiment is removed from the electrolysis cell and is allowed to lie upon a new detector square for several days. Table III presents the results of these experiments. Whereas fresh o-rings that have never been used in an electrolysis experiment produce an average of 16.6 tracks/cm^2, once-used o-rings often produce a much larger number of tracks. In some cases the number of tracks is so great that counting becomes impractical, and the area of largest number density coincides roughly with the circular outline of the o-ring. Figure 5 is an unmagnified image of an etched detector square after a post-electrolysis o-ring had lain upon it for three days. Figure 6 is a 100X photomicrograph of a small portion of the circular hazy perimeter shown in Fig. 5. It should be noted that before etching no trace of pits can be seen. Similar circular haze rings, composed of a multitude of nuclear tracks, have been also found on detector squares used in electrolysis experiments.

The tracks produced by a post-electrolysis o-ring placed upon a CR39 chip cannot have been caused by an ordinary radioactive contaminant in or on the o-ring. First of all, new unused o-rings produce only small numbers of tracks (Table I). However, there is the possibility that the o-rings become radioactively contaminated during the preparation or the disassembly of the electrolysis experiment perhaps by contact with laboratory furniture. This idea was explored

by two sets of experiments. In the first set two new, as-received o-rings were suspended in the laboratory air for two days and then rubbed on the top surface of the laboratory bench. After this the o-rings were placed on detector chips for two days. This procedure resulted in 42.3 and 11.3 tracks/cm^2. The first value is for the o-ring suspended in air near an operating electrolysis cell. In the second set of experiments one side of each of two new, as-received o-rings was rubbed against the top of the laboratory bench on which many electrolyses had been carried out. The other sides of the two o-rings were not contacted to the laboratory bench. Then, each o-ring was clamped between two detector chips for two days. After etching the tracks/cm^2 found on the chip surfaces that had been in contact with the rubbed sides of the o-rings were 6.8 and 7.1. The two chip surfaces that had been in contact with the unrubbed sides of the two o-rings developed 5.0 and 13.6 tracks/cm^2. These results show that the track densities listed in Table III and displayed in Fig.6 can not be artifacts of contact of the o-rings with radioactive contaminants in the laboratory. Another unlikely source of radioactive contamination was explored. This is the high-vacuum grease applied to the grooves of the glass joints to facilitate assembly with the o-rings. Crosses were drawn on one side of each of two detector chips with the grease and one day later the chips were etched and examined. The track densities found on the greased sides were 11.7 and 13.4 tracks/cm^2, whereas on the clean sides the track densities were 11.1 and 9.3. The high-vacuum grease is not a source of radioactive contamination.

Although the number density of tracks along the perimeter of a post-electrolysis o-ring is larger than that within the area bounded by the circle of the o-ring, the number density within the circle is often larger than the track density of the controls. Figure 7 shows the etch pits near the center of the area of the detector chip bounded by the post-electrolysis o-ring that had been laid upon it (experiment number 1 in Table III). The detector area represented by the photomicrograph is about 1 cm away from the o-ring and measures approximately 0.008 cm^2, so that the number density of tracks is in the thousands. Furthermore, the largest angle at which a charged nuclear particle would produce a damage trail in the area of Fig.7 would be made by a particle leaving the o-ring from a point about 2 mm from the detector surface (see Fig.8). The tangent of this maximum angle would be about 0.2. However, this angle, about 11 degrees, is smaller than the critical angle [3] for a damage trail to be made visible by etching (about 36 degrees) so that particles coming directly from any part of the o-ring could not cause etch pits at 1 cm separation from the circle of the o-ring. This argument has been confirmed by the procedure of depositing a light coating of uranium acetate upon an as-received o-ring, keeping the coated o-ring between two CR39 squares for four days, then etching the detector chips. Examination showed that haze patterns following the circumference of the o-rings had developed, similar to that illustrated by Fig.5. However, the central regions of the areas bounded by the haze rings were clear. The track densities were 55 tracks/cm^2 on one chip and 70 on the other. These numbers are to be compared with the background value of 55 per cm2 and with over 2000 tracks /cm^2 found in the center of the chip pictured in Fig.5.

That a high density of etch pits can be produced near the center of the overlying o-ring is evidence that the nuclear tracks generated in a post-electrolysis experiment are not the result of contamination on the o-ring. The residual effect is a real phenomenon. One must conclude that something residing in the o-rings previously used in electrolysis caused the generation of energetic charged particles near the center of the area, and that these charged particles then impinged upon the detector chip at angles larger (ie.,closer to the perpendicular) than the critical angle for etching pits.

IV. CONCLUSIONS

Several definite conclusions can be drawn. First, a nuclear reaction can occur during electrolysis. A technique has been developed that consistently produces evidence of a nuclear phenomenon in the form of nuclear tracks made visible by etching. Second, nuclear tracks can be produced beyond the 1 mm thickness of the plastic detector. The inference may be made that at the site of electrolysis uncharged particles are created that are the precursors of the energetic charged particles whose damage trails can be observed after etching the CR39 detectors. It is further inferred that the precursor entity decays or reacts with an element in the air or with an element within the plastic of the Mylar film to generate charged particles. Third, the neutral precursor particle can persist for an as yet undetermined time in the materials that constitute the electrolysis cell and can react to produce charged particles in the absence of electrolysis.

Many questions are left unanswered. Chief among these is the nature of the precursor particle. Another important question is the relation between the nuclear reaction made manifest by the observed nuclear pits in the CR39 and the excess thermal energy that has been measured calorimetrically in other research. It is clear that the thermal energy

deposited by the formation of the charged particles observed in this research is much too small to account for the calorimetrically measured thermal energy. Much research remains to be done. The aims of the present work have been only to develop an experimental technique that reproducibly shows that a nuclear reaction can accompany electrolysis, to demonstrate some of the characteristics of the nuclear reaction, and to provide convincing evidence that justifies initiating extensive research in this new segment of nuclear physics not only because of its intrinsic scientific interest but also because of the possible development of a useful source of energy.

ACKNOWLEDGMENTS
Suggestions from and discussions with J.C. Fisher are gratefully acknowledged.

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FIGURES

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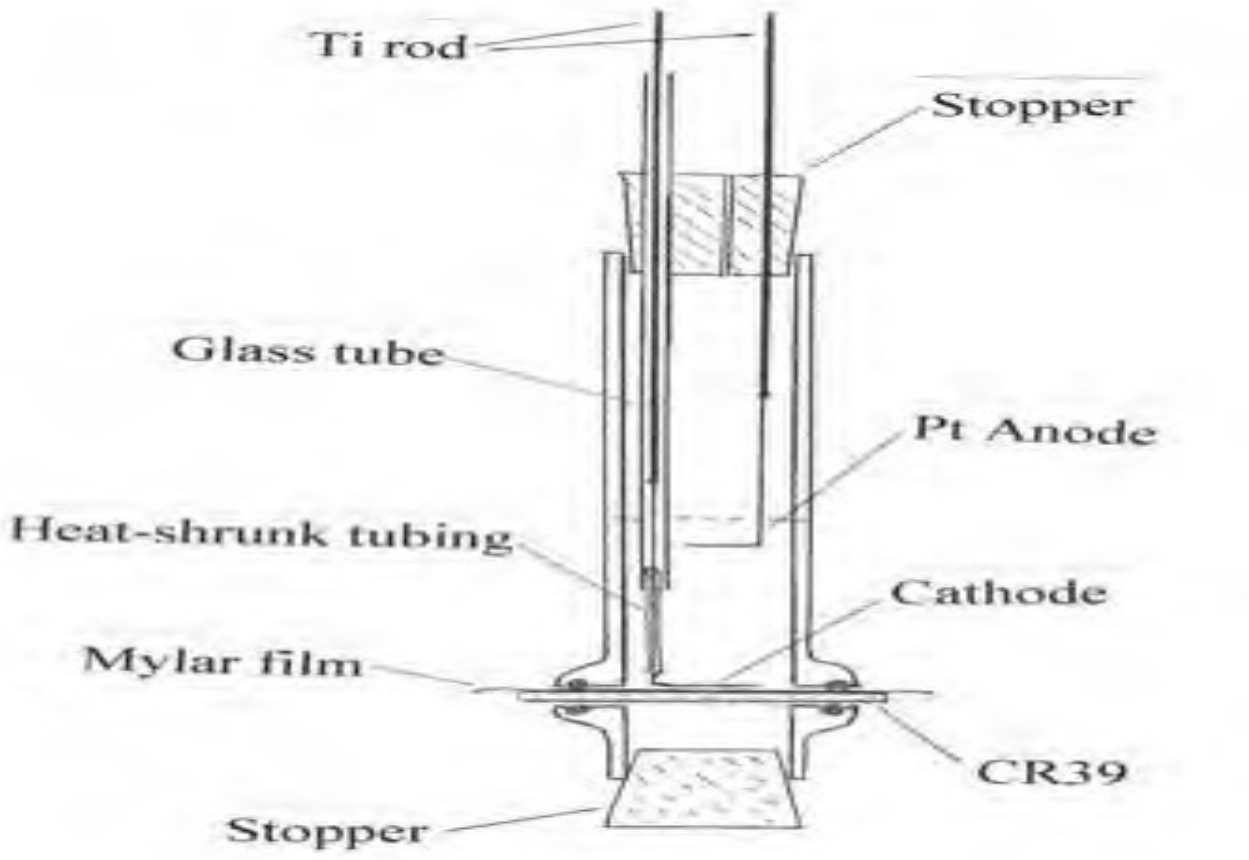

FIG. 1. The glass cell employed for electrolysis. The cathode assembly can be slid up and down gently to contact the Mylar film lying upon the detector chip.

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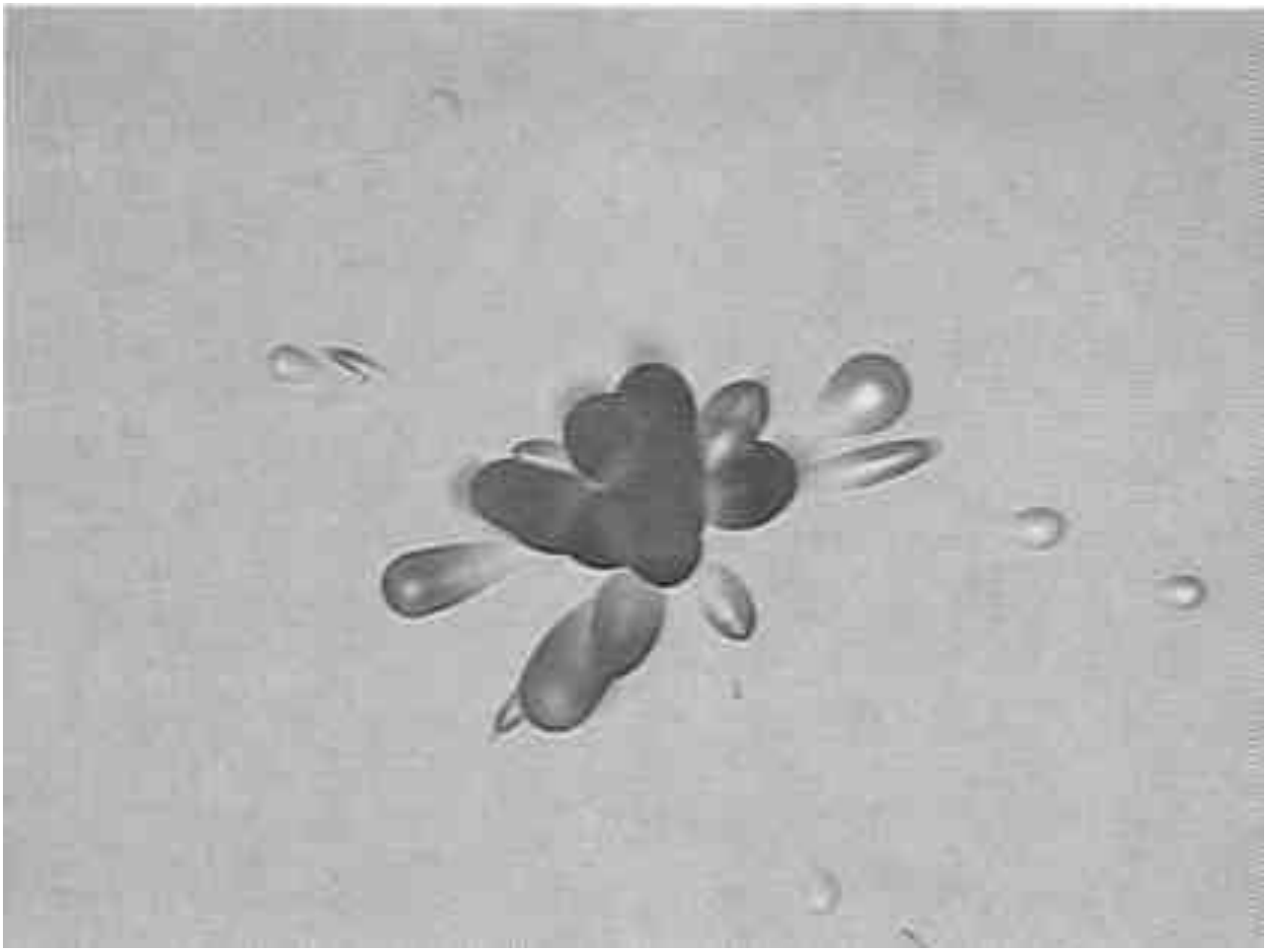

FIG. 2. The nuclear track configuration here called a rosette (original magnification 500X). The major axes of the elliptical and conical pits point to a common origin, and their sense shows that all of the tracks emanate from a common point. A rosette can result from a very small particle of a radioactive substance deposited upon the detector surface.

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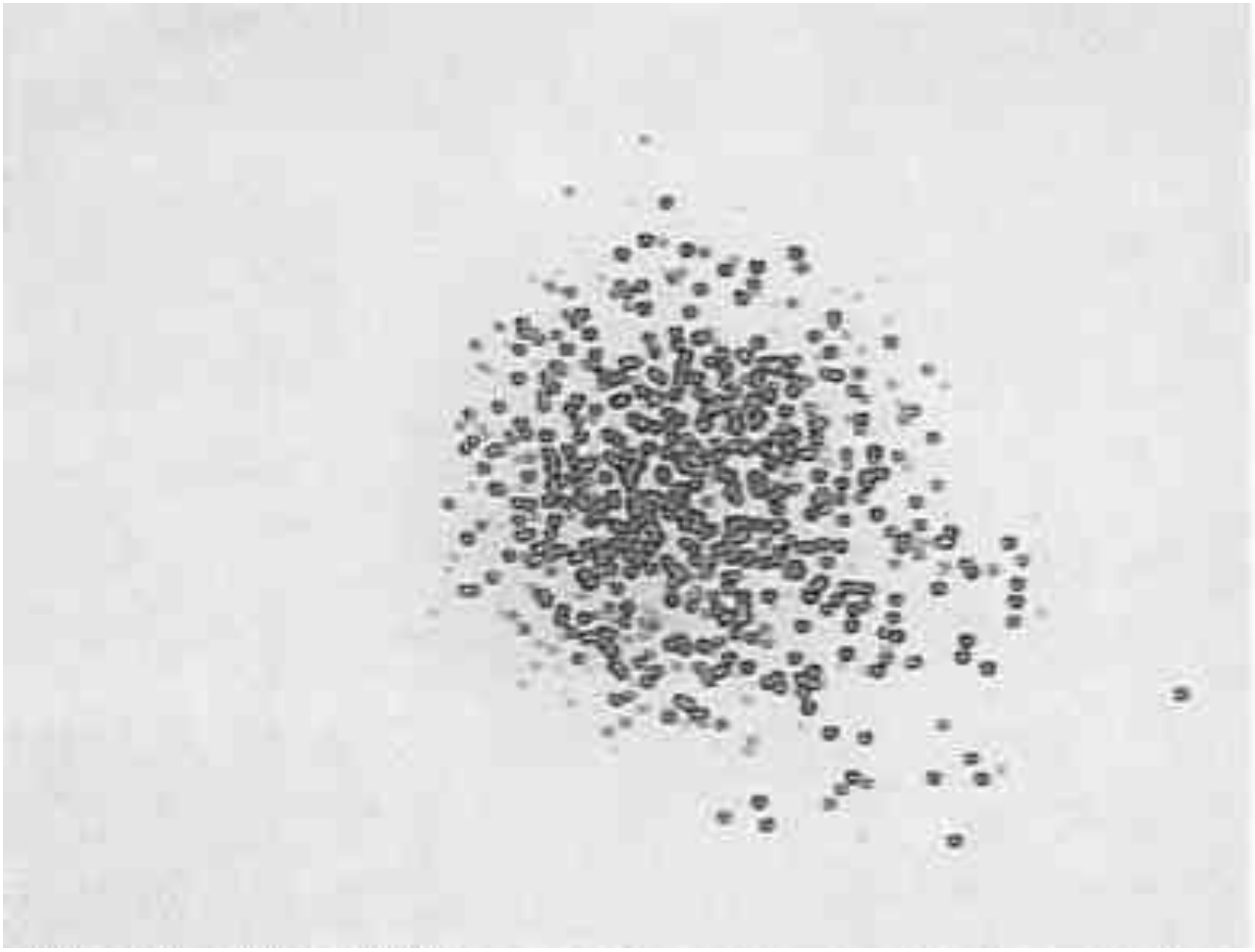

FIG. 3. A typical group of nuclear pits here called a cluster (original magnification 100X). It is a high-density grouping of nuclear tracks without any orientational relation among them, occupying an area of the detector chip otherwise fairly devoid of tracks.

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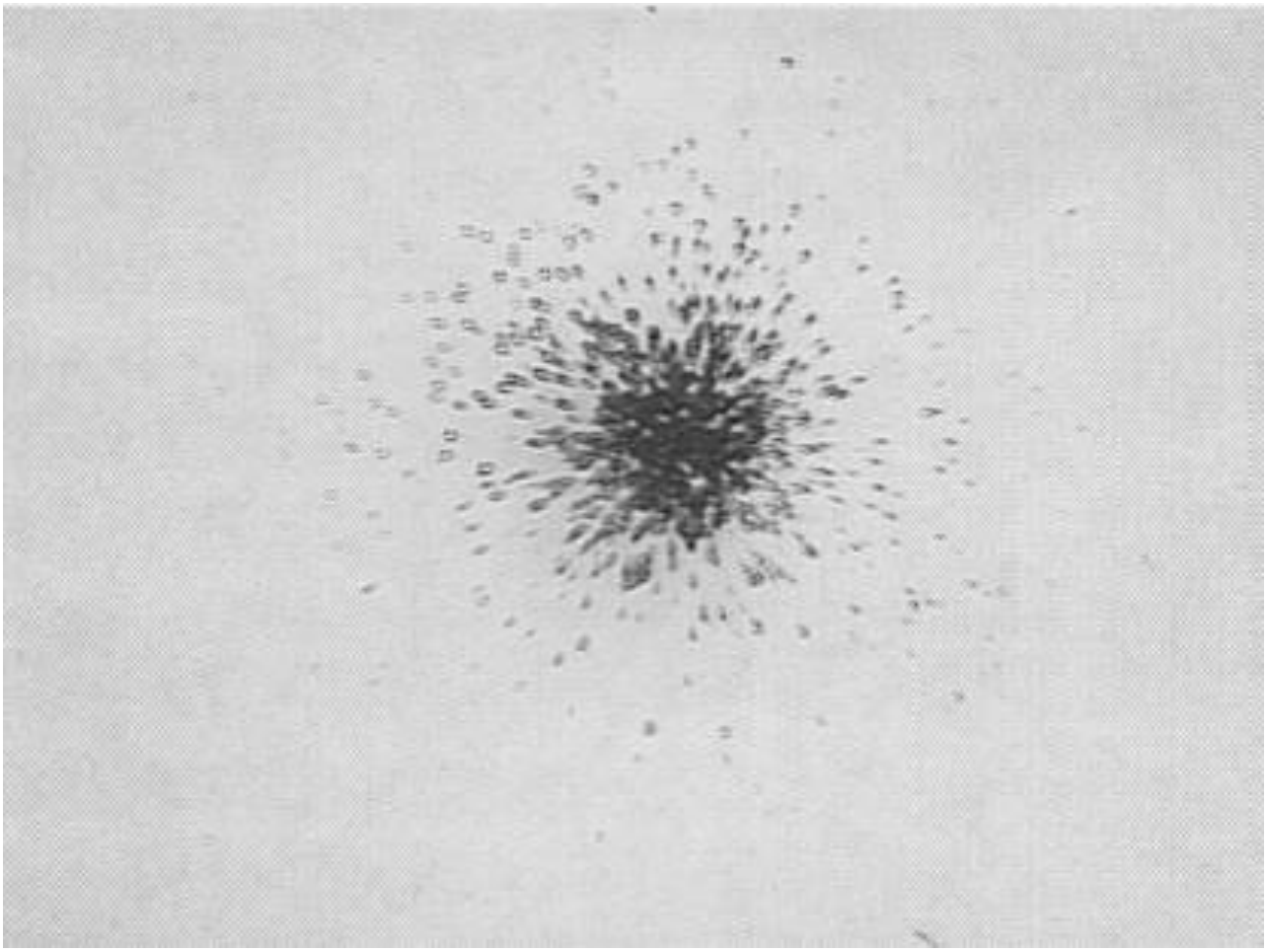

FIG. 4. A radiating cluster (original magnification 100X) typically containing pits of various shapes with the major axes of the non-circular pits radiating out from a common center.

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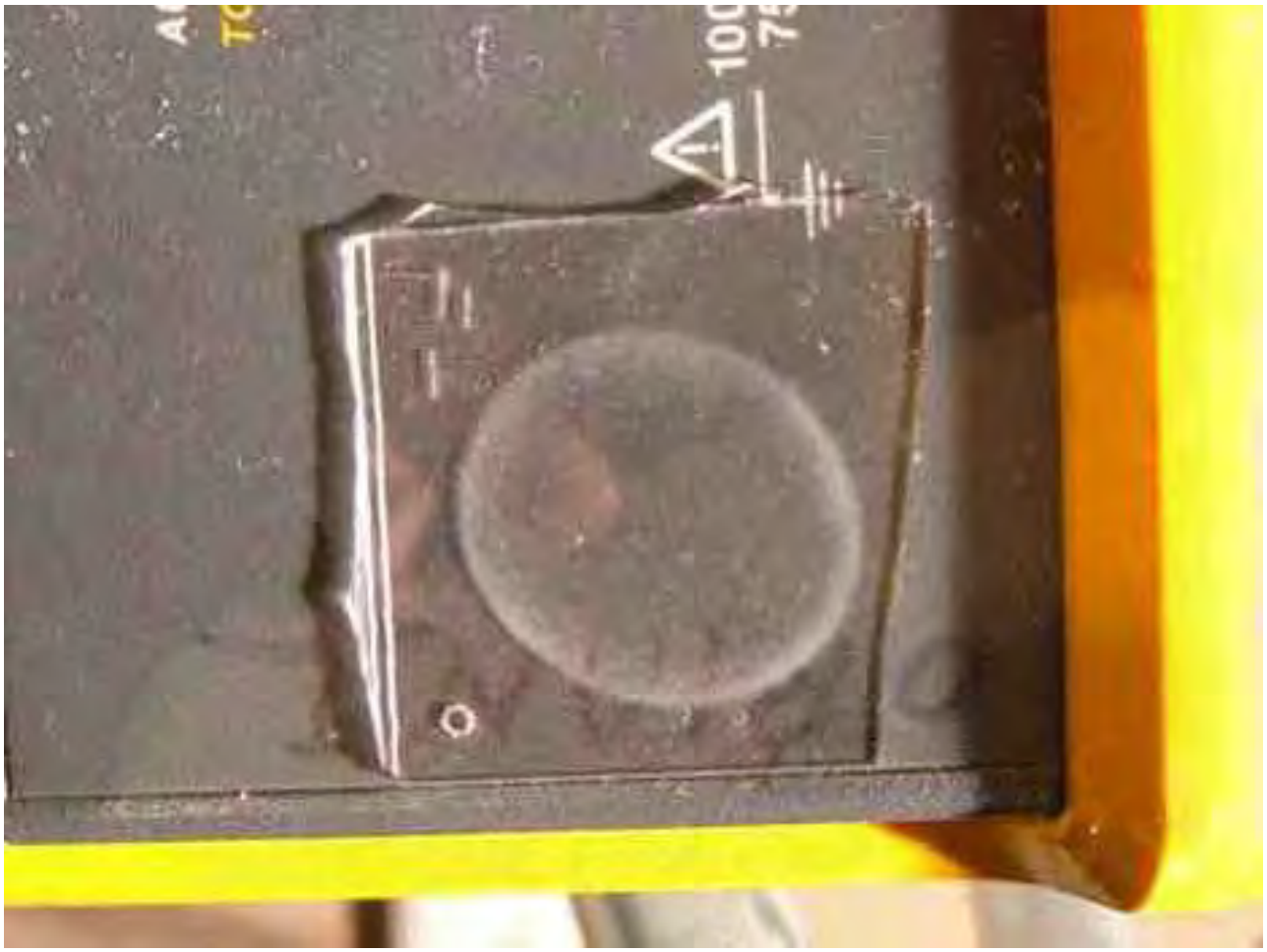

FIG. 5. Photograph of an etched detector chip upon which had been laid an o-ring previously used in an operating electrolysis cell.

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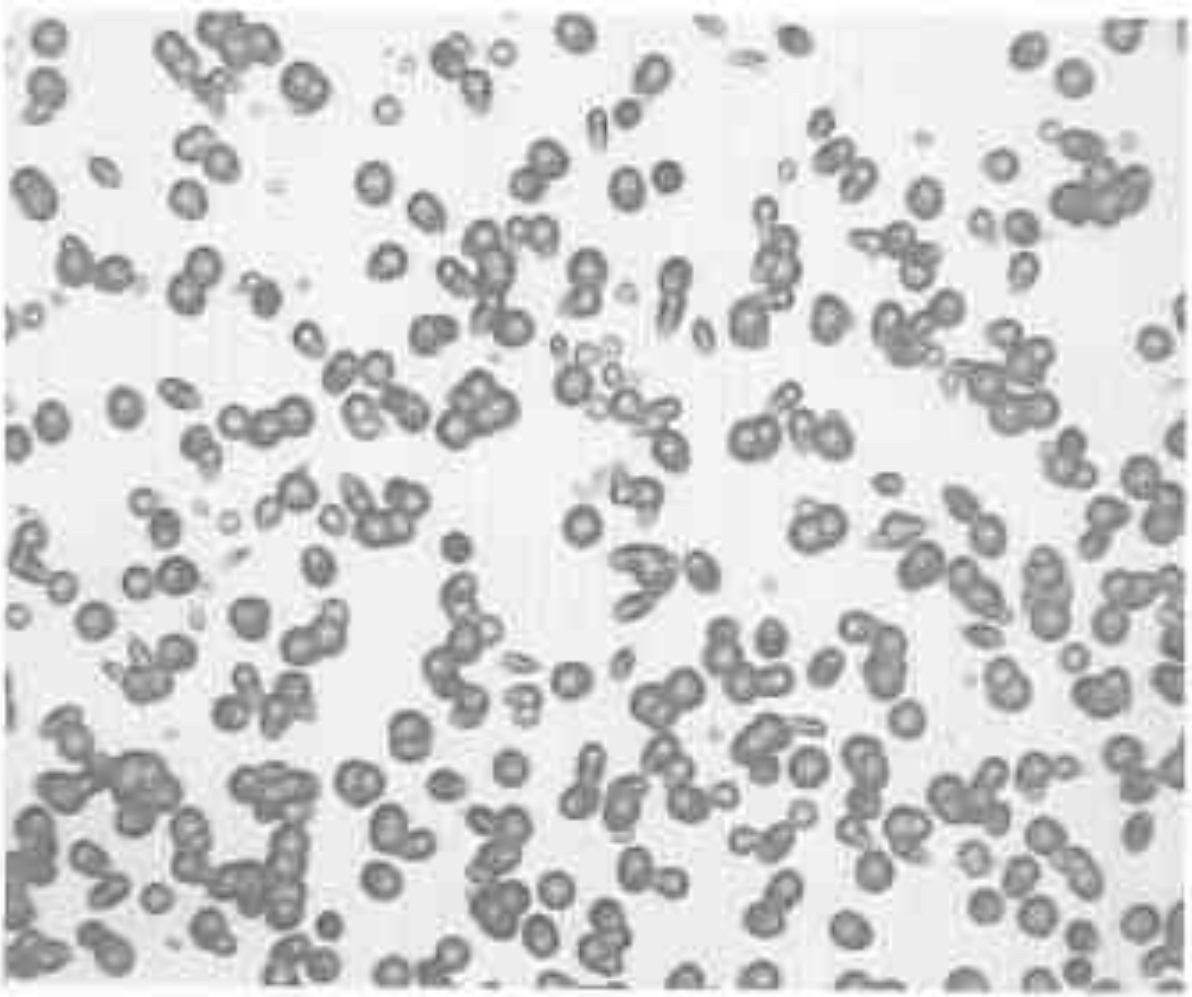

FIG. 6. Photomicrograph (originally 100X) of a portion of the circular haze region of Fig.5.

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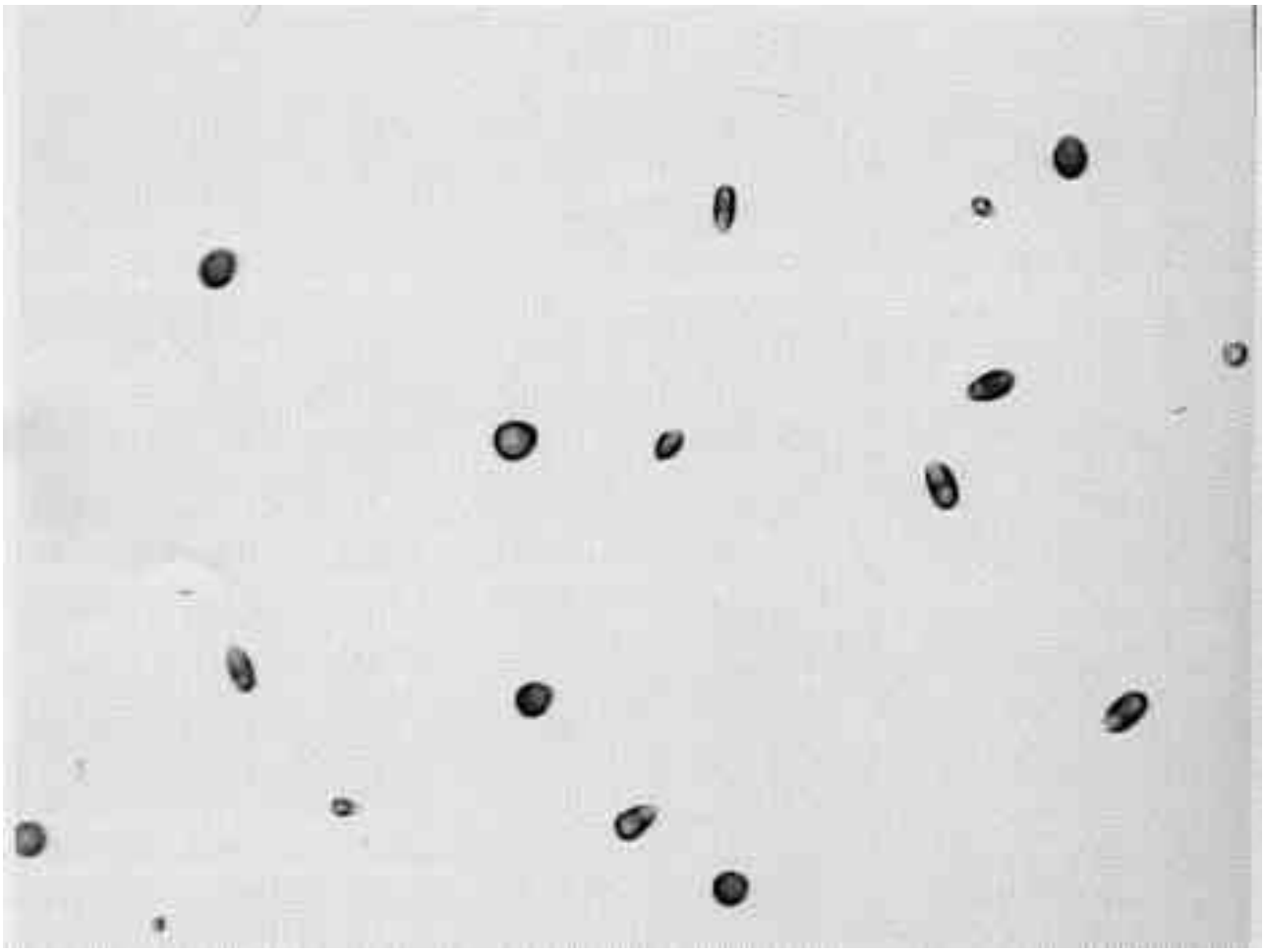

FIG. 7. Photomicrograph (100X original magnification) of 0.008 cm2 of etched detector chip very near the center of the area that had been bounded by the o-ring (see Fig.5) in a post-electrolysis experiment.

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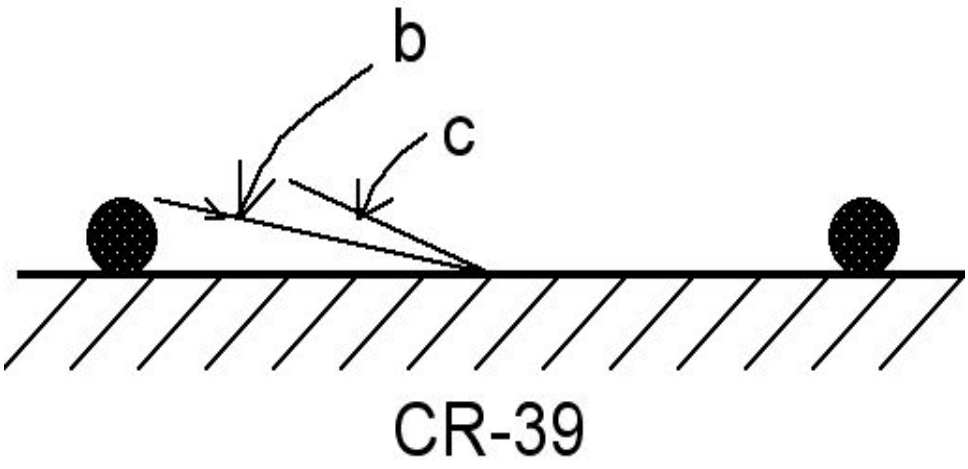

(A)

FIG. 8. Schematic diagram of the cross-section of a previously used o-ring resting upon a detector chip. Angle b, about 11 degrees, is the largest possible angle between the detector surface and the path of a nuclear particle going directly from the o-ring to the center of the area enclosed by the o-ring. Angle c is the critical angle (3) of particle impingement smaller than which etching does not produce a pit to mark the damage trail. This angle is about 36 degrees (9).

TABLE I
Results of Control Experiments, Tracks/cm^2

Configuration	First CR39 sheet Average	First CR39 sheet Std.Dev.	Second CR39 sheet Average	Second CR39 sheet Std.Dev.
Wrapped in Mylar	26.4	12.1	13.6	6.8
With new o-rings			16.6	9.7
In stock electrolyte solution			6.5	3.7
Within assembled unused cell			15.4	5.2

TABLE II
Summary of Consecutive Electrolysis Experiments. The first 11 experiments were done with the detector chips cut from the first CR39 sheet (see Table I), all of the rest were done with a second sheet.

Experiment Number	Cell	Current mA	Duration hours	Tracks/cm^2 Front	Rear
1 a	S	0.2-25	168	284	150
2 *	S	0.1-45	120	156 C	160
3 b	S	12-100	102	---	---
4	S	70-143	67	352	16
5	S	30-95	96	393 C	498 C
6	S	50	97	76	74
7	S	5-50	96.5	71 C	96
8	S	10-100	94	80	70 c
9	S	300	65	d	#
10	B	10,25	98	98	40 e
11	S	12,28	97	229 c	48 e
12	S	20	95	38 e	167 e,163 e
13	B	27	95	193 e	298
14	S	20	94	11	81 c
15	B	19	94	195	49 c
16	S	40	94	36 e	9 c, 103
17	S	39	94	127	9 c, 32 f
18	S	60	93	28	102 c
19	B	60	93	47	35 f
20	S	80	117	72 e	41 c
21	B	80	117	60 e	132 c,f
22	S	100	93	426	207
23	B	101	93	62	51 f
24	S	50	96	102	38 f
25	B	50	96	26	344 f

a The electrolyte in these two experiments was LiCl plus PdCl2 in D2O with Ag as cathode material. For all other experiments Li2SO4 in H2O with Ni as cathode was employed.
b In experiment 3, the detector chip was lost before a careful count of the clearly large number of tracks could be made.
c Some of the tracks appear in clusters.

d The number of tracks was so great that counting was impractical.
e The blue protective film supplied by the manufacturer was kept on the detector chip; in exps. 12, 16, and 17, only one-half of the detector surface was kept covered by the film.
f D2O was maintained within the closed air space below the detector chip.

TABLE III

Residual nuclear activity. The mean value of the controls for fresh o-rings upon detector chips is 16.6 with standard deviation of 9.7.

Experiment	Exposure (days)	Tracks/cm^2 Front	Rear
1	3	a,b	176
2	3	a,b	17.4
3	5	70	211
4	4	30	176
5	4	c	
6	3	16	c
7	3	78	
8	3	a,b	a
9	3	362	25c
10	3	13.4	13.4c
11	4	350	
12	4	228	20.5
13	4	a,b	a

a The number of tracks was so large that counting was impractical.

b The circle of the o-ring was followed by the swarm of nuclear tracks. See Fig. 5.
c A cluster of tracks appeared.

335) On emission of nuclear particles caused by electrolysis

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Abstract:
Abstract: Numerous tracks of charged nuclear particles, emitted during electrolysis, were discovered by Oriani and Fisher (3). More recently, residual activity -- emission of nuclear particles after electrolysis -- was discovered by Oriani (5). This presentation, prompted by my participation in The Galileo Project, describes ten experiments conducted to replicate the reported results. Common CR-39 detectors were used in these experiments. A total of eight clusters of tracks were found in two out of six electrolysis experiments. Three clusters were also found in one of four experiments conducted to study residual activity. But track densities outside clusters were always essentially the same as on control chips. Arguments are presented against trivial explanation of clusters, such as natural radioactivity and cosmic rays.

1) Introduction
1) This is an investigation of claims (1,2,3,4) that charged nuclear particles are occasionally emitted during electrolysis. The accepted point of view is that chemical processes (interactions involving outer electrons in atoms and molecules) are too weak to produce emission of nucleons from atomic nuclei. Yet, several qualified researchers, such as A. Lipson, R. Oriani and S. Jones, have been reporting unexpected emission of nuclear particles, for many years. Experimental facts that conflict with existing theories should be studied rather than rejected. My study was prompted by a recent report of Richard Oriani (5), and by participation in The Galileo Project (6). Oriani discovered, by using CR-39 detectors, that nuclear particles are emitted during electrolysis. Later he discovered that particles are also emitted after electrolysis. I will refer to what happens during electrolysis as “Oriani effect” and what happens after electrolysis as “residual activity.” The first effect has been studied by Oriani for several years. In the spring of 2007, Oriani‘s recent results were posted on the discussion list for CMNS researchers. The report shows that unexplained effects were observed in more than twenty consecutive experiments.

2) High level of reproducibility, according to Oriani, is due to residual activity. Each consecutive electrolysis experiment presumably benefits from what happens during the previous experiment. The probability of triggering Oriani effect in a new electrolytic cell is said to be very low, but it becomes very high after the first success. That is why having a residually active O-ring became part of the protocol offered to researchers, like myself, who tried to validate the claims. The first experiment had to be performed with O-rings removed from Oriani's cell, shortly after electrolysis, and received by one of us about 24 hours later. Subsequent electrolysis experiments, with the same O-rings, were expected to reliably reproduce the effect. According to Oriani‘s paper (5), distributions of tracks left by nuclear particles on CR-39 detectors are mostly uniform, more or less. But sometimes tracks appeared in the form of small clusters. The following five nuclear phenomena were identified:

a) production of clusters of tracks during electrolysis
b) production of unclustered tracks during electrolysis
c) production of clusters of tracks after electrolysis
d) production of unclustered tracks after electrolysis
e) residually active (seeded) O-rings, in a cell, increase the probability of (a) and (b).

3) A spectacular cluster of residual activity tracks, shown in Figure 1, was recorded by Oriani during a recent

experiment. In showing this photo, Oriani, wrote; “this cluster was produced in a residual effect experiment. An o-ring previously used in an electrolysis run was placed upon a detector chip and the whole wrapped in aluminum foil, then etched after three days. The very interesting fact is that the cluster appeared not on the detector surface upon which had been placed the o-ring but rather on the opposite side of the chip. Furthermore the latter side had remained covered by the manufacturer-supplied blue plastic protective film throughout the experiment. Try to explain this as a result of radioactive contamination !” The blue protective film is thick enough to stop alpha particles from radioactive substances.

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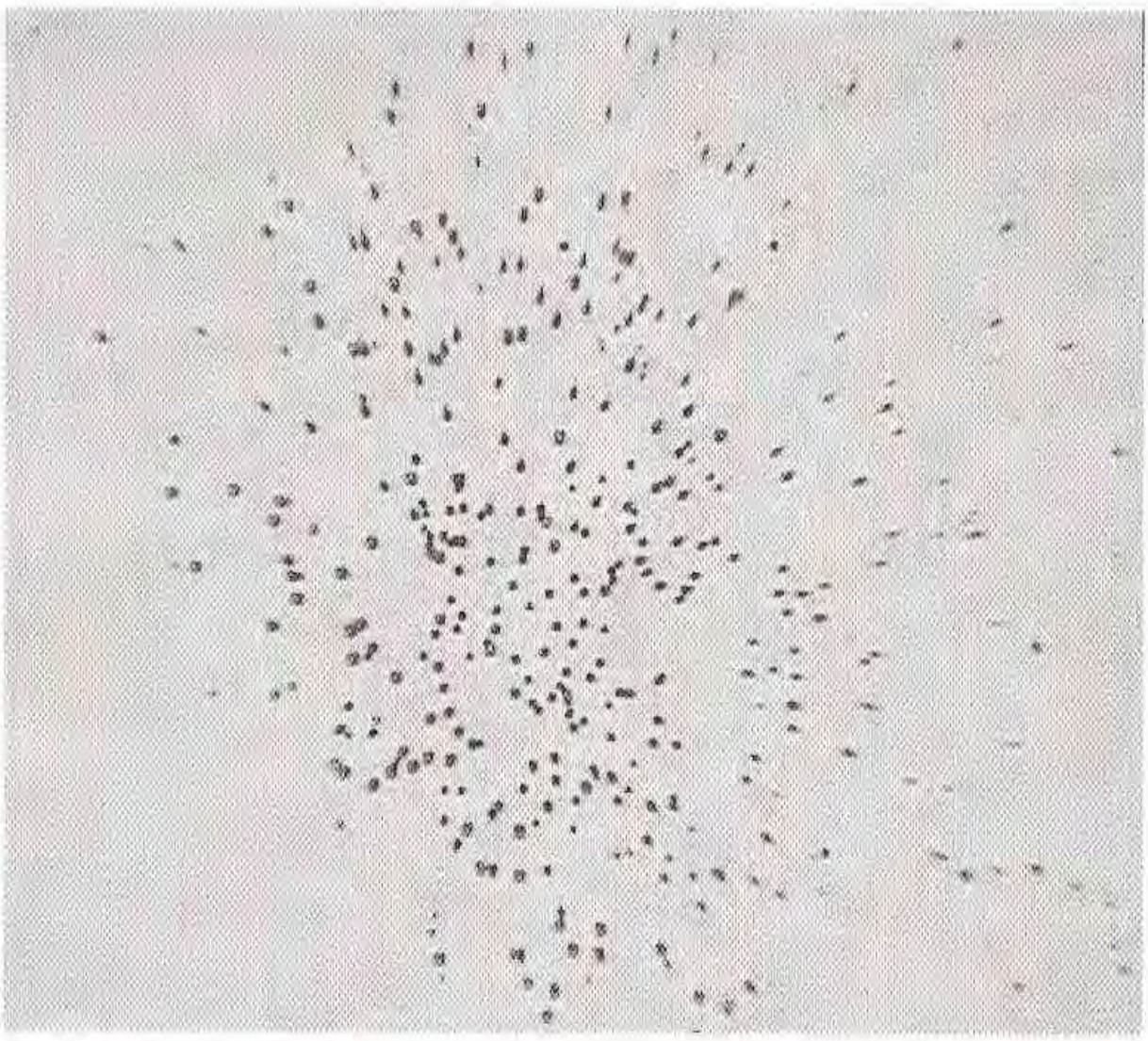

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Figure 1
A cluster produced during Oriani‘s residual activity experiment lasting three days. The magnification is X100; the photographed area is approximately 950 by 760 microns.

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What follows describes my simple apparatus and my results. In the closing section I will argue against the idea that

clustered tracks are due to natural radioactivity or cosmic rays. Paragraphs are numbered to facilitate discussion over the Internet.

2) Experimental setup and its use.

4) The electrolytic cell used in my experiments was identical to one of Oriani's cells, as schematically illustrated in Figure 2. The electrolyte was Li_2SO_4 in ordinary distilled water; the concentration was 22 grams per liter, as recommended by Oriani (5). The anode was Pt and the cathode was Ni. The cell was a glass tube containing about 10 cc of the electrolyte. The bottom of the tube was made from a replaceable 3 by 3 cm chip of CR-39 plastic material. That chip (thickness about 1 mm) was separated from the electrolyte by a layer of mylar whose thickness was 6 microns.The Ni-wire cathode, in the form of a spiral pancake, was in contact with the mylar while the Pt-wire anode, also in the form of a spiral pancake, was about 15 mm above the cathode. Potential differences applied to the cell were between about 5 and 12 volts, depending on the desired current (between 30 mA and 150 mA during preliminary experiments). At 3 V the current was close to zero. The concentration of the electrolyte was allowed to double (approximately) during experiments because water lost via electrolysis was usually not replaced till the volume of the electrolyte was reduced to approximately one half of the initial volume. The current, however, was kept constant, usually 42 mA, by an electronic stabilizer outside the power supply.

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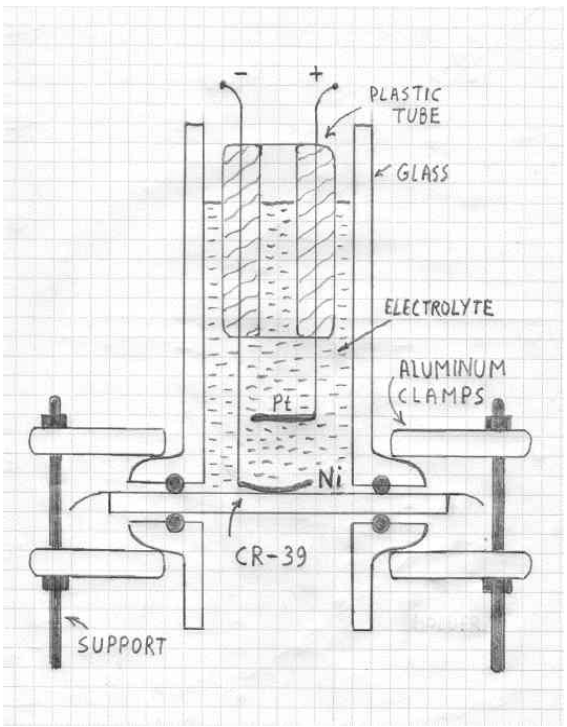
(A) -->  . (B) --> 

Figure 2
(A) The empty electrolytic cell and (B), its schematic diagram (showing the inner components). The diameter of the coin is close to 25 mm. The Ni cathode is separated from the CR-39 chip by a mylar foil whose thickness is 6 microns. Two flexible Viton O-rings are pressed against the CR-39 chip to prevent leackage of the electrolyte.

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5) The CR-39 chips were cut from a Fukuvi Chemical sheet purchased from Landauer Inc. (7) One of the well known properties of CR-39 material is its ability to record tracks of nuclear projectiles, such as protons or alpha particles (8), as illustrated in Figure 3. (A) shows tracks due alpha particles from an ^{241}Am source; (B) shows a cluster of tracks due to a tiny grain of radioactive material. To make tracks visible, under a microscope, CR-39 chips were etched in the 6.25 N solution of NaOH, at 72 C, for 6 hours.

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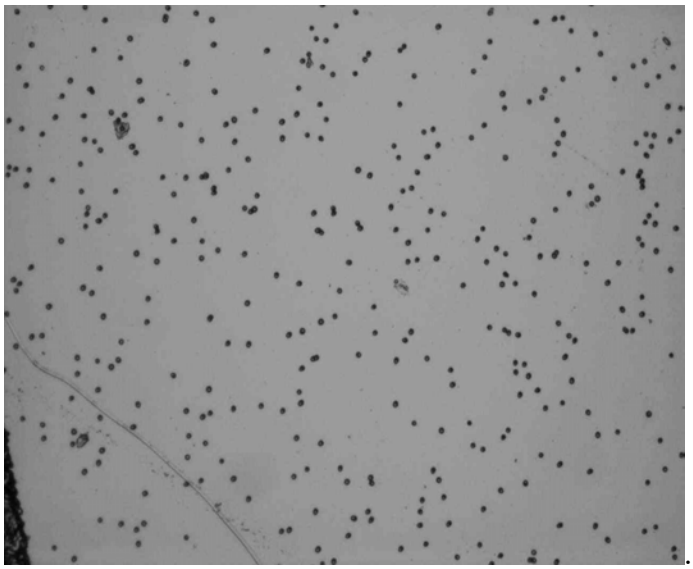
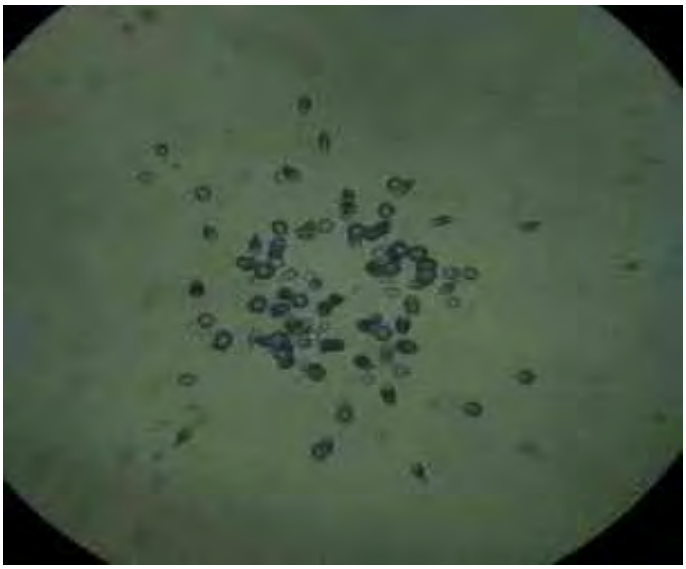
(A)  . (B) 

Figure 3 (A) Tracks of alpha particles from an americium source. Surface imperfections and dust particles can also be seen. The photographed area is 1000 by 1300 microns; magnification is X40. (B) An island of tracks around a grain containing uranium. This photo was provided by Scott Little. The magnification in (B) was higher than in (A); etching conditions were also not the same.

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6) According to (5), numbers of alpha-like tracks, produced in CR-39 during the electrolysis, greatly exceed the numbers of tracks on control chips. The densities of tracks in control chips fluctuated between approximately 5 and 25 tr/cm^2. The densities on electrolysis chips, on the other hand, were most often several hundred. In some cases they were "too high to count." My experiments failed to confirm these observations, except for small clusters of tracks. Oriani's paper has a table with numerical results (track densities) from about 20 consecutive experiments. The label C, which stands for "cluster" appears 14 times in his table. It indicates that clusters of tracks were often observed. By my definition a cluster is an island of many tracks surrounded by track-free areas, as illustrated in Figure 1. In this paper I will describe results from ten experiments: I, II, III, IV, V, VI, VII, VIII, IX and X. The first six of them were performed to study the (a) and (b) effects, described in Section 1. The last four experiments were performed to study the effects (c) and (d). What follows is a brief description of these experiments. Pictures of my clusters will be shown in Sections 3 and 4.

** Electrolysis in Experiment I lasted 5 days, using Oriani's seeded O-rings. The rings were received 24 hours after they were removed from Oriani's cell. The experiment started shortly after the package arrived.
** Electrolysis in Experiment II lasted 21 days, using the same O-rings as in Experiment I. The experiment started shortly after the end of Experiment I.
** Electrolysis in Experiment III lasted 5 days, using the same O-rings as in Experiment II. The experiment started shortly after the end of Experiment II.
** Electrolysis in Experiment IV lasted 5 days, using another freshly seeded O-ring from Oriani. The experiment started shortly after that O-ring was received.
** Electrolysis in Experiment V lasted 5 days, using the same O-ring as in Experiment IV. The experiment started shortly after Experiment IV.
** Electrolysis in Experiment VI lasted 5 days, using the same O-ring as in Experiment V. The experiment started shortly after the end of Experiment V.
**** Experiment VII was conducted outside the electrolytic cell. Several small CR-39 chips were applied, for two days, to O-rings used in experiment III.
**** Experiment VIII was also conducted outside the electrolytic cell. Two large CR-39 chips were applied, for two days, to another freshly seeded O-ring sent to me by Oriani.
**** Experiment IX was similar to Experiment VIII, except it started at the end of Experiment VIII.
**** Experiment X was similar to Experiment IX, except it started at the end of Experiment IX.

7) Four clusters of tracks were found on the electrolysis chip from Experiment I, zero clusters were found on the electrolysis chip from Experiment II and four clusters were found on the electrolysis chip from Experiment III. Experiments IV, V and VI failed to produce clusters. Zero clusters were found in Experiment VII. But three clusters were found in experiment VIII. Experiments IX and X also failed to produce clusters. In all ten experiments, track densities outside clusters were found to be essentially the same as on control chips. In other words, effects (b) and (d) were not produced in my experiments.

3) Clusters of tracks:
Figure 4 shows the first cluster found in Experiment I. It occupies an area larger than other clusters produced on the same CR-39 chip. That is why two fields, (A) and (B), were needed to show all of its tracks. Tracks in that cluster seem to be less dark (shallower ?) than tracks on others clusters.

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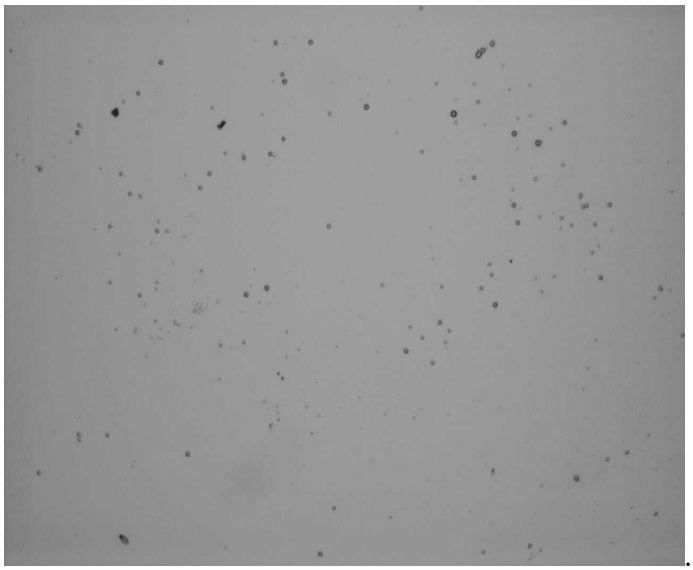
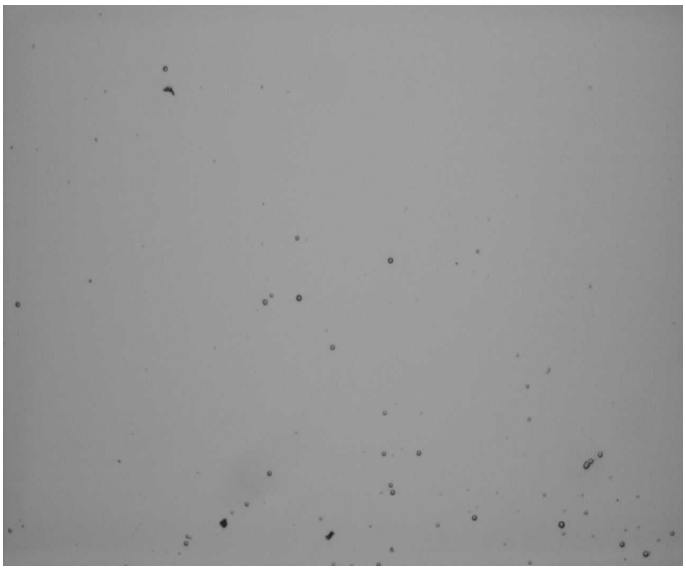
(A)  . (B) 

Figure 4
Tracks of my largest cluster. It was produced during electrolysis Experiment I. Each of the two photographed areas is a field of 1000 by 1300 microns; magnification is X40. Most of the tracks appear in the field (A); the field (B) shows the remaining tracks of the same cluster. Some tracks in the upper part of (A) can also be seen in (B). The background outside the cluster was most often less than one track per field.

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8) Two smaller clusters, with much darker tracks, are shown in Figures 5A and 5B. Note that tracks of the cluster of Figure 5A are mostly circular while a large fraction of tracks in the clusters of Figure 5B are conical or elliptical. Significance of non-circular tracks will be discussed later.
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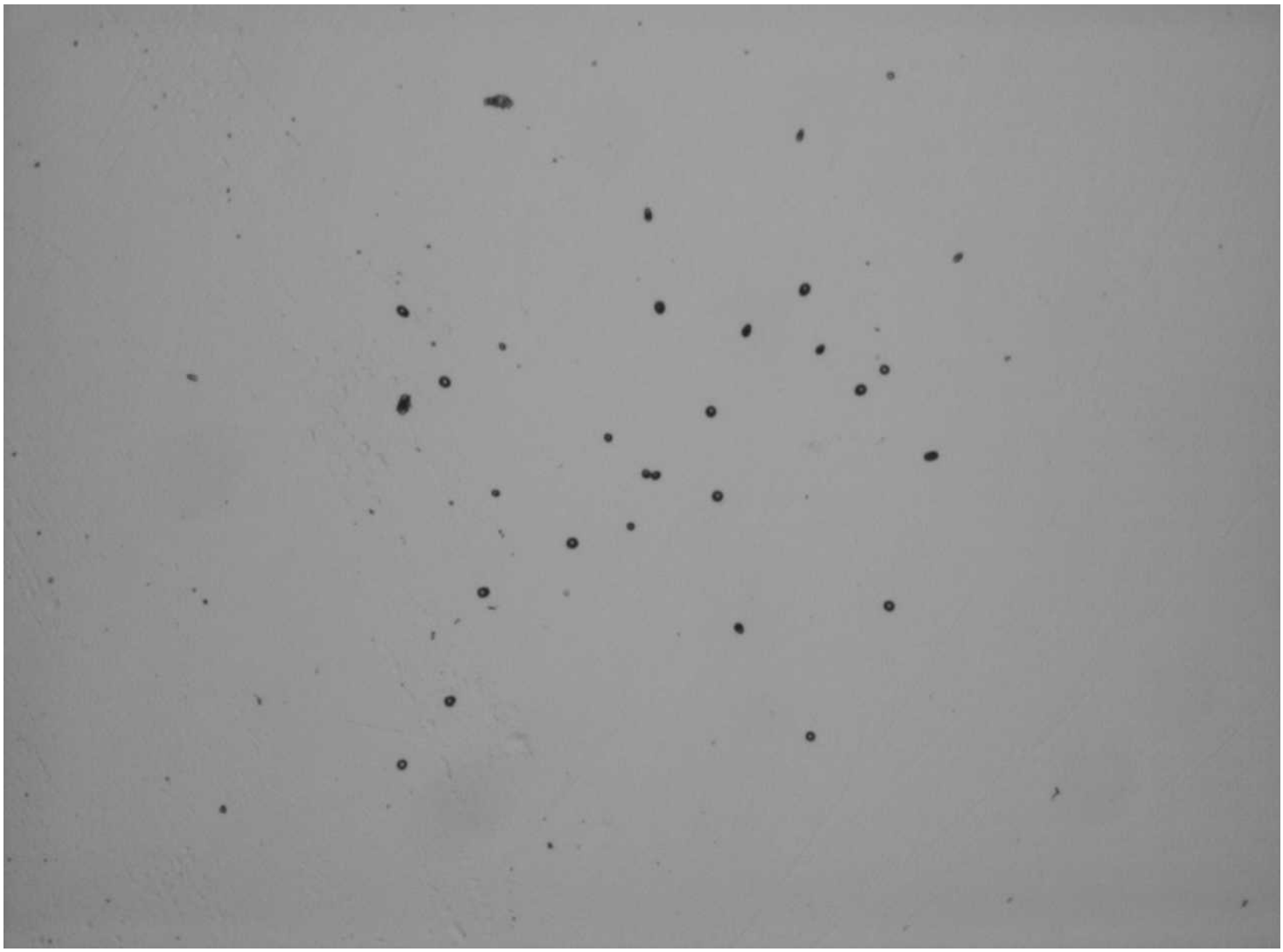
(A) 

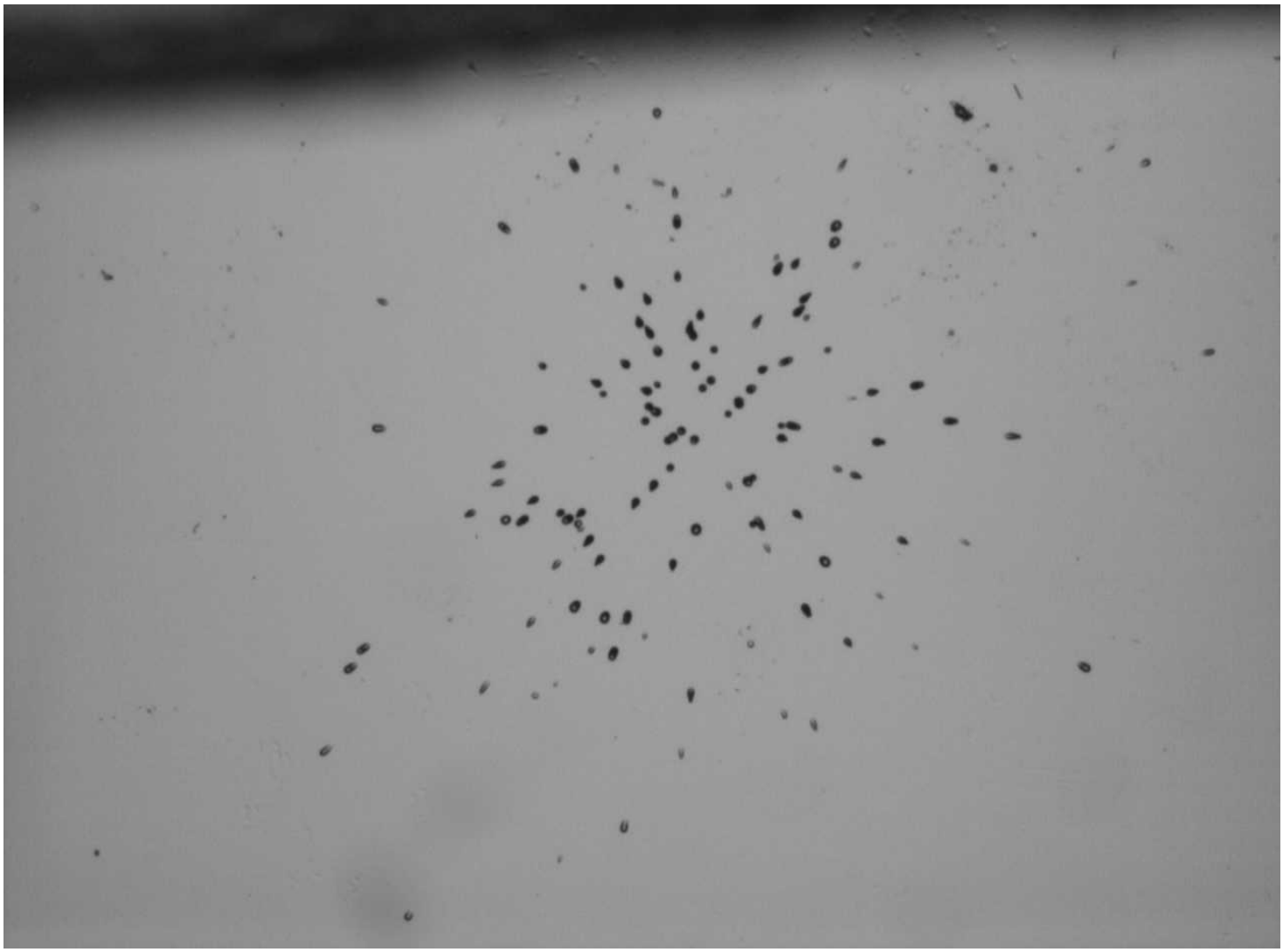
(B) 

Figure 5 (first A then B)
Two clusters also produced during my electrolysis experiment I. Each photographed area is 1000 by 1300 microns; each magnification is X40. The etching time was 6 hours. Figure 8 will show the same cluster after additional 5 hours of etching.

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9) Not a single cluster of tracks was found on the control chip. This is significant in the context of a discussion that subsequently evolved over the Internet. Little and Little (9) also recorded clusters by using seeded O-rings. But they suspect that clusters might be due to tiny particles of radioactive material, such as uranium or radium. Figure 3A shows a cluster of tracks around a grain of uranium-containing material; they deliberately deposited it on CR-39 to illustrate the effect. My control chip had the same history as my electrolysis chip. In Section 6 I will explain why, in my opinion, the cluster shown in Figure 5B, and Oriani's cluster shown in Figure 1, cannot be due to a tiny spot of alpha-radioactive contamination. Several non-circular tracks, under ten times higher magnification, are shown in Figure 6. They belong to the cluster shown in Figure 5B.
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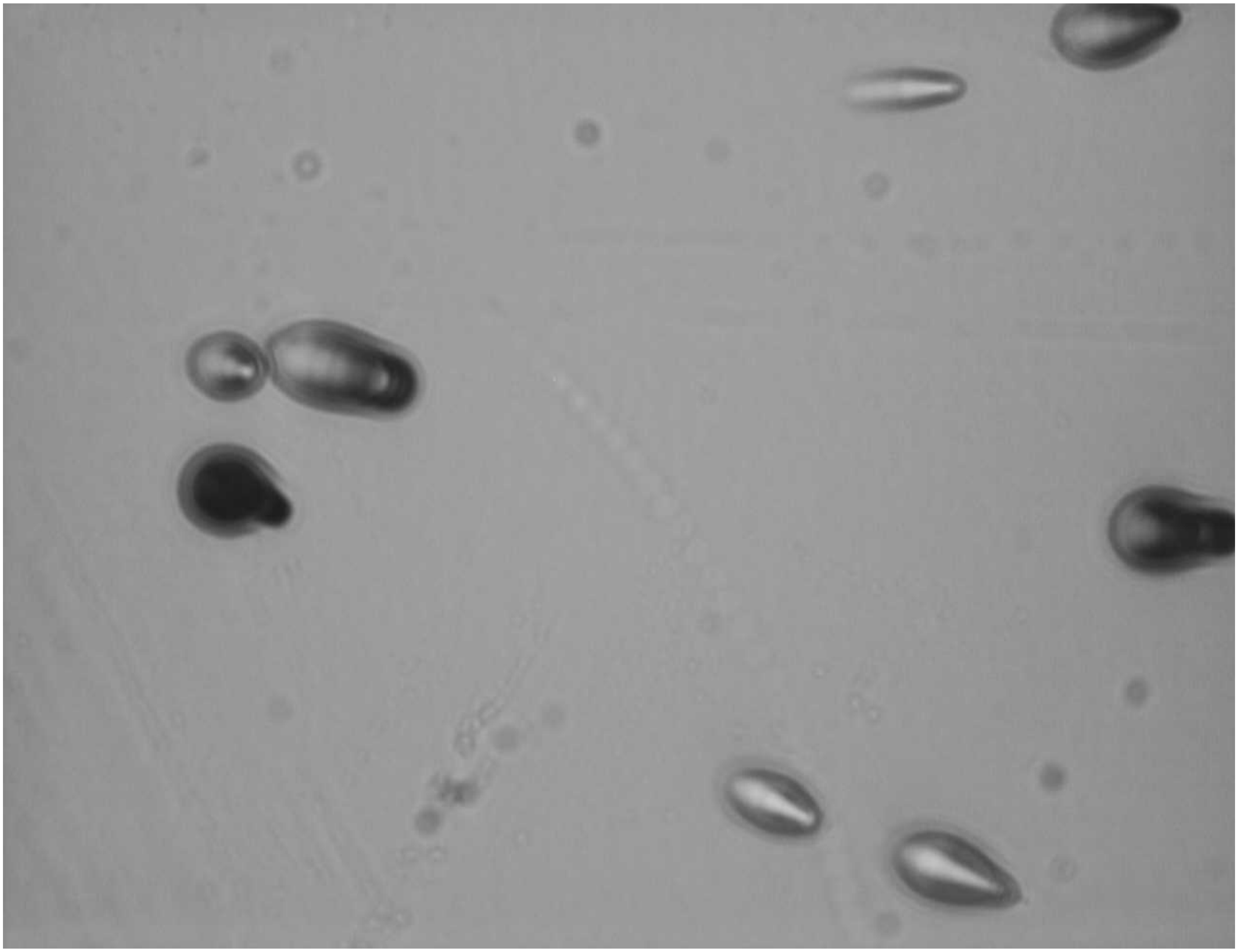

Figure 6
Some elliptical and conical tracks under the magnification of X400. The photographed area is 100 by 130 microns.

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10) The fourth cluster, produced during the experiment I, was not photographed when the chip was examined after 6 hours of etching. I probably misidentified it as the already-photographed island of tracks. But the fourth cluster was immediately recognized after an additional 5 hours of etching. The photograph of that cluster, with much larger tracks (due to additional etching) is shown in Figure 7.

It is remarkable that average track densities, within clusters, are very much higher than the mean 15 tr/cm^2 background. For the cluster shown in Figures 5A the local density was close to 6000 tr/cm^2; this is 400 times higher than the mean background. For the cluster shown in Figures 5B the local density was close to 25000 tr/cm^2; this is about 1700 times higher than the expected background. For the cluster shown in Figures 7 the local density was close to 20000 tr/cm^2. This is again three orders of magnitude above the background. Additional etching did not produce many more excessive tracks, as expected by Oriani. The large cluster, shown in Figures 4A and 4B also had about 90 tracks. But the area covered by these tracks was larger than for other clusters. The mean local density, for that cluster, turned out to be close to 7000 tr/cm^2.

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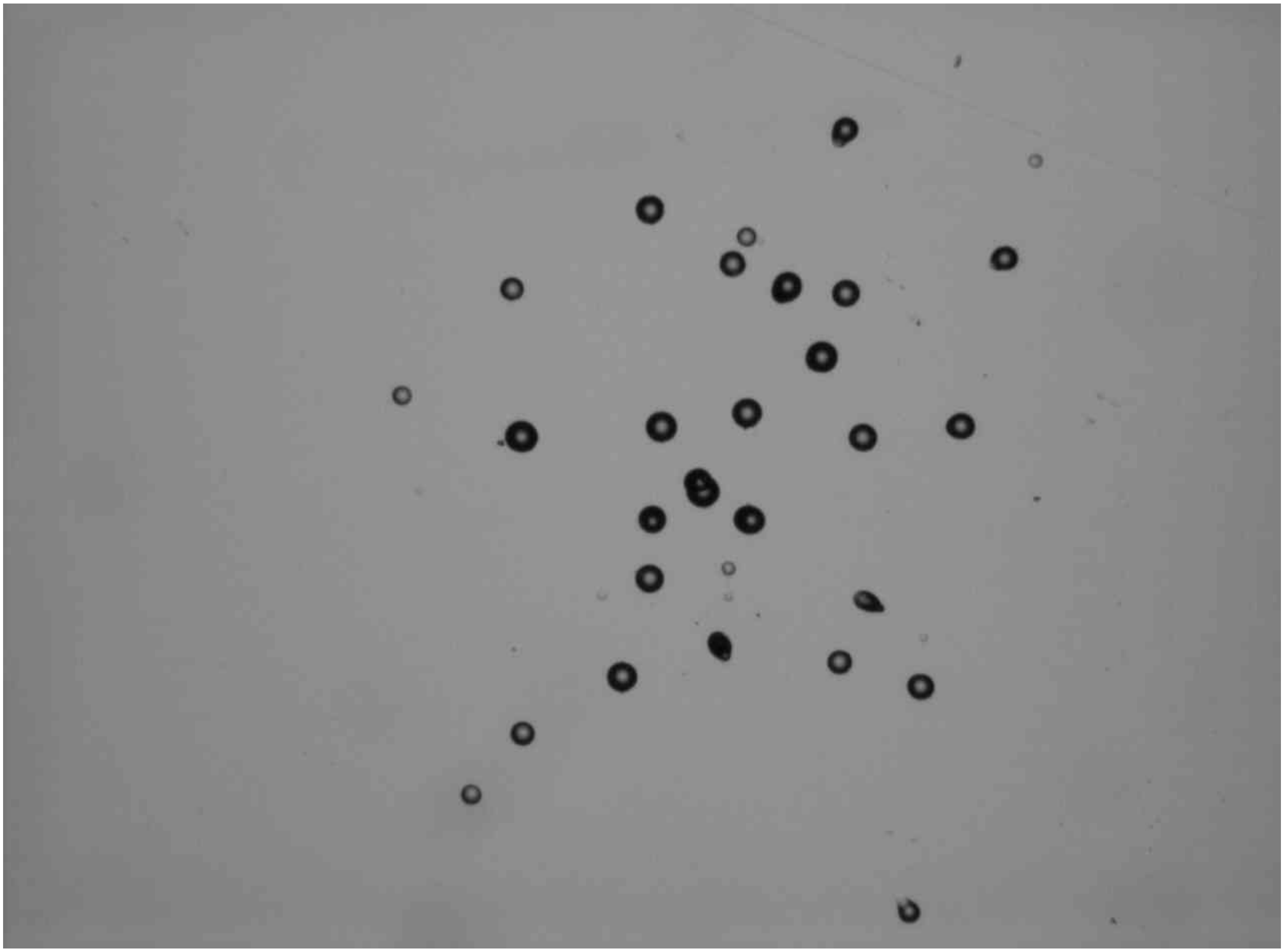

Figure 7
Another cluster from experiment I, after 11 hours of etching. The photographed areas is 1000 by 1300 microns; magnification is X40.

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11) One detail is worth mentioning before describing results from other electrolysis experiments. Comparing Figure 5B and Figure 8, which are photos of the same cluster, one can see that many tracks that were elliptical in Figure 5B became nearly circular in Figure 8. That was an important lesson; over-etching makes counting easier but it tends to destroy important information. In fact, etching for only 4 hours, instead of 6 hours, is likely to be more desirable, when information provided by orientation of tracks is important.

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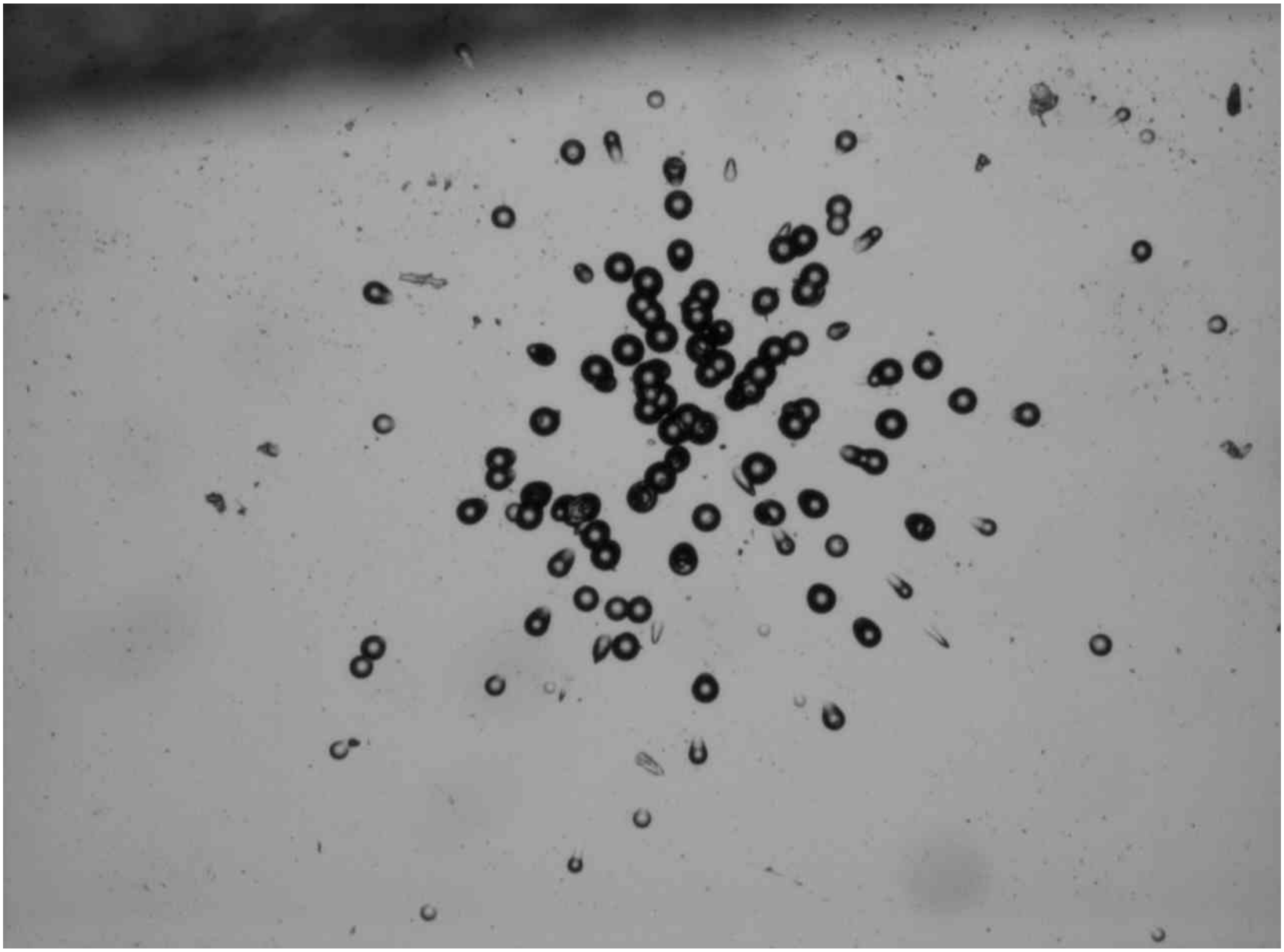

Figure 8
Same cluster as in Figure 5B but after 11 hours of etching. The photographed areas is 1000 by 1300 microns; magnification is X40.

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12) Not a single cluster, or excessive average track density, was found on the CR-39 chip used in Experiment II. It is easy to show that the probability of not recording a single cluster in 21 days, when four clusters were produced in 5 days, would be very low for random fluctuations. An important precondition for Oriani effect was probably not satisfied during experiment II. Clusters are not reproducible on demand. But Experiment III again produced four clusters. Three of them are shown in Figures 9A, 9B and 10A. Track densities within these clusters are 24000 tr/cm^2, 615 tr/cm^2 and 2385 tr/cm^2, respectively. Experiments IV, V and VI, however, failed to produce clusters.

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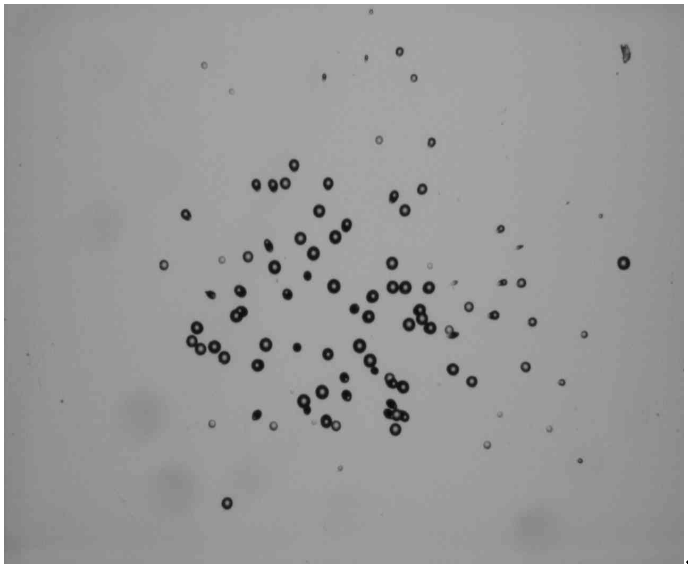
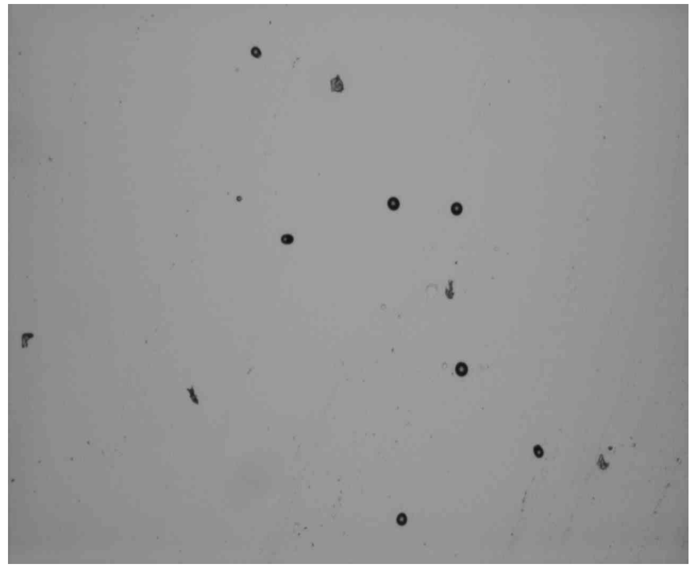
(A)  . (B) 

Figure 9
Two clusters produced during experiment III. Each photographed area is 1000 by 1300 microns; each magnification is X40.

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13) The cluster in Figure 10B is very peculiar. This becomes evident when it is seen under the usual magnification, as in Figure 11. The tracks are very close to each other; they can be surrounded by a circle whose diameter is comparable to ranges of alpha particles in CR-39 (10) or in the electrolyte.
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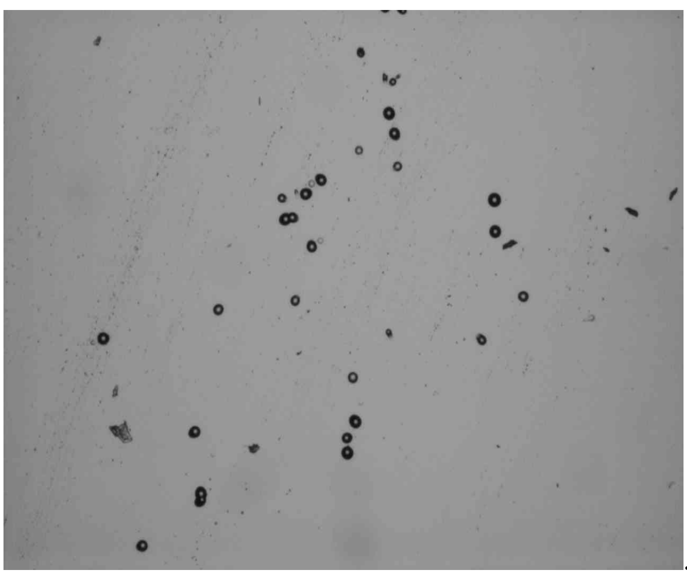
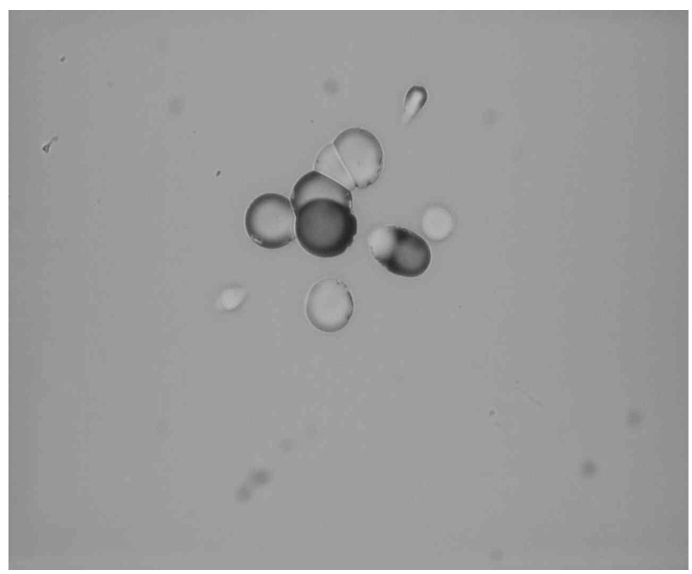
(A)  . (B) 

Figure 10
Two more clusters produced during Experiment III. The photographed area in A is 1000 by 1300 microns, under magnification X40. The photographed area in B is 200 by 260 microns, under higher magnification X200.

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On that basis it is reasonable to suspect that several near-by tracks might be due to alpha particles emitted from a tiny grain of radioactive material, such as radium or uranium. Similar patterns of near-by tracks, named rosettes, were reported by Oriani. He considers them to be part of unexplained background rather than clusters due to electrolysis.

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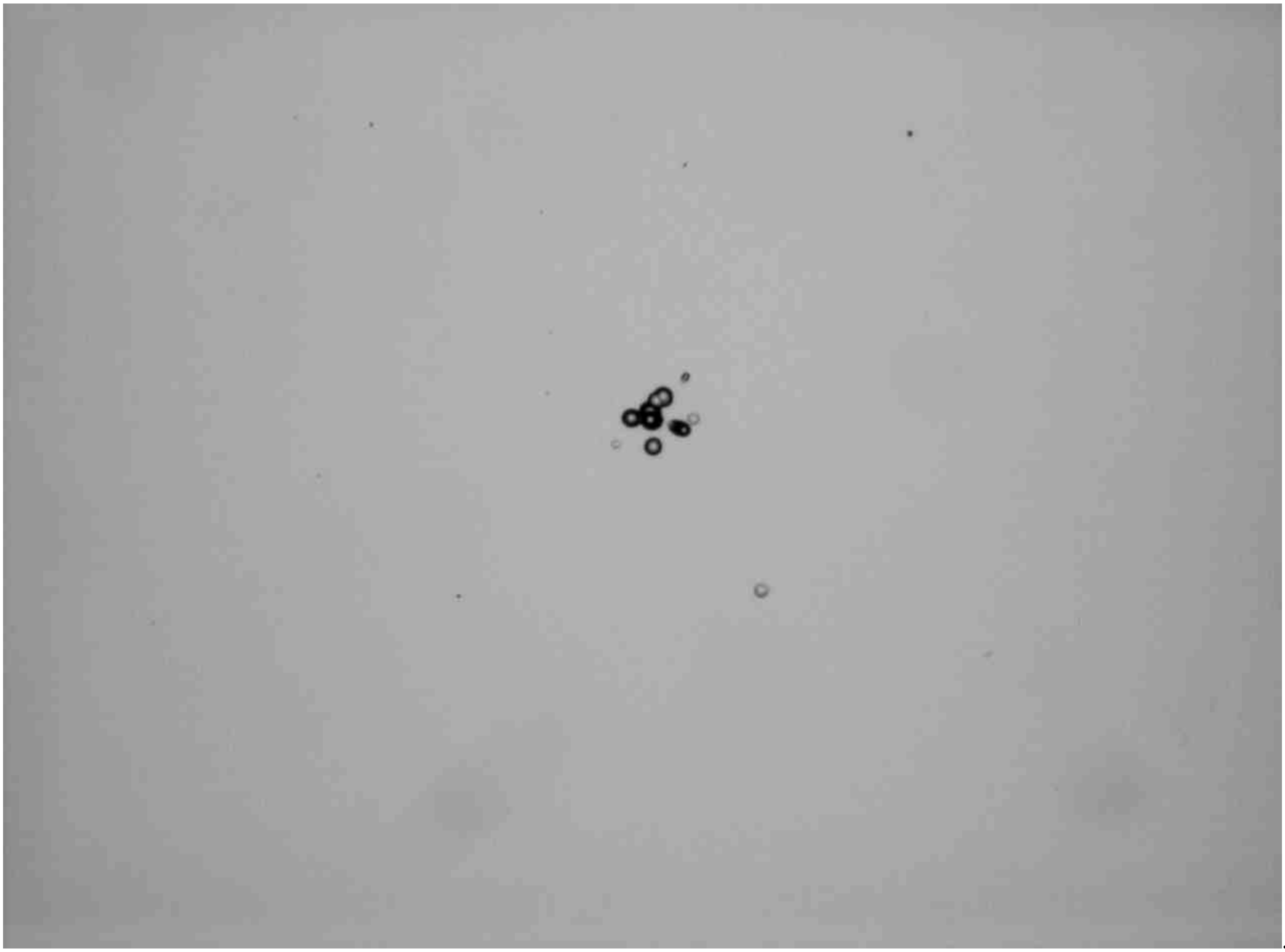

Figure 11
A rosette cluster produced during the experiment III. The photographed areas is 1000 by 1300 microns; magnification is X40. The etching time was 6 hours. Notce the usual low background, outside the rosette -- typically 0.5 tracks per field of that size.

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4) Clusters due to residual activity
14) Residual activity has been mentioned in Section 1 above. It is presumably induced in O-rings during electrolysis. Before using O-rings in the electrolytic cell (to induce residual activity) Oriani exposes them to CR-39 detectors, for many days, This is done to demonstrate that no alpha radioactive contamination is present on the surfaces. My own examination of new Oriani O-rings confirmed absence of measurable surface contamination. A test for residual activity begins after O-rings are removed from the electrolytic cell, usually after about four days of electrolysis. Oriani does this in his laboratory and sends the O-rings to those who desire to replicate experiments. A residual-activity test in progress is shown in Figure 12.

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(A)

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Figure 12
Residual activity setup. An O-ring is sandwiched between two CR-39 chips. The setup is supported by a test tube holder. A film of protective plastic, unpeeled from a chip, is shown on the right.

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15) Two large clusters (about 6000 by 6000 microns each) were produced in Experiment VIII, one on each chip. The areas are much larger than for clusters produced during the electrolysis. Unfortunately, my lowest magnification, X40, was still too high to produce single pictures of entire clusters. Only centers of these clusters are shown in Figure 13. The average densities within these clusters turned out to be close to 250 tr/cm^2. This is 17 times higher than the mean backgrund.

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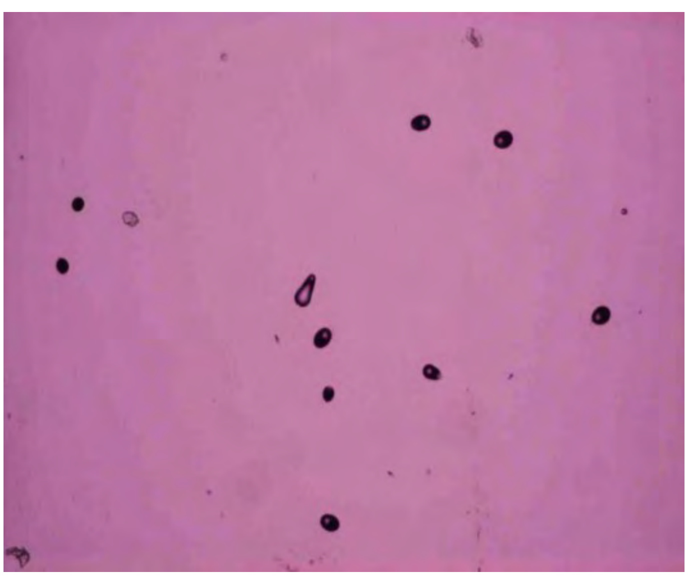
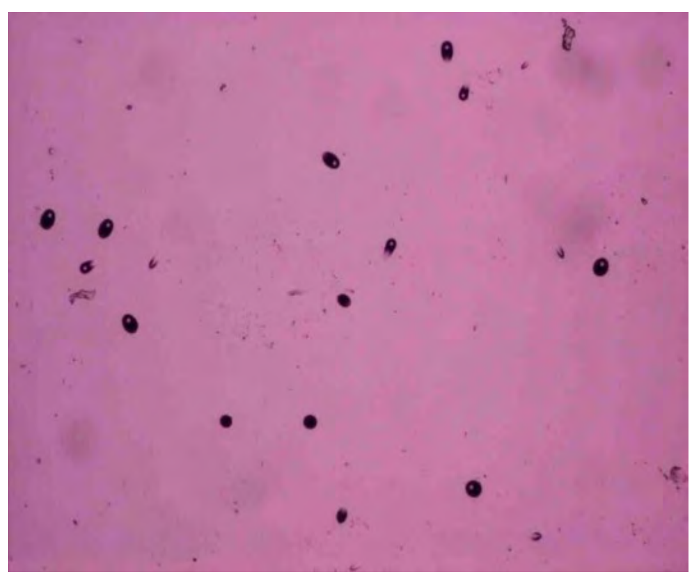
(A)  . (B) 

Figure 13
Central areas of tracks from two large clusters produced in Experiment VIII. Each photographed area is 1300 microns by 1000 microns; magnification is X40.

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16) A very small cluster, also from experiment VIII, is shown in Figure 14A. Its tracks are nearly twice as large as tracks of alpha particles, shown in Figure 14B. Alpha particles were recorded at a corner of the chip on which the small cluster was found. That means that etching conditions were identical. What kind of projectiles can produce tracks two times larger that those due to alpha particles of 4 MeV? Note that tracks in Figure 13 are also different from tracks due to alpha particles from my source. But they are not larger. The local density of 13 large tracks in Figure 14 A is about 5000 tr/cm^2. This is 330 higher than the mean background.

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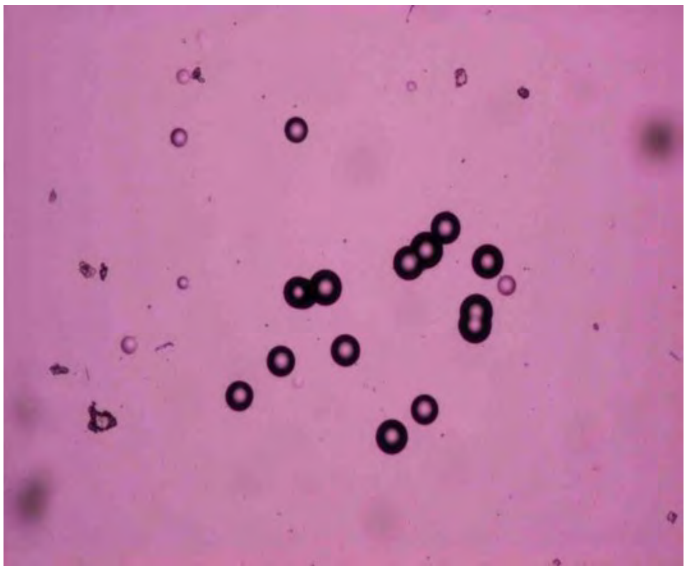
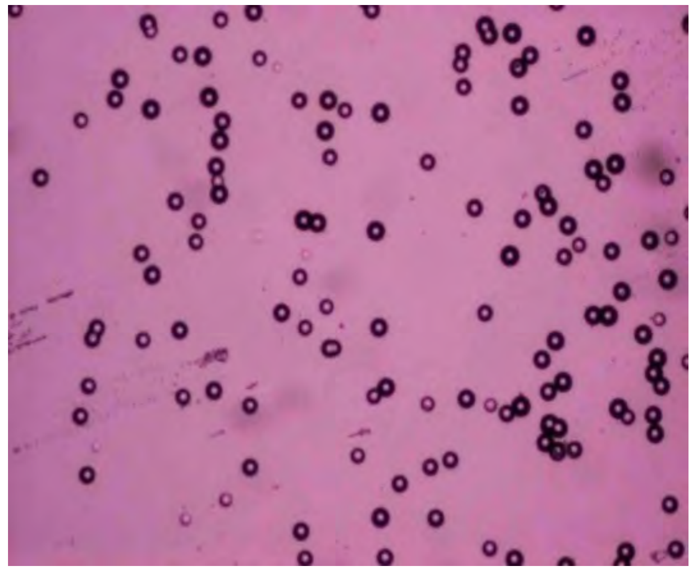
(A)  . (B) 

Figure 14
(A) A small cluster of very large tracks from experiment VIII. (B) Tracks of alpha particles of approximately 4 MeV. Each photographed area is 1300 microns by 1000 microns; the magnification is X40.

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5) Clusters of electrolysis tracks reported by other investigators
A photo of a cluster, recorded by Oriani during electrolysis experiment, is shown in Figure 15. The mean track density, within the shown cluster area (about 1 mm^2) is close to 22000 tr/cm^2. This is at least 5000 times larger than the expected background.

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(A) ---> .

Figure 15

Oriani's cluster recorded in CR-39 during electrolysis. The number of tracks in that cluster is about 220. The total photographed area of that composite photo was approximately 760 microns by 1300 microns. Orientations of elliptical tracks near the cluster periphery seem not to be random.

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17) Figure 16 shows two clusters recorded by John Fisher during electrolysis. The chips were sent to me and I photographed the cluster. The large fuzzy spots are tracks on the opposite side of the CR-39 chip. Their sizes are exaggerated because tracks on the other side are out of focus. The cluster was located on that part of the chip that was in air during the three day-long experiment.

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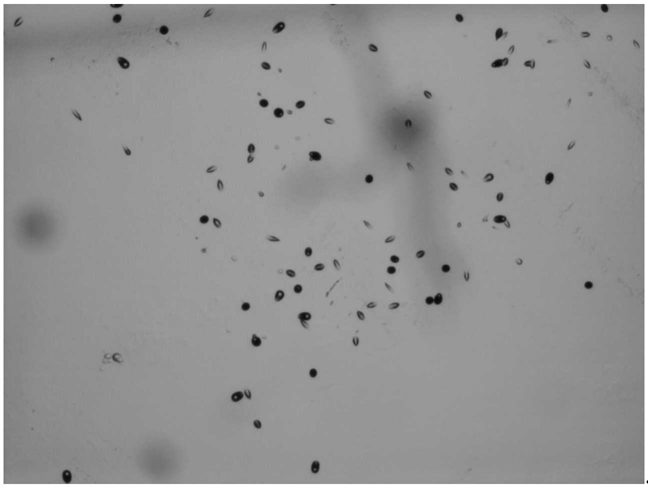
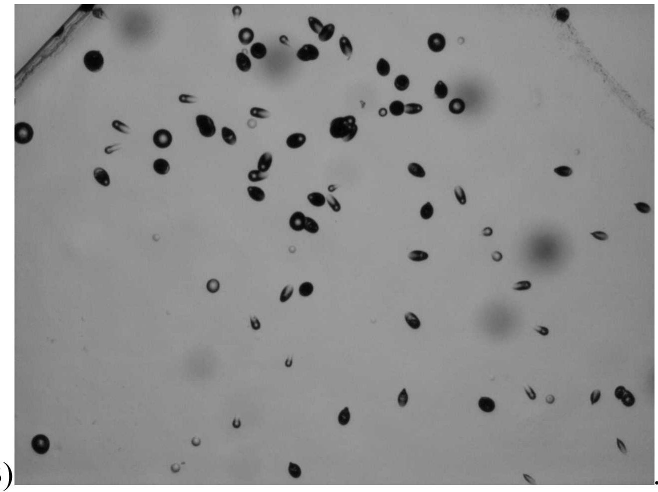
(A)  . (B)  .

Figure 16
Two of Fisher clusters recorded in CR-39 during the electrolysis. The magnification was 40 and the photographed area was 1300 microns by 1000 microns.

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19) Figure 17 shows an electrolysis cluster reported by Marissa and Scott Little (9). In posting the picture Marissa wrote: “There is an obvious cluster of tracks on the chip! The cluster is on the side facing away from the cathode and is outside the o-ring area. There are several, much smaller clusters on the area outside the o-ring as well (containing 3-10 tracks.)” Most of the tracks are inside an area whose diameter is about 1400 microns. The mean track density, in this area, is close to 6000 tr/cm^2. This is about 1300 times higher as the average background on their new CR-39. At the time of this writing Scott and Marissa are trying to disprove their “null hypothesis.” According to that hypothesis, excess tracks are due to radioactive contamination, most likely to a radon progeny.

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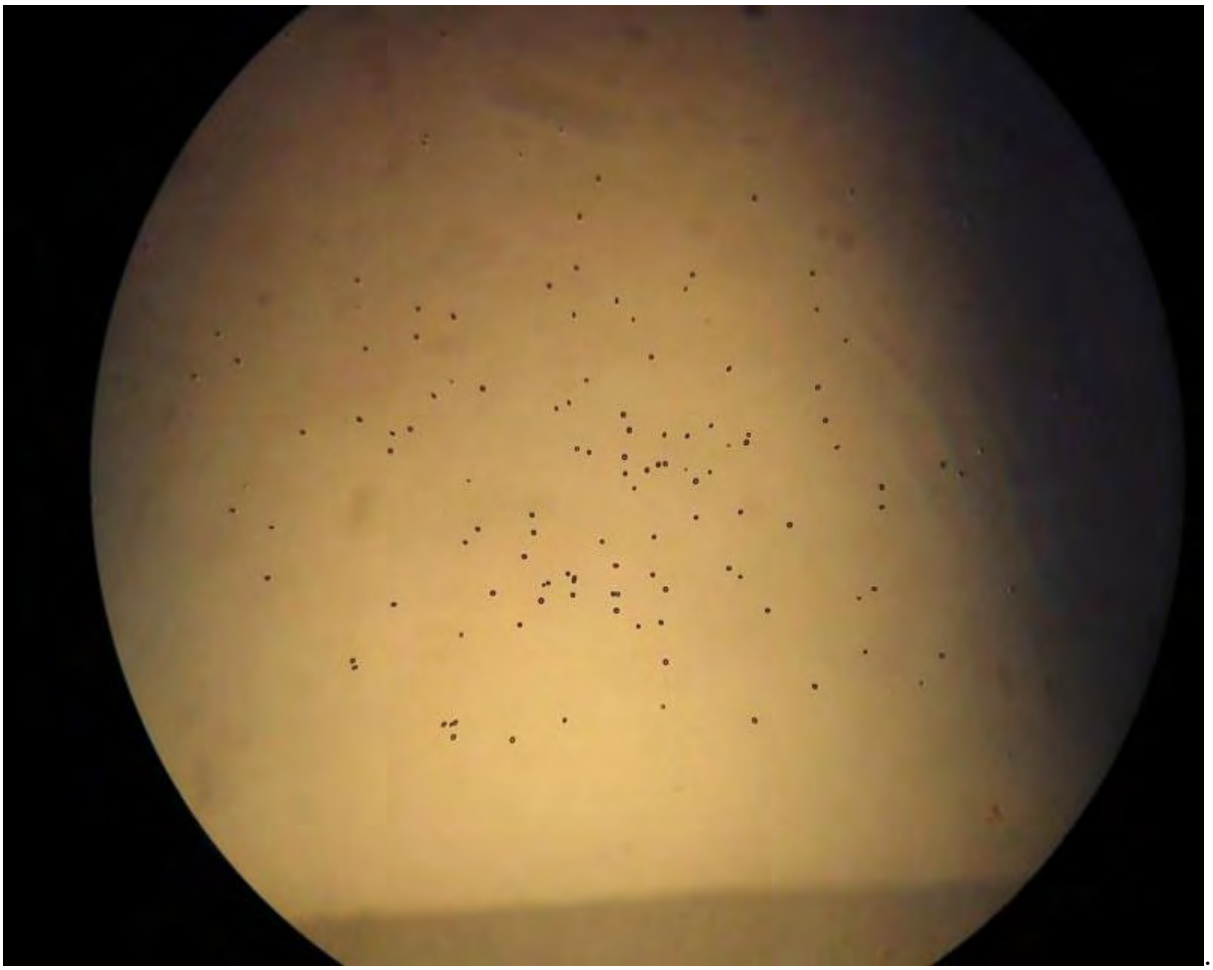
(A) --->  .

Figure 17
Little's cluster recorded in CR-39 during electrolysis. It was the only large cluster produced in 30 experiments. The magnification was 40 and the photographed area has the diameter of 2800 microns.

Clustering of tracks indicate that nuclear reactions are localized in space. Unfortunately, clustering tells us nothing about localization in time. Are clusters produced in sudden bursts of activity or via slow accumulation of tracks? Such questions can not be answered by those of us who use CR-39 detectors. But they are likely to be answered by users of electronic detectors. Data collected by these detectors can be recorded frequently, for example every six hours, without interrupting long experiments. Electronic detectors, providing information about energies of intercepted particles, can also be used to identify particles.

= =

6) Discussion:
20) The cluster shown in Figure 5 B has 109, plus or minus 10 tracks; the uncertainty is due to the arbitrary value of the "threshold of rejection." That is a common problem in counting tracks on CR-39 surfaces, especially when these surfaces are not clean. The most remarkable feature of that cluster is a large number of nearly-elliptical tracts. Orientations of these tracks are certainly not random. Frank DiBianca, after seeing the picture spent some time in tracing straight lines along the focal axes of non-circular tracks, as illustrated in Figure 18. Considering the angular uncertainties in drawing the lines, and possible scattering effects, the picture is consistent with the idea that all lines intersect at one point. The diameter of the small circle, 75 microns, is about ten times smaller that the diameter of the area in which most of the tracks are located. The size of the cluster is large in comparison with ranges of typical alpha particles. For 6 MeV particles, for example, the range in CR-39, or in electrolyte, is close to 50 microns (10). A similar set of lines can be drawn through numerous elliptical tracks shown in Figure 1.

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(A) ->

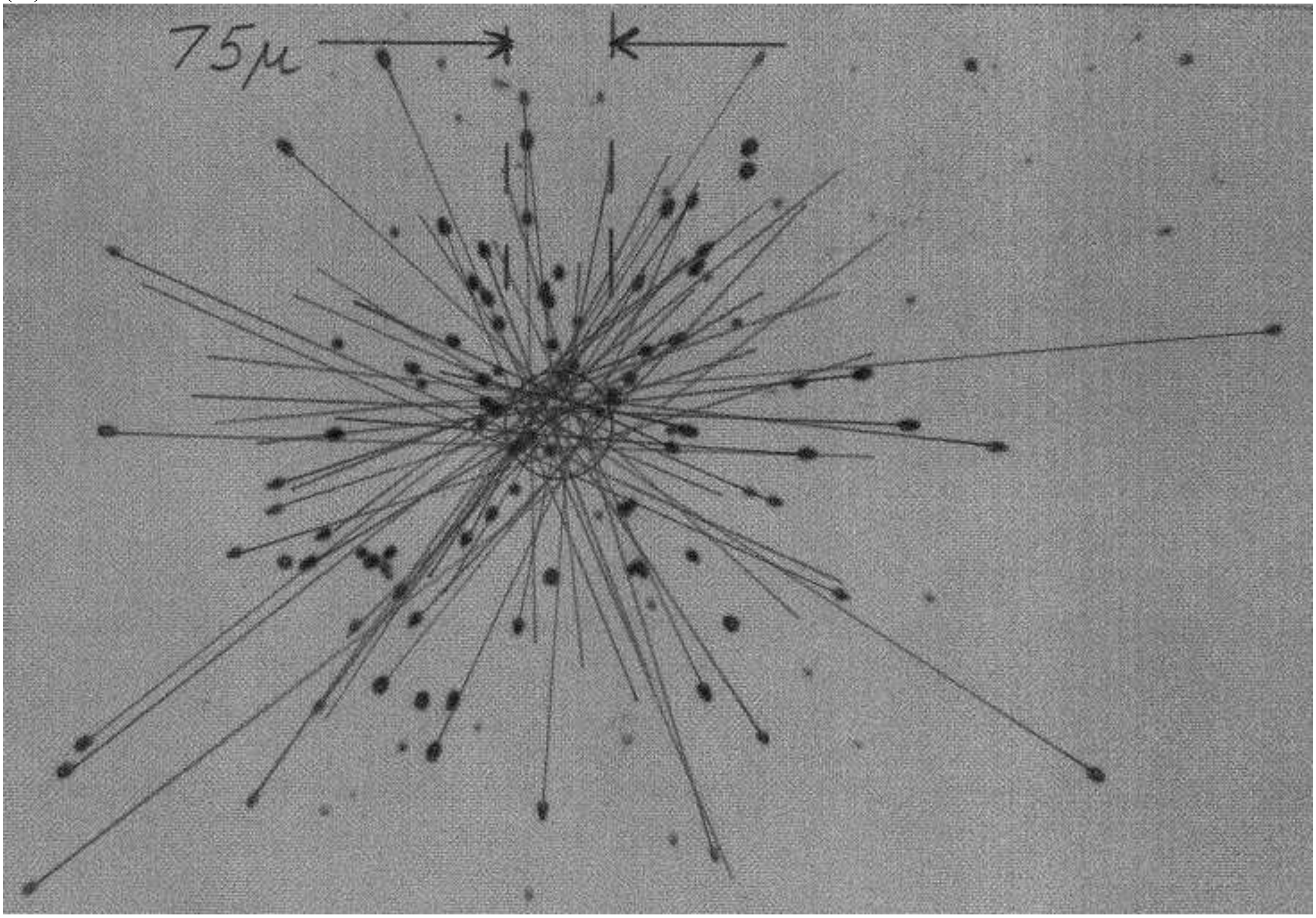

Figure 18
This picture shows tracks already seen in Figure 5 B. The photographed area is 1000 by 1300 microns; magnification was X40.

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21) One additional feature should be noted on Figures 5B and 1. Tracks near the centers of clusters are frequently round while tracks near peripheries of the cluster are mostly elliptical. In fact, the average ellipticity seems to increase with the distance from the central region. It is certainly too early to speculate about significance of such patterns. Also significant are locations of my clusters. Some of them, as in Figure 5A, were found on the upper CR-39 surface (facing the mylar foil and the electrolyte) while others, as in Fig 5B, were found on the lower surface (facing air below the cell). The cluster shown in Figure 1 was not produced on the CR-39 surface facing the O-ring; it was produced on the surface separated from the O-ring by about 1 mm of CR-39 plastic. The same was reported about the cluster shown in Figure 17. It is tempting to speculate that observed tracks result from collisions between protons in CR-39 ($C_{12}H_{18}O_7$) and some kind of neutral particles. Neutral particles do not ionize matter directly; that is why they can travel long distances before colliding with protons. I am not going to elaborate on this because my goal, at this stage, is to convince myself (and others) that Oriani cluster effect is real. In my opinion, theoretical considerations should wait till experiments become reproducible. It is remarkable that clusters of charged particles, reported in 2004 (3) have been observed in recent experiments of Richard Oriani, Marissa Little, John Fisher and myself. Something interesting is going on and research in this area should continue.

22) Is it possible that observed clusters are due to well known processes? One trivial explanation has already been mentioned, it is possibly the presence of tiny grains of alpha-radioactive substances, as in Figure 3B. Another has to do

with cosmic rays. Let me address them, one after another, in the context of clusters from Figure 1 and 5 B. A grain of alpha radioactive substance, such as uranium, would produce tracks whose orientations are random, except when sizes of clusters are not much larger than ranges of alpha particles. Orientations of tracks in Figure 5B (and also in Figure 1) are certainly not random. That is a sufficient argument for rejecting the idea of contamination, as far as I am concerned. Another strong argument against contamination is total absence of clusters on numerous control chips (see the Appendix).

23) Can clusters be due to cosmic rays? Collisions in which relativistic heavy ions disintegrate, producing showers of nucleons, have been studied by using high energy accelerators, for example, at Brookhaven National Laboratory. Suppose that most of the tracks identified in Figure 5 B are due to neutrons and protons resulting from total disintegration of a single ultra-relativistic ion, somewhere near the detector. What is the probability that such ion can be found at sea level? Fortunately, this question has already been answered. According to (11), the theoretically calculated flux, for ions with Z=25, at see level, is 10^{-34} particles per m^2 per second. This translates to about $3*10^{-21}$ particles per km^2 per year. For particles with much higher Z the expected flux is many orders of magnitude smaller.

24) The second cosmic ray phenomenon to consider are showers. Primary cosmic ray charged particles, mostly protons, interact with the atmosphere by producing showers of pions and muons. Can a cluster seen in Figure 5B be due to such particles? The answer is a definite no. The ionization density of pions and muons is too low to produce tracks in CR-39 detectors. I am familiar with one study (12), conducted at Brokhaven National Laboratory, in which CR-39 chips were coated with fissionable material and exposed to beams of pions with several energies between 500 MeV and 2300 MeV. The purpose was to measure cross sections of fission induced by pions. This was done by counting tracks due to fission fragments. The reported cross sections indicate that billions of pions traverse CR-39 chips for each set of fission fragments. Observation of fission fragment tracks would not be possible if individual pions could form tracks in CR-39 detectors. Cosmic ray showers, by the way, are known to be very wide, near sea level. On that basis I am inclined to conclude that small clusters observed in my experiments were not due to cosmic rays.

10) Appendix:

26) A friend who read the above asked for more information about control experiments. Here is a brief summary of facts:

a) Six electrolysis experiments were conducted but only two (I and III) produced clusters. In other words, two successes (seven clusters and one rosette) and four failures -- to replicate clusters during electrolysis.

(b) Four residual activity experiments were conducted but clusters were observed in experiment VIII only. In other words, one success (three clusters) and three failures.

(c) Not a single cluster was found on control chips. During the experiments, control chips were exposed to air (in the same room but away from the electrolytic cell and from the seeded O-rings). Two CR-39 control chips, A and B, were used in parallel with Experiment I. Chip A was covered by the original blue protective coating while the coating was removed from chip B. Chip B was exposed to air while chip A was squeezed between two O-rings and two flat plates. Four screws were used to apply pressure to the O-Orings. That pressure was higher than in the electrolytic cell. The mean track density on Chip A was ~3 tr/cm^2 ; the mean track density on chip B was ~8 tr/cm^2 . The conclusion was that mechanical pressure, applied to a CR-39 chip through O-rings, does not produce excessive track-looking defects. In a private message Oriani wrote that the mean background on his control chips was 16.6 tr/cm^2 , with, standard deviation of 9.7 tr/cm^2. My control-experiment results also fluctuated widely around ~15 tr/cm^2 . Note that even a high, 25 tr/cm^2 , translates into 0.35 tracks per my 1.3 by 1.0 mm field. Fortunately, fluctuations of background densities are inconsequential; local densities within clusters turned out to be at least 100 times higher.

(d) Absence of clusters on the chip used in the electrolysis experiment II is convincing evidence that neither electrolyte nor the O-rings were contaminated with grains of an alpha-radioactive material, such as ^{212}Pb, ^{226}Ra or ^{238}U. I am referring to the suspicion that the electrolyte, or the O-rings, might have been contaminated with such material during Experiment I. If this were true then Experiment II would also produce clusters. The electrolyte and the O-rings, used in Experiment II (21 days of electrolysis) were the same as in Experiment I (5 days of electrolysis). The number of

clusters produced in Experiment II would exceed the number produced in Experiment I. Zero clusters in experiment II versus four clusters in experiment I is not consistent with the idea of contamination. But it is consistent with the idea of irreproducibility.

(e) John Fisher suggested that additional CR-39 chips should be placed in air, very close to the cell. That was on the day 3 of Experiment II. I did this. Four small (about 1 by 2 cm) chips were placed next to the cell for the remaining 18 days of electrolysis. No clusters were recorded on these chips. This is an acceptable argument against absence of tiny alpha-radioactive aerosoles in air.

(f) Absence of clusters on chips used in residual activity experiments VII, IX and X is also acceptable evidence against the suspicion that seeded O-rings were contaminated with tiny grains of an alpha-radioactive material.

(g) The overall conclusion is that something interesting is going on, and that additional investigations are needed. Absence of reproducibility is the most disturbing thing in this episode. But this should not prevent us from further investigations. A clear yes-or-no answer, about nuclear effect due to electrolysis, will be found, sooner or later. Absence of reproducibility does not mean that a phenomenon is unreal; it means that we must learn how to control it.

11) References:

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2) F.W. Keeney at al., Proceedings of ICCF10, page 509 (2003).
3) R. A. Oriani and J. C. Fisher: “Detection of energetic charged particles during electrolysis;” Proceedings of ICCF10, 2003, pp 577-584. Also see Jpn. J. Appl. Phys. 41,6180, 2002 and two papers in the proceedings of ICCf11, 2004, pp 281-303)
4) P.A. Mossier-Boss et al., “Production of high energy particles using the Pd/D co-deposition process.” Paper presented at the APS meeting, March 5, 2007, Denver Colorado. Also see Naturwissenschaften (2007) DOI 10.1007/g00114-007-0221-7.
5) Oriani; draft of an unpublished manuscript, August 2007. That draft can be seen at: http://csam.montclair.edu/~kowalski/cf/333oriani2007.html
6) S. Krivit in the November 2006 issue of New Energy Times (items 7 and 8) at http://newenergytimes.com/2006/NET19.htm
7) Radon Division of Landauer Inc. (http://www.landauerinc.com/contact/)
8) D. Nikezic and K.N. Yub, “Formation and growth of tracks in nuclear track materials” in Material Scince and Engineering, R46 (2004) p 51 to 123.
9) Marissa and Scott Little -- See their website http://www.earthtech.org/PACA/logbook.html 10) R.P. Henke and E.V. Benton. “Charged particles tracks in polymers,” Naval Radiological Defence Lab; Technical report AD06611796, Octiber 3, 1967.
11) P.B. Price; “Do energetic heavy nuclei penetrate deeply into Earth’s atnosphere?”;Proc. Natl. Acad. Sci. USA, vol77, No 1, pp 44-48, January 1980
12) H.M. Khan et al., Nuclear Physics A vol 781, Issue 3-4, 15 January 207, pp 296-305.

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336) On emission of nuclear particles caused by electrolysis

Ludwik Kowalski; 11/3/2007

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Montclair State University, Montclair, NJ, USA

Abstract:
Numerous tracks of charged nuclear particles, emitted during electrolysis, were discovered by Oriani and Fisher (1). More recently, emission of such particles after electrolysis was discovered by Oriani (2). This presentation is based on ten experiments conducted to replicate the reported results. Seven clusters of tracks were found in two out of six electrolysis experiments. Three clusters were also found in one of four experiments conducted to study emission of nuclear particles after electrolysis. Arguments are presented against trivial explanation of clusters, such as natural radioactivity and cosmic rays.

1) Introduction

The field of so-called “cold fusion,” also known as CANR (chemically assisted nuclear reactions), belongs to the realm of protoscience. The accepted paradigm is that chemical processes -- interactions involving outer electrons in atoms and molecules -- are too weak to produce emission of nucleons from atomic nuclei. Yet, several qualified researchers, such as A. Lipson (3), R. Oriani (1) and S. Jones (4), have for many years been reporting unexpected emission of nuclear particles. Experimental facts that conflict with existing theories should be studied rather than rejected. A flowchart illustrating evolution of protoscience toward science is shown in Figure 1.

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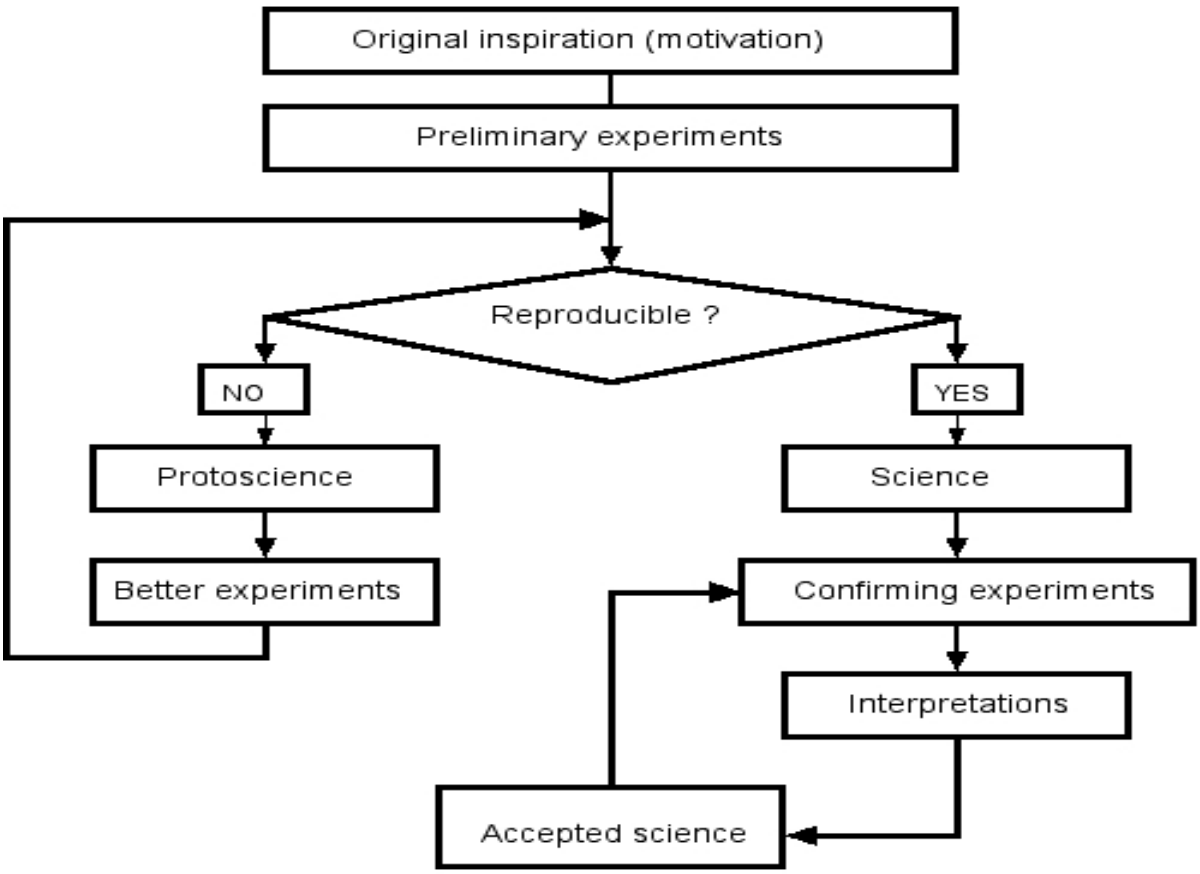

(A)

Figure 1
Schematic illustration of evolution of protoscience toward accepted science.
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This study, prompted by recent reports (2,5), confirms that an unexpected nuclear process seems to be occasionally triggered by a chemical process. According to Oriani (2), whose protocol was used in my investigation, nuclear particles are emitted during and after electrolysis. The high level of reproducibility, according to him, is due to residual activity. Each consecutive electrolysis experiment presumably benefits from what happens during the previous experiment. The probability of triggering a CANR effect in a new electrolytic cell is said to be very low, but it becomes very high after the first success. The probability is also said to be high in a new cell containing o-rings used in successful experiments. That is why my experiments were performed with o-rings previously used by Oriani.

According to (2), distributions of tracks left by nuclear particles on CR-39 detectors are randomly uniform. But sometimes tracks appear in the form of clusters, as illustrated in Figure 2.

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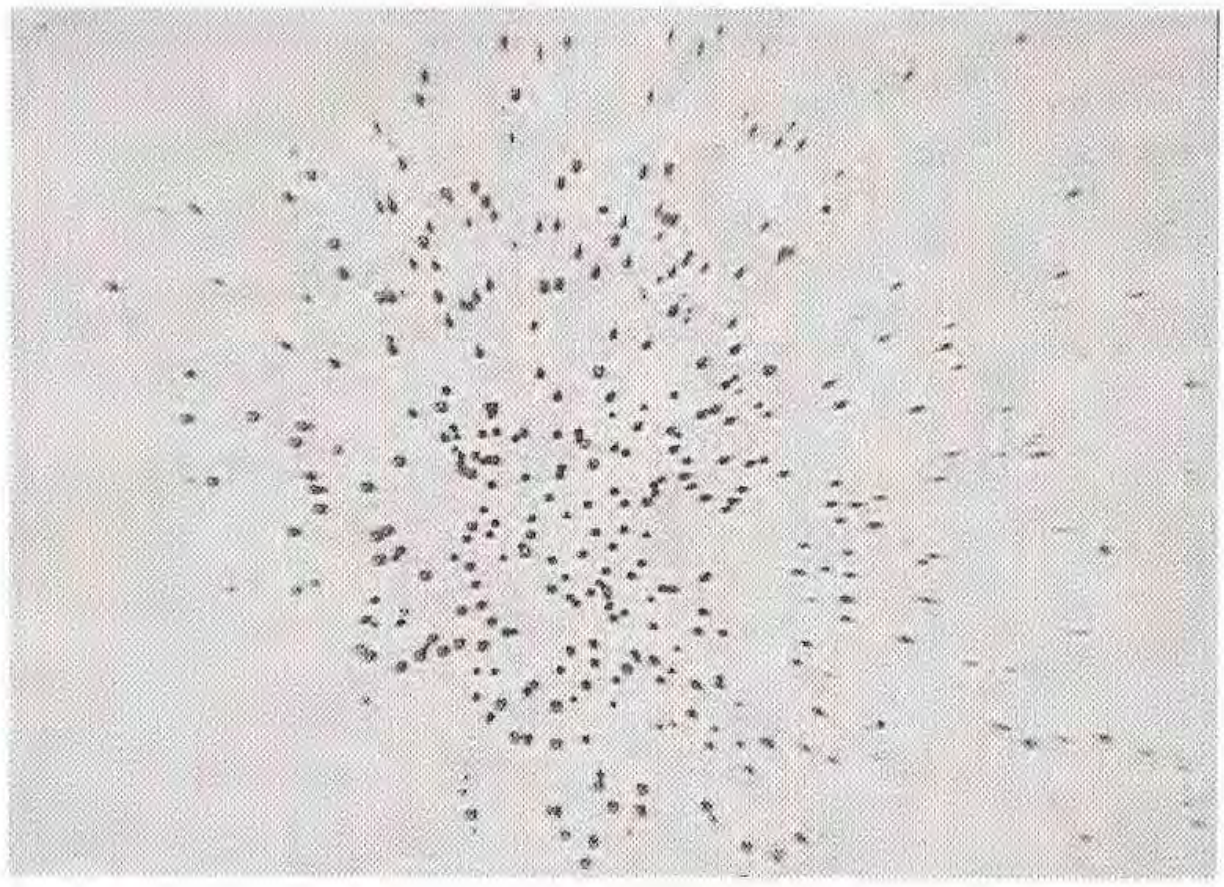

(A)

Figure 2
A cluster of post-electrolysis tracks discovered by Oriani (8). This cluster was produced when the CR-39 chip was exposed to an o-ring removed from the electrolytic cell. The mean density, about 30000 tr/cm^2, is 2000 times higher than outside the cluster.
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I was able to confirm occasional production of clusters at local densities much higher than the background. The densities of uniformly distributed tracks, however, were found to be consistent with the background. Can radioactivity acquired by these rings in Oriani's cell be responsible for clusters produced in my experiments? My answer to this question will be negative. But the issue is far from being resolved, as indicated in the appendix. What follows describes my simple apparatus and results. In the closing section I will argue against the idea that clustered tracks are due to natural radioactivity or cosmic rays.

2) Experimental setup.
The electrolytic cell used in my experiments was identical to one of Oriani's cells, as schematically illustrated in Figure 3A. The electrolyte was Li_2SO_4 in ordinary distilled water at a concentration of 22 grams per liter. The anode was Pt and the cathode was Ni. The cell consisted of a set of a glass tubes containing about 10 cc of the electrolyte. The bottom of the cell was made from a replaceable 3 by 3 cm piece of CR-39 plastic material. That piece (thickness about 1 mm) was separated from the electrolyte by a layer of mylar whose thickness was 6 microns. The Ni-wire cathode, in the form of a spiral pancake, was in contact with the mylar while the Pt-wire anode, also in the form of a spiral pancake, was about 15 mm above the cathode. The cell was designed in anticipation of experiments in which CR-39 would be removed and a silicon detector, placed below the mylar window, would be used to measure energies of charged nuclear particles.

Potential differences applied to the cell were between about 5 and 12 volts, depending on the desired current (between 30 mA and 150 mA during preliminary experiments). At 3 volts the current was close to zero. The concentration of the electrolyte was allowed to double (approximately) during experiments because water lost via electrolysis was usually not replaced until the volume of the electrolyte was reduced to approximately one half of the initial volume. The current, however, was kept constant, usually 42 mA, by an electronic stabilizer outside the power supply.

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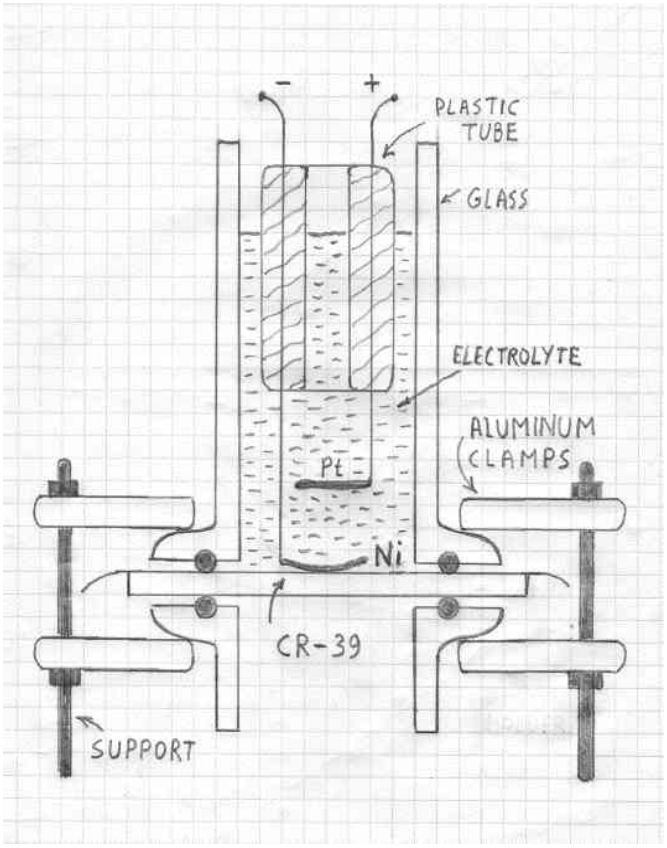

B ->

Figure 3
(A) Two flexible Viton o-rings are pressed against the CR-39 chip (by clamps applied to glass tubes) to prevent leakage of the electrolyte. The inner diameter of each tube was close to 15 mm; the wire diameters (Pt and Ni) were 0.5 mm. The Ni cathode was a “foot” supporting the rigid anode-cathode structure. (B) A CR-39 chip on top of an o-ring, as used to detect nuclear particles emitted after electrolysis.
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The CR-39 chips were cut from a Fukuvi Chemical sheet purchased from Landauer Inc. (6). One of the well known properties of CR-39 material is its ability to record nuclear projectiles, such as protons or alpha particles (7). Tracks of such particles become microscopically visible after etching. Sizes of observed tracks increase with time and temperature of etching. A cluster consisting of over 100 tracks is shown in Figure 4. That figure is a microphotograph of a small area of CR-39 that was removed from the electrolytic cell after five days of electrolysis and etched for six hours. Round pits are tracks of particles that were intercepted at small angles of incidence; elliptical and conical pits are tracks of particles intercepted at larger angles. After twelve hours of etching nearly all tracks become round, as illustrated in Figure 5. Nuclear activity after electrolysis was studied by placing CR-39 chips on the o-rings removed from the cell, as illustrated in Figure 3B.

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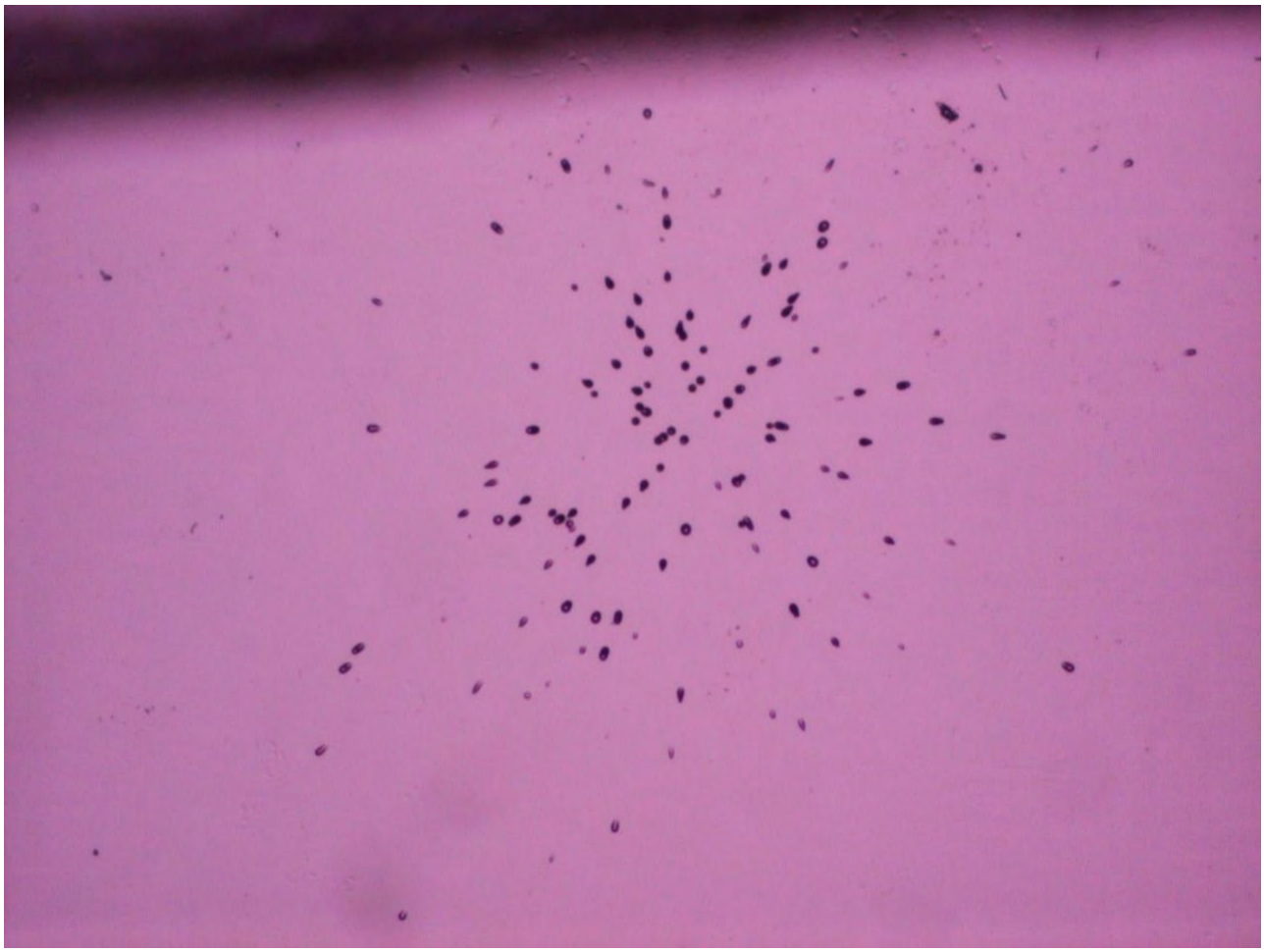
(A) 

Figure 4
Cluster of tracks produced during five hours of electrolysis. The area of 1000 by 1300 microns was photographed under a magnification of X40, after six hours of etching. The mean track density, over the area of 1 mm^2, is about 11000 tracks per cm^2. This is three orders of magnitude higher than in the areas outside the four clusters, on the same CR-39 chip. A section of this photo, under a higher magnification, is shown in Figure 6.
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All CR-39 detectors were etched for six hours in a small beaker. The average temperature was close to 72 C. But local temperatures, near individual detectors, were most probably not identical (because the etching solution was not stirred). Sizes of tracks are known to depend on the product to the etching time and temperature. It is reasonable to assume that observed difference in sizes and shapes of tracks, in different clusters, were due to differences in local temperatures. The effect of etching time on the tracks appearance can be seen by comparing Figure 4 and Figure 5.

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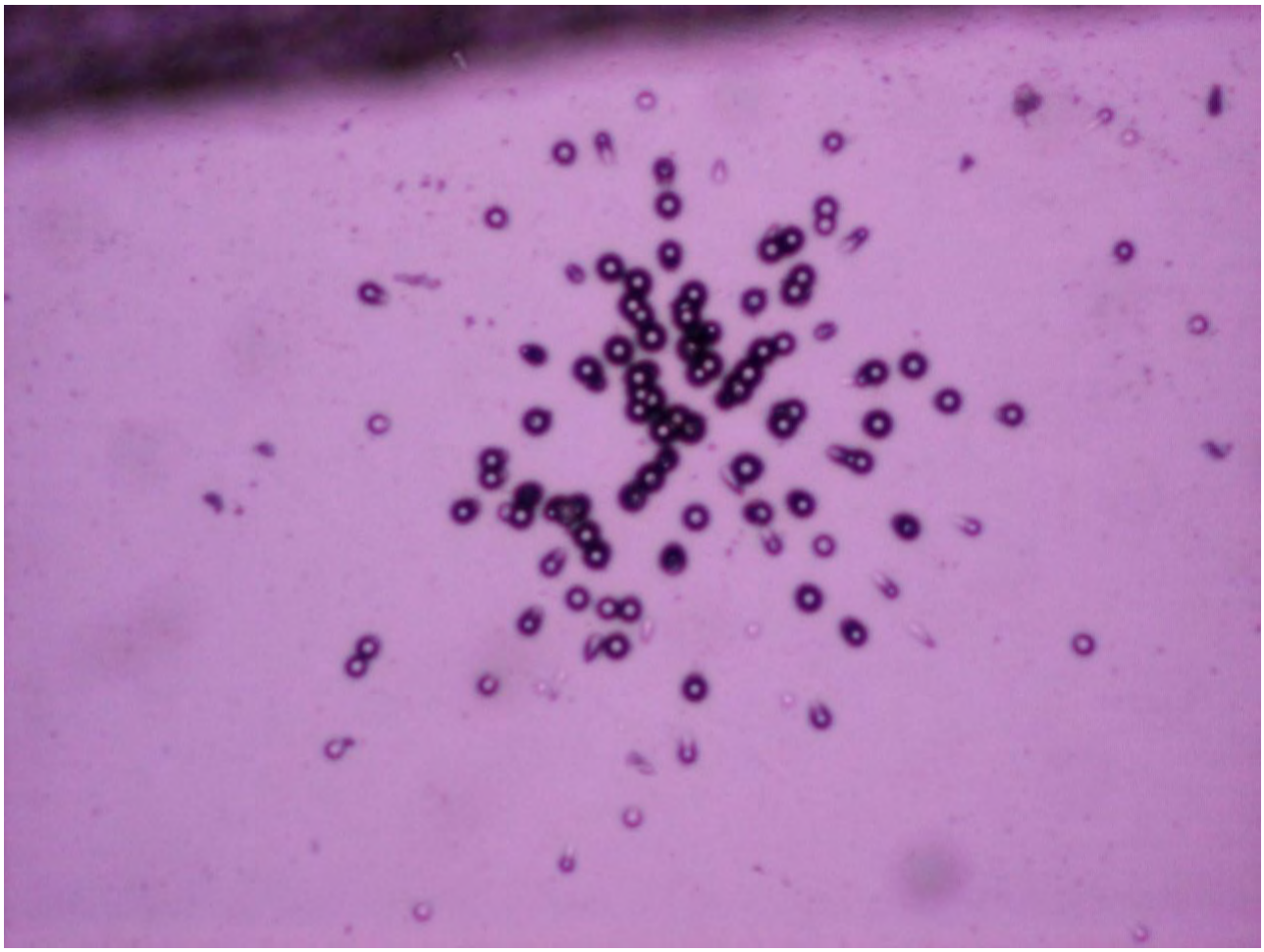
(A) 

Figure 5
The microphotograph of the same cluster as in Figure 4 but after six additional hours of etching. As before, an area of 1000 by 1300 microns was photographed under a magnification of X40.
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Control experiments with CR-39 chips in air showed that background track densities, after five days of exposure, fluctuated widely. The mean density was 15.5 tracks per square centimeter; the standard deviation was 9.7 tr/cm^2. Similar densities were found on CR-39 chips exposed to the electrolyte, and to other unused components of the cell (glass, o-rings, platinum and nickel). Note that even 25 tr/cm^2 density translates into 0.35 tracks per my low magnification field of 1.3 by 1.0 mm. In other words, fluctuations of background densities are inconsequential; local densities within clusters are usually much higher.

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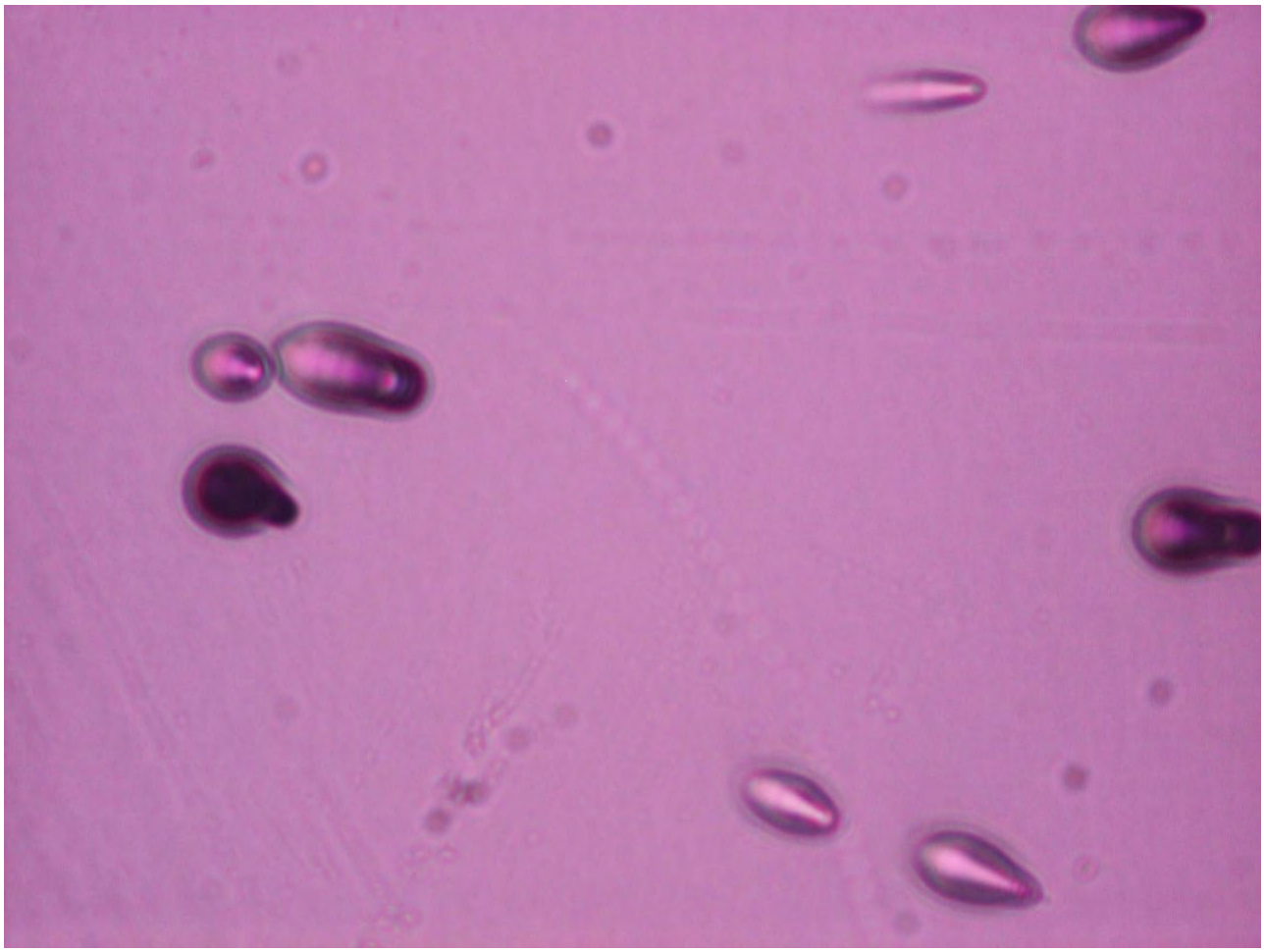
(A) 

Figure 6
Elliptical and conical tracks in a small section of the area shown in Figure 4. The magnification was X400; the total photographed area was 130 by 100 microns.
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3) Experimental results
Six experiments were performed to observe Oriani's electrolysis effect. But only two of them were successful in terms of finding clusters of tracks. Two pictures of one post-electrolysis cluster are shown in Figures 4 and 5. Observed clusters are described in Table 1.

Table 1
Experiments to study nuclear particles emitted during electrolysis:

Electrolysis in Experiment I lasted 5 days, using Oriani's seeded o-rings. The rings were received 24 hours after they were removed from Oriani's cell. The experiment started shortly after the package arrived. Four clusters were produced. Numbers of tracks in these clusters were: 109, 75, 30, and 29. The corresponding mean track densities were: 11000, 5000, 3000, and 4800, respectively, per square centimeter.

Electrolysis in Experiment II lasted 21 days, using the same o-rings as in Experiment I. The experiment started shortly after the end of Experiment I. Zero clusters were produced.

Electrolysis in Experiment III lasted 5 days, using the same o-rings as in Experiment II. The experiment started shortly after the end of Experiment II. Three clusters were produced. Numbers of tracks in these clusters were: 85, 35, and 9. The corresponding mean track densities were 11000, 4400, and 900, respectively, per square centimeter.

Electrolysis in Experiment IV lasted 5 days, using another freshly seeded o-ring from Oriani. The experiment started shortly after that O-ring was received. Zero clusters were produced.

Electrolysis in Experiment V lasted 5 days, using the same o-ring as in Experiment IV. The experiment started shortly after Experiment IV. Zero clusters were produced.

Electrolysis in Experiment VI lasted 5 days, using the same o-ring as in Experiment V. The experiment started shortly after the end of Experiment V. Zero clusters were produced.

Experiments to study nuclear particles emitted after electrolysis:.

Experiment VII was conducted outside the electrolytic cell. Several small CR-39 chips were exposed to the o-ring used in Experiment III. The exposure time was two days; Zero clusters were produced.

Experiment VIII was also conducted outside the electrolytic cell. Two large CR-39 chips were applied to another freshly seeded o-ring sent to me by Oriani. Three clusters were produced during two days of exposure. Numbers of tracks in these clusters were: 110, 60 and 13 The corresponding mean track densities were 360, 300, and 2600, respectively, per square centimeter.

Experiment IX was similar to Experiment VIII, except that it started at the end of Experiment VIII. Zero clusters were produced during two days of exposure.

Experiment X was similar to Experiment IX, except that it started at the end of Experiment IX. Zero clusters were produced during two days of exposure.

But one cluster from Experiment III, shown in Figure 7, was very different. Similar patterns of nearby tracks, named rosettes, were reported by Oriani. He considers them to be part of background rather than clusters due to electrolysis. It is reasonable to assume that tracks in very small clusters are due to alpha particles emitted from a tiny grain of radioactive contamination, such as radium or uranium. This supposition is based on the fact that distances between radially oriented tracks are smaller than ranges of alpha particles in CR-39 or in the electrolyte (typically less than 50 microns). Peripheral tracks in Figure 4, on the other hand, are often separated by distances much larger than tracks of alpha particles. Significance of this observation will be discussed in the next section. It is remarkable that not a single cluster was found on more than twenty chips from control experiments. This is a strong indication that larger clusters are not due to background tracks.

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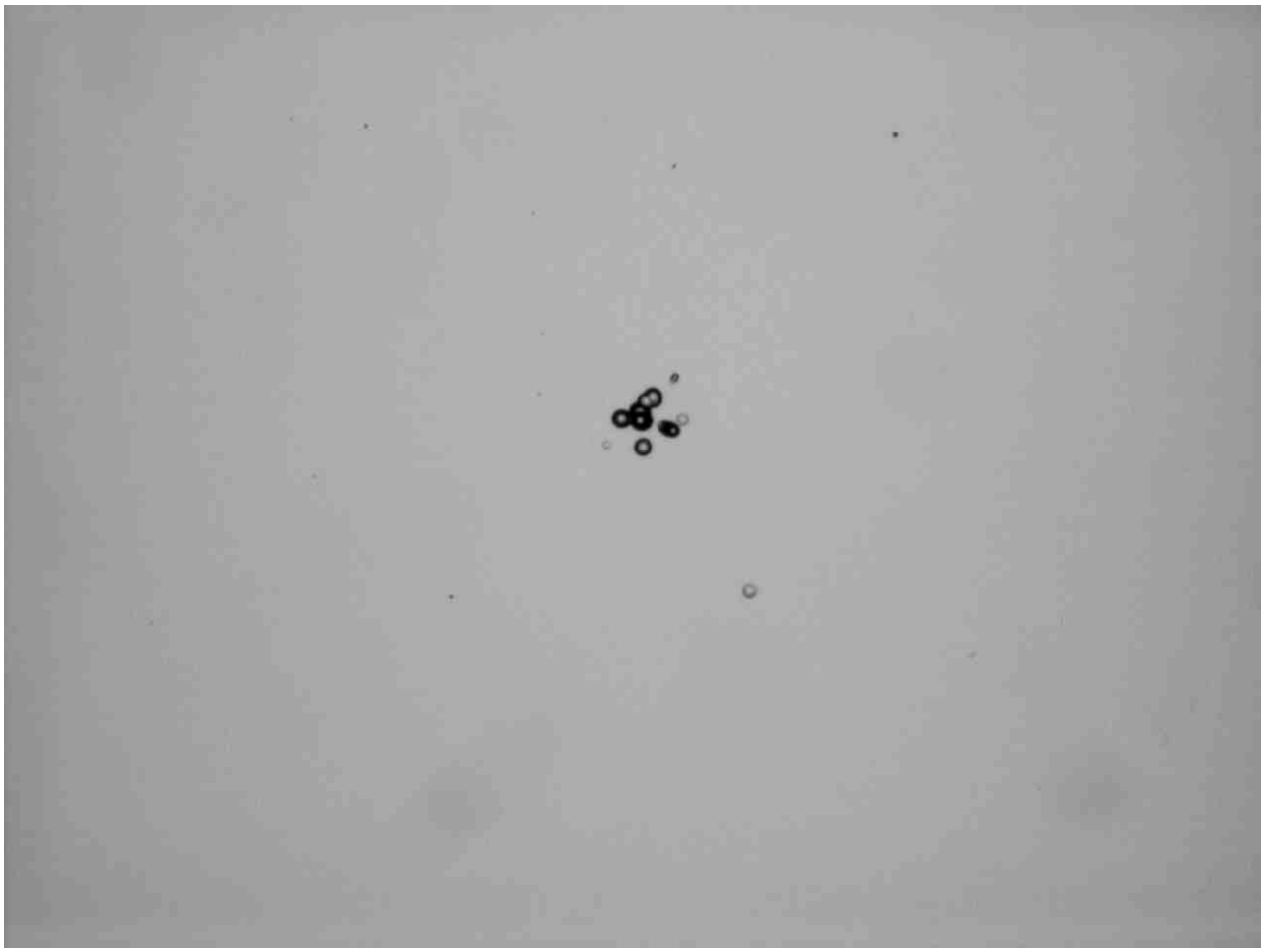
(A) 

Figure 7
The microphotograph of a very small cluster of tracks produced in Experiment III. The photographed area was 1000 by 1300 microns; the magnification was X40.
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4) Discussion
It is clear that random fluctuations could not be responsible for zero clusters in the Experiment II (electrolysis lasting 21 days) and for four clusters in the Experiment I (electrolysis lasting five days). The same is true for clusters recorded after electrolysis. In both situations formation of clusters seems to be due to some uncontrollable factors. That is why further investigations are needed. In this section I want to show that neither contamination nor cosmic rays can be responsible for clusters shown in in Figures 1 and 4. The main argument is trivial -- ten clusters were produced on experimental chips and zero clusters were produced on all control chips.

Another argument against contamination is based on orientations of noncircular tracks. Orientations of these tracks, separated from each other by distances larger than ranges of alpha particles (in CR-39 and in water), would be random if clusters were due, for example, to large uranium particles. But orientations are certainly not random. Tracks near the centers of clusters are frequently round while tracks near peripheries are mostly elliptical. In fact, the average ellipticity seems to increase with the distance from the central region.

It is tempting to speculate that observed tracks result from collisions between protons in CR-39 ($C_{12}H_{18}O_7$) and some kind of neutral particles. Neutral particles do not ionize matter directly; that is why they can travel long distances before colliding with protons. I am not going to elaborate on this because my goal, at this stage, is to convince myself (and others) that Oriani-type clusters are due to electrolysis. In that context it is important to mention that clusters were also reported by John Fisher (9) and by Marissa Little (10). The protocols used by these researchers were essentially the same as that used by Oriani. Clusters produced during electrolysis were also observed in SPAWAR experiments

with sequential etching (11). Sequential etching was probably used to etch away myriads of chemically caused pits known to be present on the surface. Clusters with track deep inside the CR-39 material, if confirmed, would be highly significant. Note that SPAWAR experiments (5) were totally independent of Oriani's experiments, and of his protocol. Also note that in all cases local track densities, within clusters, were much higher than in the background.

Is it possible that observed clusters are due to cosmic rays? Collisions in which relativistic heavy ions disintegrate, producing showers of nucleons, have been studied using high energy accelerators, for example, at Brookhaven National Laboratory. Suppose that most of the tracks identified in Figure 4 are due to neutrons and protons resulting from the disintegration of a single ultra-relativistic ion, somewhere near the detector. What is the probability that such an ion can be found at sea level? Fortunately, this question has already been answered. According to (12), the theoretically calculated flux, for ions with Z=25, at sea level, is 10^{-34} particles per m^2 per second. This amounts to about $3*10^{-21}$ particles per km^2 per year. For particles with much higher Z the expected flux is many orders of magnitude smaller.

The second cosmic ray phenomenon to consider is showers. Primary cosmic ray charged particles, mostly protons, interact with the atmosphere by producing showers of pions and muons. Can a cluster seen in Figure 4 be due to such particles? The answer is a definite no. The ionization density of pions and muons is too low to produce tracks in CR-39 detectors. In one study (13), CR-39 chips were coated with fissionable material and exposed to beams of pions with several energies between 500 MeV and 2300 MeV. The purpose was to measure cross sections of fission induced by pions. This was done by counting tracks due to fission fragments. The reported cross sections indicate that billions of pions traverse CR-39 chips for each set of fission fragments. Observation of fission-fragment tracks would not be possible if individual pions could form tracks in CR-39 detectors.

On that basis I am inclined to conclude that clusters observed in my experiments were not due to cosmic rays, or to alpha-radioactive substances. Something interesting is going on and research in this controversial area should continue. The goal should be to identify hidden factors responsible for irreproducibility of experimental results. Unfortunately, present investigations of clusters belong to protoscience, as defined in Figure 1. Protoscience should not be confused with pseudoscience, such as astrology. Methods of validation in protoscience are not different from methods used in science.

Appendix (11/9/07):
Two important objections were made to my tentative conclusion that clusters could not be attributed to contamination with alpha-radioactive substances. The first objection was made by Lipson and Rousetski, during a recent conference in Italy (14). Discussing my presentation, they invented the following scenario. A gas bubble sits on top of CR-39. A grain of alpha-radioactive material sits on top of the bubble. Ranges of alpha particles in the gas bubble are much larger than in the electrolyte. That is why a cluster can be much larger than ranges of alpha particles in the electrolyte. Furthermore, according to this assumption, most tracks would be circular near the central region and noncircular along cluster's peripheries. Tracks along peripheries would be radially-oriented, more or less, as in our experimentally-observed clusters. Commenting on the above S. Little wrote (15) " . . . The electrolyte does not wet the CR-39. Grains of radioactive material could easily be trapped between the mylar film and the CR-39, . . . " In other words, not only bubbles can produce clusters with radially oriented tracks. I agree with these speculations.

APPENDED ON JULY 1, 2009
What I am doing now amounts to a replication of experiments described in this unit. Motivation for doing this is described in Unit 368; results will be reported in Unit 373, probably in September. The only difference is minimizing contact of chips with air. Will this eliminate "widle fluctutions" of mean densities on control chips? Will we see excessive tracks outside clusters? Will we see clusters? This remains to be seen.

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2) Richard Oriani; submitted to Phys. Rev. C, October, 2007. Or draft of an unpublished manuscript, August 2007.
That draft can be seen at:

http://csam.montclair.edu/~kowalski/cf/333oriani2007.html
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5) P.A. Mossier-Boss et al., "Production of high energy particles using the
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6) Radon Division of Landauer Inc. (http://www.landauerinc.com/contact/)
7) D. Nikezic and K.N. Yub, "Formation and growth of tracks in nuclear track
materials" in Material Scince and Engineering, R46 (2004) p 51 to 123.
8) Personal information from R. Oriani; October 2007
9) Personal information from J. Fisher, cooperating with Oriani; October 2007
10) Personal information from M. Little, cooperating with Oriani; October 2007
11) Personal information from P. Boss, November 2007.
12) P.B. Price; "Do energetic heavy nuclei penetrate deeply into Earth's
atmosphere?" Proc. Natl. Acad. Sci. USA, vol77, No 1, pp 44-48, January 1980
13) H.M. Khan et al., Nuclear Physics A vol 781, Issue 3-4, 15 January 207, pp 296-305.
14) The 8th International Workshop on Anomalies in Hydrogen- and Deuterium-Loaded Metals in Catania, Italy
(October 2007).
15) Scott Little, Earthtech International, Inc. (in a message posted on the private Internet list for CMNS researchers,
11/8/07).

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337) On emission of nuclear radiation due to glow discharge

Ludwik Kowalski; 11/17/2007

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In 2002 my renewed interest in cold fusion was triggered by research of Karabut (unit #13 at my website). That was five years ago. I was impressed by the description of spectacular nuclear effects due to the glow discharge. During a workshop in Italy (Catania, October 2007) I was equally impressed by the ongoing research of Ed Storms and Brian Scanlan. Their glow discharge apparatus is also said to produce nuclear effect -- emission of high energy electrons (0.8 MeV) and of yet to be identified heavier projectiles, perhaps protons, neutrons or alpha particles. This unit is devoted to what I learned about this recently-started investigation, and to my understanding of it. The report of S & S can be downloaded from the LENR-CANR library (1).

The glow discharge apparatus is essentially a diode filled with a gas at low pressure. The gas starts glowing when sufficiently high voltage is applied. The distance between the anode and the cathode, in the S&S setup, was about 8 mm. Their gas was D2, mixed with small amounts of oxyge. The pressure was probably close to 10 Torr. Essential components of the apparatus are identified in Figure 1. The figure is not geometrically realistic; it was made to match my simplified description.

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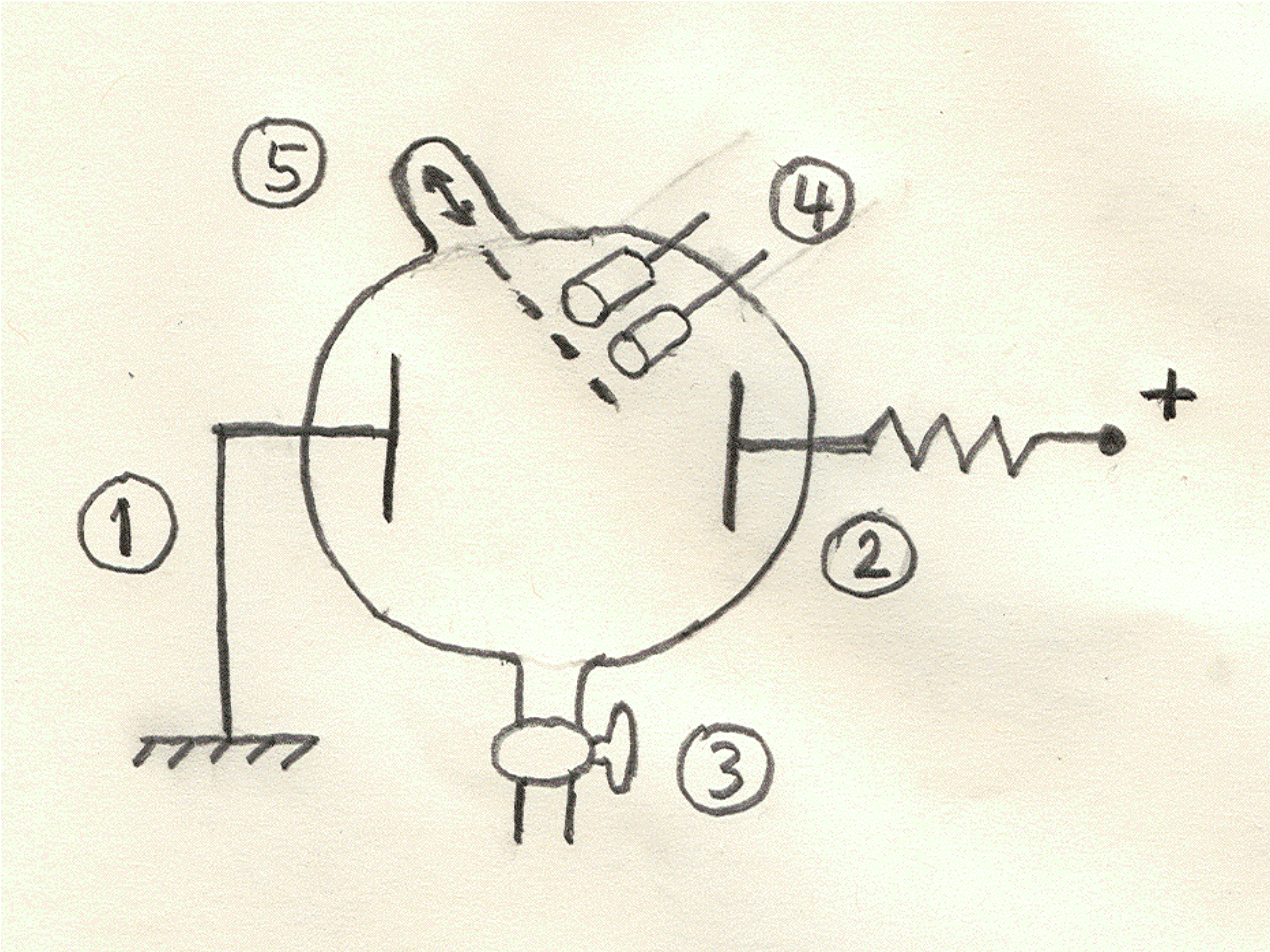

Figure 1
A schematic diagram of the apparatus used by Storms and Scanlan.

1 Grounded water-cooled cathode
2 Anode connected to the power supply via a 300 ohms resistor.
3 Valve leading to the vacuum and gas supply tubes.
4 Two detectors of nuclear radiation (GM and SBD) facing the cathode.
5 A filter of desired thickness can be inserted, as needed.
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According to preliminary investigation, based on the Geiger Muller counter (with and without filters), nuclear radiation is emitted when appropriate voltage is applied to sustain the glow discharge. My first reaction to this claim, after hearing Storms' presentation in Catania, was that the GM detector is probably picking up radio waves generated during the discharge. Three reasonable arguments were presented against this suspicion:

a) Placing a thin filter, of sufficient thickness (either metal or dielectric), between the cathode and the detector, reduces the counting rate to the background level.
b) The effect increases at a rate that is proportional to the D/O ratio.
c) The effect tends to disappear when hydrocarbons are added.

2) In a brief discussion I suggested an additional test, to rule out the possibility that high counting rates (exceeding million counts per second) are due to the radio-waves artifact. It consists of studying the effect of the voltage, applied to the GM tube, on the counting rate.

a) Use any radioactive source to establish the minimum voltage below which counting becomes impossible. Suppose the GM plateau region is between 700 and 900 volts and that nothing is counted below 550 volts.
b) Conduct an experiment at 800 volts to observe the claimed effect.

c) Lower the GM voltage to to 500 volts and repeat the experiment.

My level of confidence in the claimed effect would increase dramatically if the high counting rate remained essentially constant in the plateau region and dropped to zero at 500 volts. The radio waves idea (or propagation of disturbabces along wires), on the other hand, would be confirmed if the GM was still counting at 500 volts. Rick Cantwell, who conducts glow discharge investigations in Colorado, also made a presentation in Catania. My understanding was that he will try to confirm the S&S results. I hope that he and the S&S team will soon conduct the suggested test and post the results on the private Internet list for CMNS researchers. On my agenda that would be the test #1. Nothing would be investigated with a GM detector responding to electromagnetic disturbances associated with the discharge.

3) According to S&S report, two kinds of radiation are emitted: monoenergetic electrons (0.8 MeV) and heavier particles. Heavier particles have not yet been identified. Electrons are said to be emitted at the rate of billions per second. How was energy of electrons determined with a GM counter? By the use of filters, as illustrated in Figure 2. In the original report (1) this was Figure 9.
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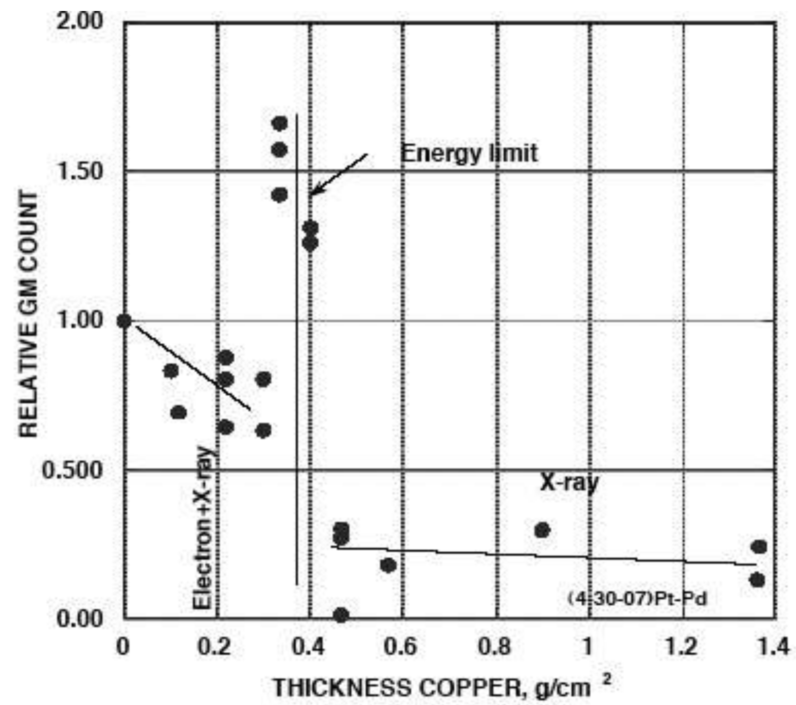

FIGURE 9. Effect of copper absorbers on the amount of radiation reaching the GM tube from a Pd+Pt cathode.

Figure 2
Counting rate verus the copper filter thickness.
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It shows that the counting range drops suddenly when the total thickness of filters becomes 400 mg/cm^2. That corresponds to monoenergetic electrons of 0.8 MeV. If the drop occurred at 500 mg/cm^2 then the energy attributed to electrons would 1.0 MeV, etc., according to a ranfe-energy formula for electrons. But the figure also shows a peak, at the thickness of about 350 mg/cm^2. The report states that the peak is due to bremsstrahlung radiation. Unfortunately, this is not at all convincing. My expectation would be a monotonic increase of the intensity of BR up to the maximum thickness of 400 mg/cm^2. The combined intensity (electrons plus BR) should not produce a very narrow peak at 350 mg/cm^2. I suspect that some kind of artifact is responsible for the shape of the curve in Figure 2. What can it be? To answer to this question I would like to know what absolute counting rates were. The numbers along the vertical axis are relative counts, where 1.0 is assigned to the result without a filter. The absolute values were probably close to 100. How else could wide fluctuations of data points (with respect to straight segments) be explained?

4) Referring to results obtained with the SBD (surface barrier detector), the report states that "charged particles having several energies were detected." Unfortunately, the spectrum with energy peaks is not shown. Monoenergrtic electrons of 0.8 MeV would produce their own peak in that spectrum. I will insert additional information when it becomes available, probably in another formal progress report or via our private Internet discussion list. This is an interesting line of research; I will try to follow its future developments.

Footnote:
I am composing this unit after receiving a message from a stranger. That person asked me why was nothing new posted at my CF website for many months.

Appended on 11/19/07:

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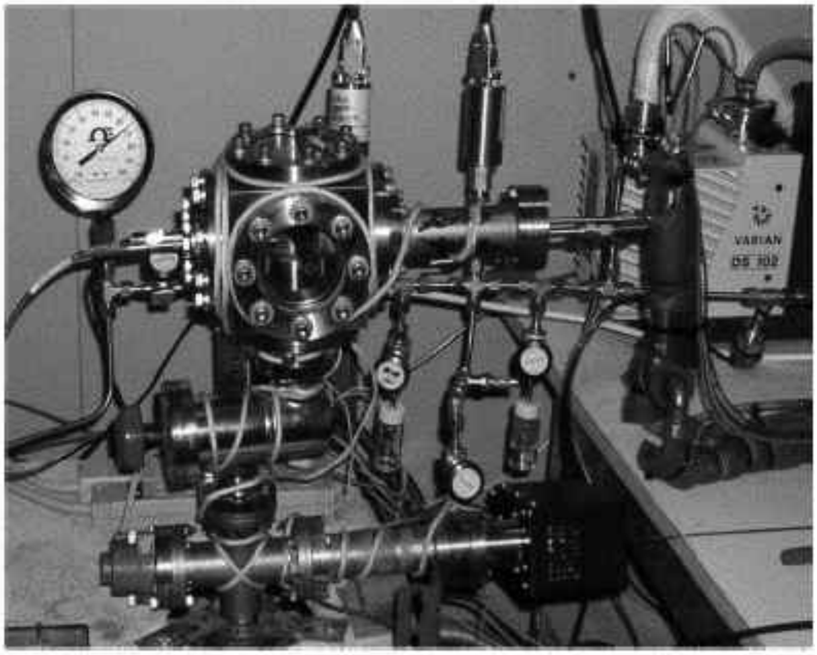

Figure 3
The overall view of the S&S apparatus.
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The overall view of the apparatus in Figure 3 shows its complexity, as far as practical implementation of a simple idea is concerned. The anode (see Fig 1) was made from Pd wire of 2 mm diameter. Cathodes, in the form of removable foils, were made from several materials, such as copper, palladium and silver. The observed results were more or less the same when different cathodes were used.

References:

1) Go to http://www/lenr-canr.org and click on “library.” Files are listed alphabetically (accordig to the first author). Scroll to a file you want and click the “download.”

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338) Quotes from our private list messages about theories

Ludwik Kowalski; 11/25/2007

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Introduction

On 11/2/07, Steve Krivit, the editor of New Energy Times,

http://www.newenergytimes.com/contact/contact.htm

posted the following message on a private list for CMNS researchers:

Fellow researchers,
1. Anyone who knows of any CMNS/LENR theory that is in any way correct should submit such comment to New Energy Times by Dec. 1.
2. I do not care whether the commentary is fully or only partially supportive of the theory; anything positive will do.
3. Positive commentary can be only from nonauthors of the specific theory. No self-promotion!
4. On anyone's request to me within one week, I will seek comment from Richard Garwin about any CMNS/LENR theory and report back with his response in New Energy Times. You must attach the related papers so I can forward them to Garwin.

Interesting comments about theories were subsequently made by several researchers. This morning (11/24/07) I asked Dr. Edmund Storms, the author of a new book:

"The Science of Low Energy Nuclear Reactions:
A Comprehensive Compilation of Evidence and Explanations About Cold Fusion."
(Published in 2007 by World Scientific Publishing Company)

to summarize what has been posted so far. His reply (see below) prompted me to compose, and post, this unit. It is a collage of quotes, shown with premissions. To preserve anonymity, contributors are idetifies as Researcher 1, Researcher 2, etc. (abbreviated as R1, R2, etc.). But first let me show the reply from Ed Storms (received this morning).

Reply to my message (11/24/07):
This is a good idea. However, it has two main problems. It takes time that I would rather spend finding out what is actually happening in Nature to which a theory can be applied. And, second, it is a sure way to lose friends in the CF community. Theoreticians take their ideas very personally and criticism, either implied or real, is not usually taken kindly. Criticizing theories that are either wrong or not useful gets us nowhere. The only useful activity is finding out from Nature what is actually happening, rather than making assumptions about the process. I made my previous comments only because a few people showed interest and because I object when theories are presented as real and useful when they are obviously wrong. I have no problem when people make efforts to understand the phenomenon with humility and an acknowledgment that their efforts are only a small and imperfect description of a plausible part of the process. Such an approach allows us to work together to achieve a sincere understanding, rather than an ego trip for a few people.

Selection from recent messages

R1 (11/2/07):
I think you need to focus your analysis on the assumptions, not on the math. The math is always correct as far as it goes. However, this fact means nothing if the assumptions on which the math is based are wrong. For example, Larsen-Widom assume that an electron can gain weight by accumulating energy from a low voltage field and that this stored energy allows the electron to combine with a proton to make a neutron. This assumption is in conflict with the behavior of the electron in every other situation, except the one to which they apply their model.

As another example, R2 makes the assumption that deuterons can form a cluster in the tetrahedral sites within the PdD lattice, thereby causing the ions to get close enough to fuse. Here again, no evidence is found in the large literature or suggested by R2 to support this assumption. In addition, the assumption requires the PdD lattice retain the small lattice space even though the cluster is present. All experience with this and similar materials show that this does not happen. The lattice expands as the sites are occupied.

With a little encouragement, I can note similar violations of general experience exhibited by other theories in the CF field. My question is, why don't the theoreticians note such problems and attempt to avoid them? Creating a theory that is obviously in conflict with general experience does not advance the field. Instead, we all are made to look like we accept any idea, no matter how ridiculous, as long as it "explains" CF. People doing experiments are required to justify every conclusion and demonstrate they have considered every trivial possibility. Why are theoretician exempt from this requirement?

R2 (11/2/07):
I do not understand why you referred my theory here. But, I want to point out that you are misunderstanding my model. For initiation of 4D/TSC (t=0) state (about 74 pm d-d distance), I have never assumed the steady state cluster existing in PdDx lattice, but I have been studying rather dissipative structure of dynamics (very fast transient clustering motion) with very small probability in the view of usual solid state physics. For example, deuteron density of PdDx is on the order of 1E+22 deuterons/cc. According to my latest results of Langevin equation analyses, one watt per cc 4D fusion rate with 1E+11 He-4 ash/cc/s can be generated by very small probability (1E-11) of the order of 1E+11 4D/TSC(t=0) per cc/s of PdD under D-phonon excitation.

Only 4D/TSC dynamic condensation motion under the double Platonic symmetries condensing to a very small charge neutral entity (about 20 fm d-d distance at the last moment of condensation, which is small enough and can thus exist in inter-lattice space transiently; one condensation process happens within 1.4 fs in my calculation!) can realize the super-screening of Coulomb repulsion among deuterons to get to strong interaction range (about 5 fm d-d distance) with significantly large barrier penetration probability (time-dependent) to produce 2 He-4 out-going particles without visible hard radiations (when observed from outside of cells), compared with the cases of D2 molecule, D3(+) molecule and D6(2-) molecule clusters which have converged states (ground states = steady states) with 74-80-40 pm d-d distances (thus difficult to be steadily existing in PdDx lattice inter-space without expanding lattice constants). This very small order of probability (1E-11) of dynamic motion has never been treated in orthodox solid state physics of metal-hydride lattice, or surface or interface of complex condensed matter systems. The dissipative structure looks of key. We need of course further elaboration of my model.

Dear CMNS group people; For detail, please see my papers to ICCF13, Catania07 and ACS Proceedings (LENR Source Book) to be published (some preprints with ppt slides will be uploaded soon on the iscmns-web site) together with my older papers (as recently published in Vol.1 of Electronic Journal of CMNS on http://www.iscmns.org/CMNS/publications.htm).

Apart from a debate on my own model, I hope, Steve and you and others will extend fair scientific discussions on all the proposed (or published) theoretical models. Quantification of theories with right mathematics isessential to all modeling processes, as well as proper physical insights. Self-consistency in each modeling should be self-satisfied. I remember, Peter Gluck and Steve Krivit made once a survey on available theories and reported the result on New-

Energy-Times web site. Widom and Larsen are new comers after the survey. I hope they will appear in our CMNS meetings as ICCF14 to make presentations and open discussions.

In my view, there are possibly a lot of models (due to freedom of our imagination) and we need a natural selection process to get to a final resolution someday (not now) through free discussions and random competition, to be referred to the progress of experimental results. I only prefer the truth of science.

R1, after responding to specific points in the above message **(11/2/07):**
. . . I see that we agree with the approach that needs to be applied to theory. My comments are offered to stimulate discussion and give an opportunity to address the obvious issues. I look forward to a productive debate.

R3 (11/3/07):
The controversy over theory suggests that a major deficiency in this field is a good review of all models. Without some theoretical framework, how are experimentalists supposed to refine their work? But with so many conjectures which attempt to "explain" no more than a tiny fraction of experimental observations, how is any experimentalist able to take "theory" seriously.

I commend Steve Krivit for seeking facts, but it is inevitable that response to any survey will be biased opinion. The Widom-Larsen "theory" has something to commend it, but like most theories it totally fails to explain Iwamura's observations where atomic mass apparently increases by 8 or 12. This is possibly R2's objection. In my opinion the only theory which begins to explain Iwamura's results is John Fisher's poly-neutron theory. This does not mean that I believe in such a theory. Poly-neutrons may well not exist. But the underlying multiple nucleon transfer mechanism may well be correct.

In competition with John Fisher is Akito Takahashi and his tetrahedral condensation reactions. Apart from Ed Storms' comments, there are 2 other preoblems here. Firstly, such reactions are excessively energetic and would give rise to significant lethal radiation. Secondly, they are very unselective. Anything goes. 4 deuterons will react with every known natural isotope and the results would be obvious! In contrast the poly-neutron theory is gentle and therefore very selective. In particular, it predicts that 141Pr is the inert end product from 133Cs. Any volunteers to write a review of CMNS theories? ;)

R4 (11/13/07):
My response violates [Krivit's] condition 3 for the following reason: To a first approximation every theorist believes that all theories (except his own) are wrong, and he doesn't bother to read other theories carefully if at all. It is difficult to get him to comment objectively. As an alternative I suggest that each willing theorist provide a list of clear-cut implications of his own theory, including:

A. Predictions of previously unknown and subsequently verified phenomena, and
B. Additional implications that confirm previously known phenomena.

To break the ice I list here some of the implications of polyneutron theory. They can be found in more detail in the paper "Outline of Polyneutron Theory" that I presented at the ISCMNS workshop in Catania, now available on line at

http://www.iscmns.org/catania07/program.htm

A. Predictions of previously unknown and subsequently verified phenomena include:

1. Energetic particles in an electrolyte (verified by Oriani).

2. Energetic particles in the vapor over an electrolyte (verified by Oriani; a shower of 150,000 alpha particles with energies about 2 MeV).

3. Energetic particles in the air outside an electrolysis cell (verified by Oriani).

4. Transmutation of 138Ba into 144Nd (confirmed in data of Iwamura et al.).

5. Transmutation of 137Ba into 136Xe (confirmed in data of Iwamura et al.).

6. Transmutation of 137Ba into 138Ba (confirmed in data of Iwamura et al.).

B. Implications that confirm previously known phenomena include:
1. Production of 4He.
2. Production of 3H.
3. Production of energetic protons.
4. Absence of neutrons.
5. Alpha particle showers.
6. Maximum energy production of about 21 MeV per 4He in deuterated Pd.
7. Transmutation of 133Cs into 141Pr.
8. Transmutation of 138Ba into 150Sm.
9. Role of electrolysis in sustaining nuclear reaction.
10. Role of calcium in sustaining nuclear reaction in deuterium diffusion experiments.
11. Absence of bremsstrahlung.
12. Energy of 2.1 MeV for alpha particles emitted in vapor over an electrolyte.

Although the theory is pretty good at determining which reactions are exothermic and which are endothermic, it is not so good at determining the relative rates of competing exothermic reactions. I expect that my current rate assumptions will require adjustment as additional experiments provide better guidance. Of course there is a possibility that theory and experiment will come into irreconcilable conflict, in which case the theory will have to be discarded.

R5 (11/13/07):
This point from R4 is hugely significant and has been stated explicitly and formalized by two of the most able theorists I have known. Giuliano Preparata said at ICCF5 (from memory) "At any time there is only room for one correct theory. I am right, the rest of you are wrong". Julian Schwinger refused even to look at other theories lest they contaminate his thinking. The problem I have as a non theorist is that I have neither the time nor tools to understand any of the theories well enough to know what aspects of them (if any) are right. This job really does need to be done by theorists. Steve is trying to help us hurdle this barrier to progress. We need a process or protocol. Does anyone have any ideas? (PS I did not intend to dismiss R4's suggested protocol, but rather to encourage input on it.)

R1 , addressing R4 **(11/13/07):**
I think your suggested approach is excellent. You are right, the only person who accepts any theory is the originator. In addition to listing what the theory predicts, I think it is more important to list the assumptions on which the theory is based. I get the impression that many theoreticians do not understand all of the assumptions they actually make. In addition, unless the assumptions are rational and consistent with experience, the logic that follows has no meaning, no matter what it predicts. I suggest a debate about what is a proper assumption is more useful than a debate about the theory. Once people agree upon what is a good assumption, we can then discuss the best mathematical form or logic to apply to this assumption.

R2 (11/13/07):
In JCF communications (in Japanese), N. Yabuuchi pointed out: W. Heisenberg once proposed a hypothesis that electron exchange between proton and neutron made sticking force (nuclear strong interaction) of nucleus. Later we found this was wrong assumption. However his proposal helped H. Yukawa to imagine the idea of "hypothetical-meson" (charged pion in latest understanding) exchange between neutron and proton as nuclear sticking force. H. Yukawa got Nobel Prize with the pion exchange model. But later, we found the Yukawa model was not exact to be replaced with the QCD theory. Thus even "wrong assumptions" have made significant contributions to the progress of finding true laws of physics. We may learn form the history.

R6 (11/13/07): referring to R5
The test of any theory is prediction. It is hard to determine what the best theory is until..... just wait until a theory

makes a prediction and then try it. I look for theories that how to turn a knob or do something. If it cannot do that then all you can do is wait until the theorist "connects" to the real world. If someone can say try adding He-4 or put a magnetic field on or something I can do in the lab (that I can afford) I try it. The problem is that most theories are not there yet.

R7 (11/14/07):
I think that several of the theories we know in CMNS seem "good" from the point of view a experimentalist that is NOT deep expert in theories.* BTW, most of the theories are developed by real professional Scientist and it is hard to think that they make trivial errors. We can't decided about just looking to formula. But, what we really need, is a theory that is PREDICTIVE to some (or better several) experimental set-up. I am waiting for that.

R8 referring to the above message **(11/14/07):**
Like e.g. (random example ;-) the DIESECF hypothesis, which predicts that applying different current densities to the opposite sides of a Pd membrane cathode should enhance CF. The team I belong to will keep the group tuned when we will have confirmed or infirmed this, as I hope will the other teams trying the scheme (one of which has kindly informed me they were)...

R4 addressing R1 **(11/14/07):**
I think your suggested approach is excellent. You are right, the only person who accepts any theory is the originator. In addition to listing what the theory predicts, I think it is more important to list the assumptions on which the theory is based. I get the impression that many theoreticians do not understand all of the assumptions they actually make. In addition, unless the assumptions are rational and consistent with experience, the logic that follows has no meaning, no matter what it predicts. I suggest a debate about what is a proper assumption is more useful than a debate about the theory. Once people agree upon what is a good assumption, we can then discuss the best mathematical form or logic to apply to this assumption.

R1 addressing R4 **(11/14/07):**
I agree with everything you wrote. However, I think a distinction needs to be made when the word "prediction" is used. Actually, most of the time what is called a prediction is actually a statement about which of the observations the theory has been made to fit. We all know that a theoretician works hard to make the theory consistent with what is known. The result is not a prediction, but simply a version of curve fitting. A true prediction involves a behavior that is not known in advance and was not used by the theoretician to create the theory. It must be a true extrapolation of the model.

Regardless how well the theory seems to fit the observations, if the assumption on which the theory is based predicts behaviors in other fields of science that have never been observed or violates well established understanding, such assumptions need to be viewed with caution. It is simply too easy to apply various versions of math or logic to "prove" anything. The judgment of truth is always based on the reasonableness of the assumptions, and how well the theory fits ALL the observations, not just a few that happen to fit.

I have no problem with the process of exploring all ideas no matter how novel. However, everyone needs to be very humble and acknowledge that the proposed ideas probably are very far from the truth and are presented only to encourage discussion and to suggest experiments.

R1 quoting what R2 wrote about Yukawa**, (11/14/07):**
Yes, occasionally this happens. However, I suggest the much more common result of bad assumptions are distraction, waste of time, and a lot of papers that have no meaning. The point I'm making is that the initial assumptions should be examined much more carefully than is the math and logic, i.e. that part of the paper that takes up over 90% of the space and the part that is used to demonstrate that the theory has any value. In short, I think emphasis is being applied to the wrong part of the theory in most cases.

R2 (11/14/07):
Without trials of new modeling with mathematical quantification, one can not have such good occasions as Heisenberg, Yukawa and so on have experienced. Of course there are also proposed "theories" with misunderstanding

or neglecting of well established scientific facts, or often with occult ideas. Rationalism and criticism may screen these, push the progress of theorization, and are therefore important. Experimental results being accumulated may also make cross checking of theoretical predictability. If one wants to determine which theories are based on "good assumptions" or "bad assumptions", who can do it without arrogance is however the problem.It seems for me that the natural selection by free competition may get to the resolution someday after good "effective collaboration" of people for seeking truth. Anyway I hope to see hot debate on theorization of CMNS.

R9 addressing R1 **(11/15/07):**
In my humble opinion, what you say makes a lot of sense. I fully support the approach you propose.

My own comment (posted on 11/25/07):
On 11/13/07 R2 wrote ". . . Later we found this was a wrong assumption. However his proposal helped H. Yukawa . . . " That was a good reminder that theories have their own ways to evolve. The evolutionary flowchart in

http://csam.montclair.edu/~kowalski/cf/336cat.html (see Figure 1)

has two separate boxes for theories. One box is refers to "inspiration" and another refers to "explanations." Sometimes theories are developed to extrapolate from known experimental facts (explaining and predicting) and sometimes logical predictions are made on the basis of ad hock assumptions, such as existence of neutrinos. I am thining about W. Pauli who postulated existence of neutrinos in early 1930. His prediction was confirmed about ten years later. That was a well known example of an experiment inspired by an ad hock assumption. The competitive ad hock assumption of N. Bohr (non-conservation of energy at subatomic level) was abandoned. Bending of light by gravitational fields was also discovered (during a solar eclipse) after it was predicted on the basis of an ad hock theory, by A. Einstein.

Note however, that the box called "inspiration" does not have to be limited to theories. Experimental discoveries often lead to other discoveries. Marie and Pierre Curie would not start searching for radium before radioactivity was discovered by Becquerel. If I were a historian of science I would probably be able to provide illustrations in which successful experimentalists were motivated by intuition, or other factors. I hope this thread will continue.

P.S. A Sarcastic piece at http://www.tcm.phy.cam.ac.uk/~bdj10/articles/PF_fire.html, and the recent discussion of theories, made R9 compose the following satire:

R9 (11/26/07):
Recently, Pierre and Marie Curie have reported observations of a metal permanently warmer than its surroundings. Our panel has concluded that this observation is not possible, and recommend no further research in this area. It is well established that heat cannot of itself pass from a colder to a hotter body. This basic tenet of thermodynamics have been proven again and again, and has permitted major technical advances such as the steam engine. We conclude that the metal of Pierre and Marie Curie cannot draw his heat from its surroundings. If it could, it would be possible to create a perpetual motion, an idea that is widely rejected by mainstream scientists. We therefore conclude that spending money and energy on this topic would be ill-conceived.

Steve Krivit (whose message started this thread):
As of Nov. 26, I have not received a single third-party comment about any CMNS theory besides W-L. The point of the inquiry was to find out if there were any other theories that were sufficiently developed to the point that a person besides the theory's author felt sufficiently confident to provide at least a minimal endorsement for it. The W-L theory caught my attention because Dave Nagel was, at least at one time, sufficiently confident to be enthusiastic about it, and he encouraged me to look at it.

R10 (11/27/07):
An interesting example of "a minimal endorsement" can be found in the issue 75, 2007 of Infinite Energy. I am thinking about the Rudesill's interview of Storms. Both seem to endorse Mills' theory of hydrinos. Another crazy particle? The following URL will download the file. http://www.infinite-energy.com/images/pdfs/stormsinterview.pdf

R11 (11/27/07):

I would second Storms' suggestion that the hydrino (a hydrogen atom with a sub-groundstate electron orbital) is a valid contender for LENRs. While there are problems with Mill's model(s), and I believe that he is looking in the wrong region (hot plasma instead of solid state) for hydrino production, if non-photonic (or phononic) transitions can be induced, then the "groundstate" energy is no longer sacrosanct. I am actually exploring (theoretically) the transitions into a non- 1/r potential to see if this can shift the ground-state.

R3 (11/27/07):
I'm probably just ignorant of Mills' research, but if mini-atoms and molecules existed we would expect all sorts of phenomena which we do not observe.

1) We might expect detectable gamma rays from p-d fusion at the incoherent rates calculated by Koonin in NATURE VOL 339 NO. 6227 p 690-691, 29 JUNE 1989. (I pointed this out in my 27 July 2005 posting to the CANA forum.)
2) We might expect that over the 5 billion years since the solar system formed that giant planets would convert their hydrogen to very dense mini-molecules. But all the
evidence suggests that the hydrogen / helium ratio remains 3:1 as it was at the big bang.
3) If mini-atoms are the product of exotic chemistry, how is it that they have not been isolated in large quantities in the lab?
4) How are Mills' ideas consistent with Iwamura's observations?

R12 (11/27/07):
Here I go sticking my experimental nose into theoretical business.....)

As I understand it, it is relatively easy to show according to QM that the hydrogen ground state is the lowest possible energy state for a proton and an electron. If the electron is forced to exist closer to the proton, that reduction in the uncertainty of its location produces a corresponding increase in its energy. The resulting "Schroedinger pressure" counteracts the Coulomb attraction between the electron and the proton. The ground state is simply that condition in which these two forces are balanced.

This argument is supported by the fact that a neutron...essentially a collapsed hydrogen atom. has about 780 keV MORE energy than a ground-state hydrogen atom.

QM is, without question, the most successful physics theory ever derived. If it has any shortcomings, they are well hidden and very, very subtle. The argument above is not subtle. It is based upon the very foundation of QM. Countless experiments have shown this foundation to be unassailable.

Not to put too fine a point on it, the hydrino is therefore nonsense. R13, surely your understanding of QM tops that of most/all members of this forum. Would you please comment on the above?

My own contribution, (11/29/7) prompted by the sarcastic piece of R9**:**
The English translation of the famous 1903 paper (see attached file *) shows that the reported excess heat was generated at the rate of about 100 cal per hour. This translates into 116 mW. . . .
(* Source: G. Holton , General Editor, “The Discovery of Radioactivity and Transmutation” Dover Publicatons, INC. New York, 1964)

P.S. I wander how different the history of CF would be if Fleischmann and Pons, instead of claiming nuclear origin of excess heat, used the same ending as Curie and Laborde. Here is the ending of their 1903 paper again:

"The continuing development of such a quantity of heat cannot be explained by an ordinary chemical transformation. If we seek the origin of the production of heat in internal transformation, this transformation must be of a more profound nature. . . . This development of heat may still be explained by supposing that..... [our setup] makes use of an external energy of unknown nature."

A possibility of nuclear energy would certainly be raised by some speculation-inclined people. But theoretical arguments against interpretation would be decoupled from the experimental claim. The only way to challenge reality

of excess heat would be to perform careful experiments.

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339) Two simple electrical orbiting systems: possible and impossible

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In December of 2007 this note was submitted to American Journal of Physics. But it was rejected, after an interesting exchange of messages between me and the editor of the journal. The issue had nothing to do with Figier'h hypothesis; our discussion focused on Section II, more specifically, on the issue of stability of my macroscopic three-body system. I plan to discuss this topic (Section II only) on a forum for physics teachers. The outcome of that discussion, if any, will be appended at the end of this unit.

I. INTRODUCTION

A solar system is a well-known example of dynamic equilibrium of gravitational forces. The simplest model, a single planet (mass m) revolving around a single star (mass M>>m), along a circular orbit, is often analyzed mathematically in physics textbooks (1,2). An analogous electrical system is subsequently discussed in the context of Bohr's model of a hydrogen atom. Here we emphasize that all orbits are possible in a macroscopic system but only certain orbits are possible in the corresponding submicroscopic system. The quantization of angular momenta (Bohr's postulate, formulated nearly a century ago) is still a pedagogically sensible path to quantum mechanics (3,4).

This note describes a macroscopic orbital system in which two positively charged particles, of equal mass and equal charge, revolve around a negatively charged particle, along a circular orbit. The mass of the central particle is arbitrary but the magnitudes of the three charges are identical. This leads to a description of an analogous submicroscopic system. Such problems are likely to promote independent analytical thinking. Linking QM problems with corresponding classical problems, when possible, has numerous pedagogical advantages. Motivation for this note came from a debate about the idea of a system in which two protons orbit an electron (5).

II. CLASSICAL CONSIDERATIONS

The system to be described is illustrated in Figure 1. Gravitational forces are assumed to be negligible in comparison with electrical forces. In that context the mass of the central particle is totally irrelevant. The orbiting trajectory would remain the same if the mass of the central particle were changed. This is not true for a solar system; trajectories of planets would be different if the mass of the star were different.

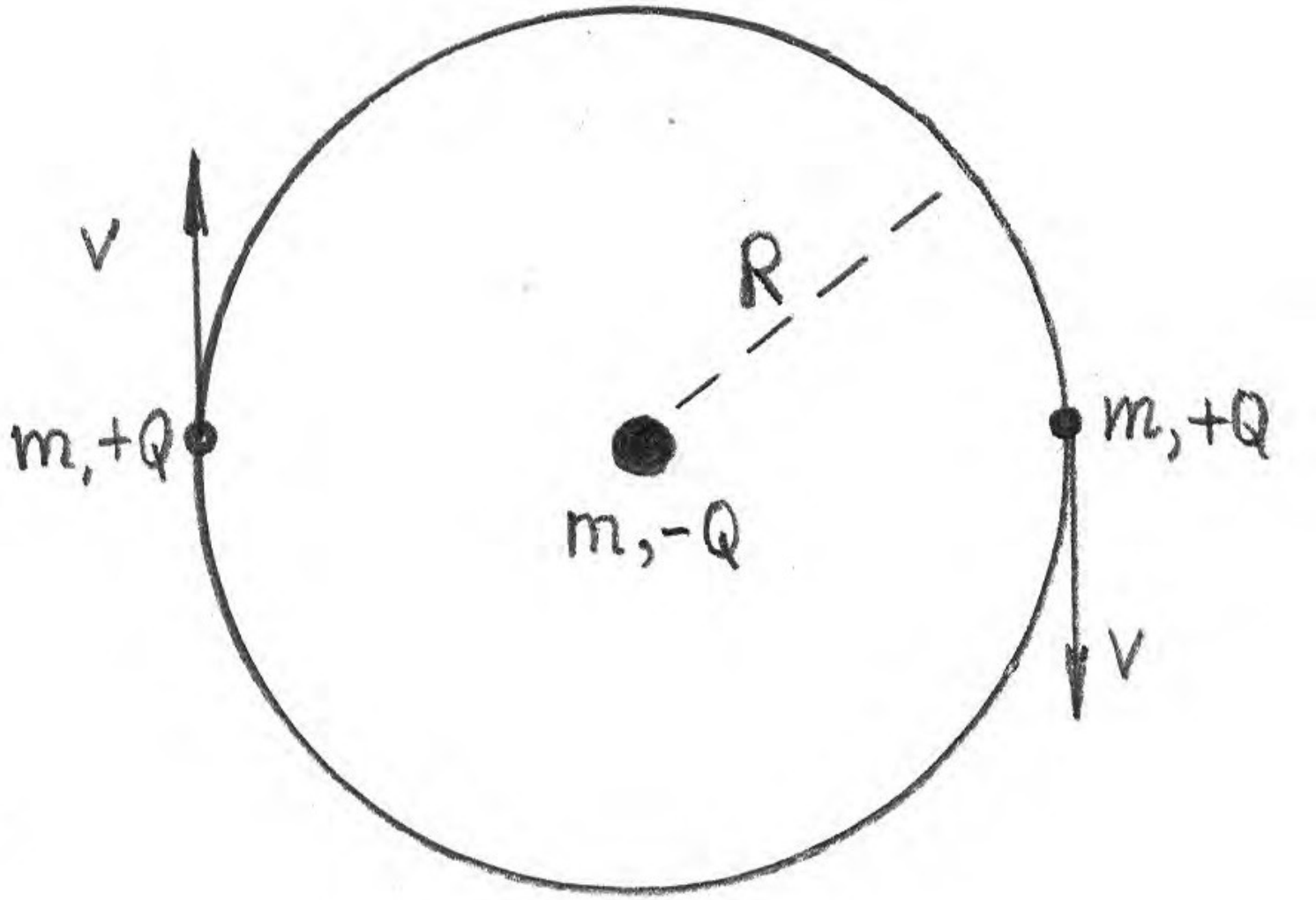

Figure 1
Caption: A three-body system to be examined in this note. Emission of electromagnetic radiation by centripetally accelerating charges will be ignored.

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For the set of three electric charges shown in Figure 1, the potential energy is:

$U = k*Q^2 / (2*R) - k*Q^2 / R - k*Q^2 / R = -(3/2)*k*Q^2/R$ (1)

where Q is the magnitude of each charge, k is the Coulomb law constant and R is the orbit radius. Negative terms prevail over the positive term and U is always negative. The central particle is at rest and the combined kinetic energy is

$K=m*v^2$

The net force on each positive particle, according to Coulomb's law, is $0.75*k*Q^2 / R^2$. The speed can thus be calculated by writing

$0.75*k*Q^2 / R^2 = m*v^2 / R$ (2)

This also shows that

$K = 0.75*k*Q^2/R$ (3)

The total relevant energy is

$E = U + K = - (3/4)*k*Q^2/R$ (4)

The angular momentum of our simple system is

$L = 2*m*v*R$ (5)

A numerical illustration:
Suppose that the mass of each particle is 1 kg and the magnitude of each charge is 1 C. What should the speeds of positive particles be if the desired radius of the circular orbit is 100 meters? The answer, according to Formula (2) is 8216 m/s. Our use of non-relativistic formulas is justified because v is much smaller than the speed of light. Note that the answer does not depend on the mass of the central particle. The potential and kinetic energies of the system, according to Formulas (1) and (3), are $-13.5*10^7$ and $6.75*10^7$ joules, respectively. The total relevant energy, according to Formula (4) is $-6.75*10^7$ joules. The angular momentum of the system, according to Formula (5), is $1.64*10^6$ J*s; this is about 39 orders of magnitude larger than Planck's constant.

The negative sign indicates that the system is bound in two dimensions. But is it also bound in three dimensions? The answer seems to be positive. To justify this answer, consider the configuration shown in Figure 2. The horizontal segment is the side view of the orbit shown in Figure 1. The cross represents the location of the central charge resulting from a virtual displacement. The distances between charges become d_1, d_2 and 2*R. The potential energy of the new configuration is

$U' = k*Q^2 / (2*R) - kQ^2/d_1 - k*Q^2/ d_2$

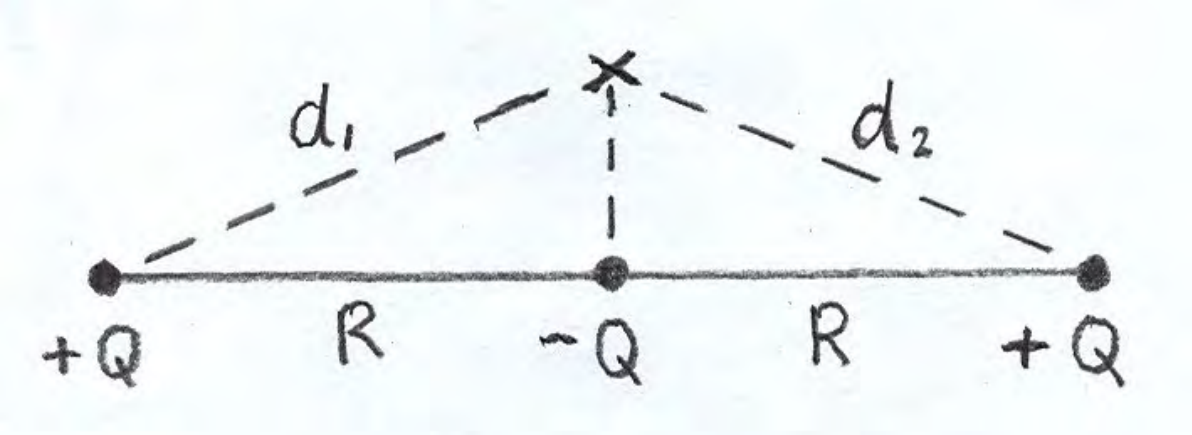

Figure 2
Caption: What happens to potential energy of the system when the central particle is virtually displaced in the the direction perpendicular to the orbit, either up or down? It becomes higher (less negative).

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The positive term does not change but the sum of negative terms becomes smaller. In other words, U' is higher (less negative) than our previous U. A vertical
displacement of the central charge by 10 meters, either up or down, corresponds to the increase of potential energy by $9*10^5$ joules. A diagram similar to that in Figure 2 can be sketched for a positive charge being displaced perpendicularly with respect to the horizontal orbit. The potential energy for that configuration, U", also turns out to be less negative that what one finds when the displacement is zero. For a vertical displacement of 10 meters, for example, the value of U" is by $4.5*10^5$ joules higher than U. It is a numerical problem and numerical illustrations are appropriate.

III. ELEMENTARY QUANTUM MECHANICAL CONSIDERATIONS
Some students might be familiar with elementary quantum mechanics (Bohr's angular momentum postulate and Heisenberg's Uncertainty Principle). These conceptual tools can be used to analyze a hypothetical submicroscopic system in which two protons orbit an electron. In a classroom debate, students can be guided to show that such a system could not exist. The set of formulas to be used in such activity is show below.

According to Bohr's angular momentum postulate,

$L=n * h$ (6)

where h is the Planck's constant divided by $2*\pi$, and n is a quantum number for the orbit. Find the smallest R for the p-e-p system (two protons orbiting an electron). The method introduced by Bohr can be used to discuss this hypothetical system. Equating right sides of the Equations (5) and (6) one has

$v = h*n / (2*m*R)$ . . . (7)

Substituting this into the Formula (2) gives

$R = h^2*n^2 / (3*k*e^2*m)$..... (8)

The energy for that orbit can be calculated by substituting this radius into Formula (4). The result is

$E = - 2.25*m*k^2*Q^4 / (h^2*n^2)$..... (9)

For each proton $m=1.6710^{-27}$ kg and $Q=+1.6*10^{-19}$ C. The charge of the electron is $-1.6*10^{-19}$ C. Knowing that $h=1.05*10^{-34}$ and $k=9*10^9$, both in SI units, one can
calculate R, v and E. For the ground state orbit (n=1) the result are: $R=9.55*10^{-15}$ m (9.55 fm), $v=3.29*10^6$ m/s (1.1% of the speed of light) and $E=-1.81*10^{-14}$ J (113 keV). Note that for larger n the radius becomes n^2 times larger than for the ground state while the absolute value of E, not surprisingly, becomes n^2 times smaller (E approaches zero when n becomes very large). In other words, according to the semi-classical Bohr's theory, the suggested p-e-p structure would be much smaller than the hydrogen atom. The binding energy of that system (the absolute value of E at n=1) would be much larger than for the hydrogen atom (where it is only 13.6 eV).

It is interesting that the distance between protons, on the ground state orbit, 19.1 fm, is still much larger than the range of the strong nuclear force (~2 fm). Only electrical forces are of any significance in this problem, as initially assumed. Also note that the energy at n=2 is -113/4=-28.2 keV. The difference, 84.7 keV, corresponds to the lowest energy photon in the Balmer series. For hydrogen, spectral lines of that series are in the ultraviolet region; for the p-e-p system they would be in the X-ray region. Speculations about significance of such findings can be educationally valuable.

The above considerations could naturally lead to Heisenberg's Uncertainty Principle, dp*dr ~ h. Is that principle (HUP) consistent with the existence of the conceived p-e-p structure? The answer turns out to be negative. To demonstrate this numerically one could begin with $E=-1.81*10^{-14}$ J (113 keV), as above. That represents the depth of the ground state potential energy well. According to initial consideration (see Figures 1 and 2) the electron was assumed to be at rest in the center. That implies that both location and momentum are accurately specified (r=0 and p=0). But this is impossible, according to HUP. By decreasing the uncertainty in r we are always increasing the uncertainty in p, and vice versa.

The maximum possible uncertainty (fuzziness) in p can be estimated from the depth of the well. An electron, randomly oscillating about the center, would escape from the well if its kinetic energy, K, were larger than the depth. This gives us the maximum possible $K = 1.81*10^{-14}$ J. The corresponding linear momentum, p, for an electron, is $1.81*10^{-22}$ kg*m/s. Using that value in HUP one has $dr=5.80*10^{-13}$ m. This is 61 times larger than the above-calculated radius of the p-e-p structure. The initial assumption (electron being confined to a small sphere at the center of the orbit) is in conflict with HUP. It is easy to show that, in order to be consistent with HUP, the mass of the electron would have to be at least as large as the mass of an alpha particle. Motivation for this note, by the way, was provided by such discussion among several researchers. The idea of a tiny particle in which two protons orbit an electron was actually suggested by Jean-Pierre Vigier (5), a French theoretical physicist who worked with Louis de Broglie. No one seriously claimed that such particles exist in nature. But theoretical speculations about them can be pedagogically instructive.

IV. CONCLUSION

a) Linking of similar classical and non-classical problems has obvious pedagogical advantages in undergraduate physics courses.
b) The idea of the possible existence of a stable p-e-p structure, conceived and published by Vigier (5), offers a rare case in which an interesting hypothesis can be numerically investigated by using elementary QM tools. Students familiar with such tools are likely to benefit from a properly structured discussion of Vigier's hypothesis. Note that the hypothetical p-e-p system is similar, but not identical, to the neutral p-e system, analyzed by Bohr.
c) The mass of the central particle, totally irrelevant in the macroscopic problem, becomes highly relevant in the analogous submicroscopic one. The idea of hidden parameters is interesting.

V. ACKNOWLEDGMENT

Contributions from Scott Little (USA) and Akito Takahashi (Japan) are highly appreciated. Information about Vigier was provided by Jean-Paul Biberian (France) and by Andrew Meulenberg (USA).

Should I interest comments about Bohr's model and HUP, found in (6) ?

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VI. APPENDIX

I was not aware, when the above was composed, that the proposed classical system would be highly unstable. In real life, unlike in idealized world of mathematical ideas and simulations, inevitable small perturbations would quickly destroy the system. This would become visible in less than one period of one idealized (perturbation-free) revolution. The corresponding two-body system, like a star and a planet, is expected to be stable under such perturbations. But this is not true for the orbiting three-body system, like three stars, or three charged macroscopic spheres. This appendix was inspired by messages posted by several physics teachers during an interesting Internet debate about stability and chaos.

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340) Can decay of radioactive substances be speeded up?

Ludwik Kowalski (1/10/08)

Montclair State University, Montclair, NJ, 07055

The issue of possible influence of chemical environment on nuclear phenomena is not new; it has been investigated by Marie Curie. The probability of a process taking place inside of an atomic nucleus, such as radioactive decay of radium, was shown to be the same in all substances containing that element. It was also shown to be independent other factors, such as high pressure, magnetic field or cooling. Factors of that kind affect atoms and molecules; they do not affect atomic nuclei. Cold fusion researchers, however, claim that this is not always true. The ongoing controversy surrounding that topic started as soon as discovery of cold fusion was announced in 1989.

In some cases, however, as reviewed in (1), the effect of chemical environment on the probability of nuclear decay has been demonstrated and understood. It happens when beta-plus radioactive substances decay via electron-capture. According to (2), for example, the nuclear decay rate of ^{7}Be implanted in palladium is about 0.8% higher than in ^{7}Be implanted in gold. According to (3) the decay rate of ^{7}Be in BeO is 54.23 days while in $Be(OH)_2$ it is 53.42 days. This amounts to the 1.5% difference. The effect of chemical surrounding on the probability of the electron-capture process has been recognized by mainstream scientists long before 1989. It is considered to be an exception from the general rule -- other forms of radioactive decay cannot be modified by making changes in chemical environment. As indicated above, the ^{7}Be effect is rather small.

Can the rate of another kind of radioactive decay be changed? The negative textbook answer to this question conflicts with observations described by at least two teams of CMNS researchers. John Dash et al., for example, reported that the decay time of the alpha-radioactive uranium was shortened by a factor of four after the material was exposed to the glow-discharge plasma for 18 hours (4). Likewise, V. Vysotskii et al., reported that the decay time of the beta-radioactive ^{137}Cs, in some biological cells, was reduced from 30 years to less than one year (5). Bacteria, as suggested by authors, are able to cause the

$^{137}Cs(p,\gamma)^{138}Ba$

transmutation process. The reaction is exothermic (5.58 MeV) but the coulomb barrier is very high (~10 MeV). I do not know why such barrier is not preventing bacteria from transmuting cesium into barium. The main question, however, has nothing to do with an explanation; it has everything to do with validity of the experimental claim. This claim is important because ^{137}Cs is a significant contributor to high radioactivity of waste produced by nuclear reactors. The suggested possibility of destroying radioactive waste with bacteria should be investigated. A confirmation would open a new era in the field of nuclear electricity. Unfortunately, as far as I know, nothing is being done to either confirm of refute the claim. Why is it so?

Excited by earlier reports on destruction of radioactive materials in a high-voltage electrolytic cell (6,7) I asked for a chance to see the replication of one experiment. The principal investigator of that study, Hal Fox, invited me and the experiment, in slightly modified form, was repeated in my presence. The modified version of the experiment was expected to demonstarte the reported effect. The observed reduction of radioactivity, however, turned out to be apparent. It was due to changes in the counting geometry occurring during the experiment. At the beginning the

radioactivity was in the electrolyte, at the end most of it was in the precipitation, at the bottom of the cell. The resulting reduction of counting efficiency produced an illusion of the loss of radioactivity, as described in (8).

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341) High voltage electrolysis: neutrons and production of tritium

Ludwik Kowalski; 12/25/2007

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Introduction

1) During a recent conference in Catania Yuri Bazhutov suggested that we use CR-39 detectors to explore a possibility that track-forming nuclear particles are emitted in one of the setup he experimented with in the past. This suggestion was a natural outcome of our conference presentations, my talk about investigations of Oriani-type effect and his poster about the ongoing search for erzions, elementary particles that might be able to account for some CMNS effects. Similar suggestion for cooperation was made by Pierre Clauzon, when I visited him in Paris (on the way to Catania). Pierre demonstarted to me generation of excess heat in an improved setup for high voltage electrolysis. About a month ago I sent each of them several CR-39 chips to perform preliminary, background testing, experiments. So it is about time I write something about about Yuri's experiment in which the CR-39 will be used. Pierre's new setup, by the way, is described in a report that can be downloaded from the library at <www.lenr-canr.html>. Numerous Bazhutov reports can also be downloaded from that library.

2) The concept of an erzion, a massive hadron, was first formulated, in 1981. Existence of such stable particle was postulated to explain energy spectra of cosmic-rays muons. According to Bazhutov's Catania presentation, there are two erzions, negative, E^o and E^-. Their internal structure, in terms of quarks, is not essential in my context. The rest mass of an erzion is said to be close to 200 u (atomic mass units), the charge of the negative erzion is the same as the charge of an electron. Negative erzions are believed to form loosly-bound structures with positive atomic nuclei. These structures are called enions. The binding energy of E^- and p, for example, is said to be ~1.5 eV, while the binding energy E^- and ^{208}Pb, is ~50eV.

Electrolytic - anodic glow discharge

3) Let me begin be summarizing a Russian paper of Y. N. Bazhutov, W.G. Grishin and W.N, Nosov. The title is "Electrolytic anodic glow discharge." The date can be inferred from the most recent reference --2003. The high voltage electrolysis apparatus used by Bazhutov et al, is illustrated in Figure 1. The upper left part shows Bazhutov's cell immersed in cooling water. The electrolyte is 7M KF in a 50% mixture of light and heavy water. The stainless steel cathode, on the right side, had the area of 400 cm^2. The anode was a tungsten rod (diameters 1 to 6 mm), only partially immersed in the liquid. The cell had to be cooled (in the aquarium-like container with ordinary water) because the electric power dissipated in the cell was rather high (glow discharge at 300 v with various currents up to 2A, depending on experiments).

4) The glow discharge at ~300 V, near the anode, was said to be quazi-stable (current in the plateau region was changing monotonically from about 1.2A at 200 V to about 1.5A at 300 V). Typically an experiment lasted for about one hour, depending on the rate at which the anode was consumed. Some electrolyte had to be added to the cell during the discharge to keep the current constant, more or less. The cell was placed into an aquarium-like vessel containing cold water. The temperature of the electrolyte was rising during the experiment, typically up to 50 C.

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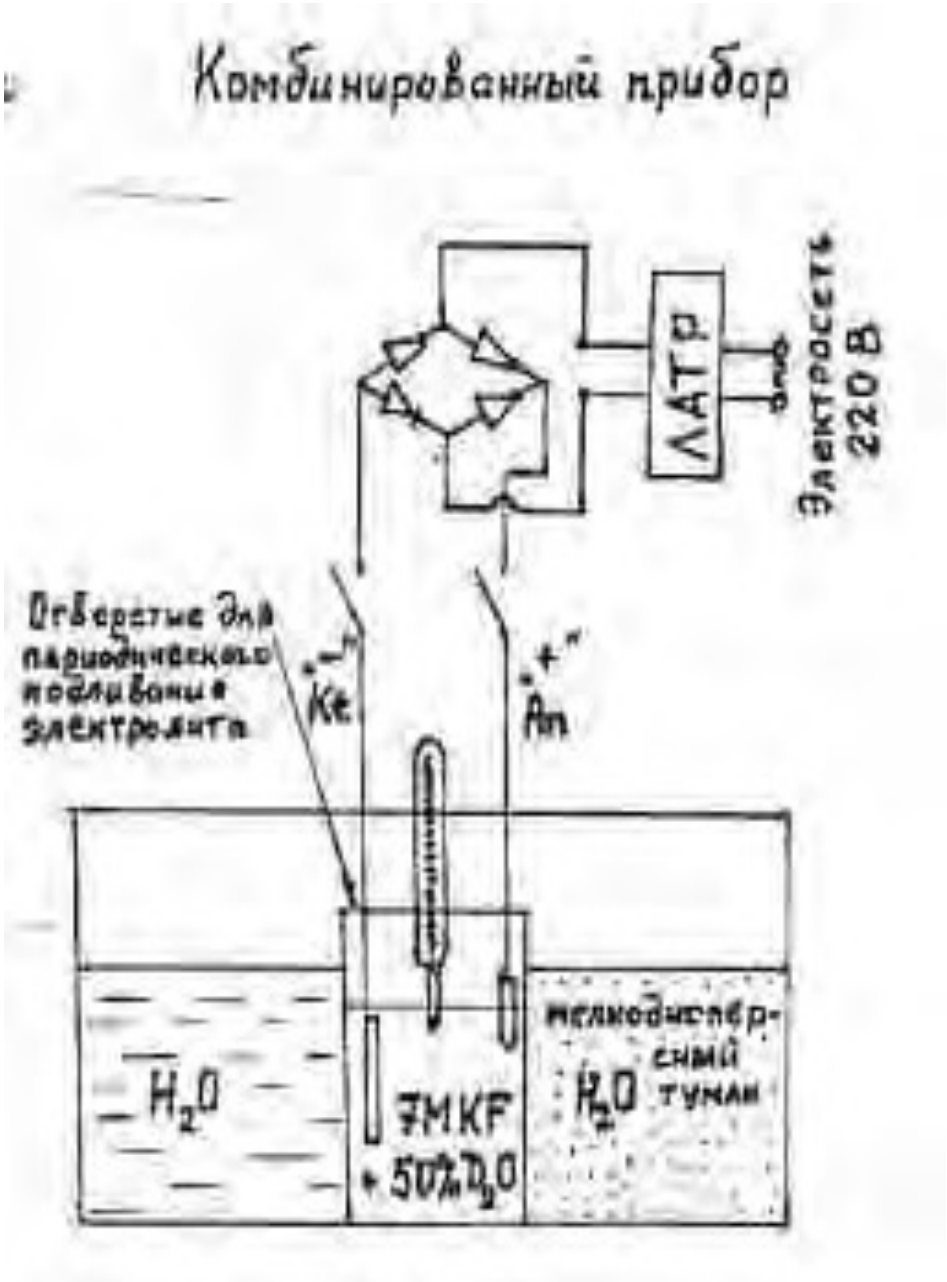

Figure 1
Bazhutov's cell. Dots in the right side of the aquarium-like vessel represent bubbles formed in cold water.

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5) The phenomenon discovered in these experiments consisted of unexplained changes in cooling water. After 40 minutes water on the right side of the cooling vessel started loosing its transparency. Water on the left side remained transparent, at the same 40 C temperature. A sample of bubbly water, removed from the right side, was tested for induced gamma radioactivity. No such radioactivity was found in it; the sample became transparent after 24 hours. Attempts to reproduce the long-term loss of cooling water transpaency with other electrolytes, and under different electrical discharge conditions, were not successful. But the effect was highly reproducible when experimenting with the tungsten-anode cell and the 7 M KF electrolyte containing 50% of heavy water.

Erzion interpretation of bubble formation

6) Note that cooling water at the right side (see Figure 1) is close to the anode while cooling water on the left side is

close to the cathode. The disappearance of bubbles, after the electrolysis, was very slow (half-life of about 10 hrs). Attempts to explain the phenomenon in terms of cavitation, and other ultrasonic effects, were not successful. The only satisfactory explanation was possible within the framework of the erzion model. Authors believe that bubbles are produced when neutral erzions, E^o, generated at the anode, are elastically scattered on ^{16}O. Enions are always present in water (one out of 10^{15} nuclei in ^{16}O is believed to be attached to a negative erzion).

7) Here are some calculations. At the current of 1 A, the flux of OH- ions, in the electrolyte, is ~10^{19} per second. Thus, the flux of enions bombarding the anode is $10^{19}*10^{-15} = 10^4$ per second. The following nuclear transmutation reactions, induced by enions, are believed to take place in the anode:

$^{180}W + E_N = E^- + {}^{181}W$ (equivalent to $^{180}W + n = {}^{181}W$)
$^{182}W + E_N = E^- + {}^{183}W$ (equivalent to $^{182}W + n = {}^{183}W$)
$^{183}W + E_N = E^- + {}^{184}W$ (equivalent to $^{183}W + n = {}^{184}W$)

8) Erzions produced in these reactions are expected to be isotropically distributed. In a 1 cm layer of the 50% D_2O electrolyte; their intensity is expected to be reduced by the factor of ten, due to inelastic nuclear reactions with deuterium. The glass wall of 2 mm is believed to be totally transparent to neural erzions. Thus the flux of neutral erzions arriving into the cooling water on the right side is expected to be about 10000*0.1*0.1=100 per second. The first 0.1 factor is due to absorption; the second is due to scattering in the electrolyte(the solid angle effect). This can be contrasted with the expected flux of negative erzions of 0.001 per second on the left side of the cell (because the layer of the electrolyte between the anode and the wall of the cell is 6 cm). That is why the effect described in the next paragraph is not observed on the left side.

9) Neutral erzions, entering cooling water have a range of about 1 cm. They collide with oxygen nuclei and produce recoils with energies close to ~0.1 MeV. Ranges of such recoils in water are about 1 micron. Each recoiling oxygen nucleus is thus able to produce a highly ionized region. Long-lasting bubbles of vapor (half-life of 10 hrs) are formed in regions of high ionization. Sizes of bubbles are of the order of 1 mm. Each negative erzion, entering cooling water, produces about 10 bubbles. The estimated number of long-lasting bubbles, produced in 1000 seconds is one million. In reading this I was thinking about bouble chambers containing liquid nitrogen. Such track-recording instruments were often used in high energy research.

Appended on 1/12/08

10) Let me begin be summarizing another Russian paper; it describes more recent resualts obtained by using a similar high-voltage electrolytic cell. The authors are Y. N. Bazhutov, V. Y. Velikdnyi, W.G. Grishin, A.W. Eremeev, E.W. Pletnikov, A. D. Rumiancev, Y. A. Sapozhnikov and N. I. Khokholov. The title is “Nuclear diagnostics of cold fusion transmutation at electrolysis with anodic gas discharge in water solutions.”

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Рис.1 Схема экспериментальной установки

1- датчик счётчика нейтронов (РУП- 1)
2- золотой или вольфрамовый анод (S<см²)
(катод - пористый Титан (S~30см²))
3- стеклянный аквариум с охлаждаемой водой (7л)
4- стеклянный корпус электролизёра (<500мл)
5- внешний свинцовый домик из стандартных модулей
6- внутренняя (дополнительная) свинцовая защита гамма – детектора
7- детектор гамма – излучения (стандартный блок «лимон» с кристаллом NaI (Tl) (Ø63x63мм²))
8- градусник

Figure 2
Experimental setup showing the detector of neutrons (on the left) and the detector of gamma rays (on the right of the cell).

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The paper begins by mentioning earlier publicaions that provided motivation for the new study. In the first publication the radioactive ^{183} Ta was produced during the electrolysis. It has been identified by characteristic gamma radiation peaks and by the half-life. The second publication has been described above (starting at point 3). The experimental setup in the new investigation was essentially the same as summarized above, except that four nuclear instruments were used in anticipation of nuclear radiation. These were:

a) NaI gamma ray spectrometer (6.3 by 6.3 cm) had a background of 100 c/s. Spectra, in the energy range of up to ~2MeV, were recorded on the 1024 multichannel analyzer.

b) A commercial gamma dosimeter “Sosna” (background 0.1 micro-rad per hour)

c) Two liquid scintillators to study production of tritium in the electrolyte (background ~1 Bk/mL). Samples of electrolyte (removed from the cell before and after each experiment) were injected into the liquid scintillator to detect tritium.

d) A commercial detector of thermal neutrons (RUP-1”) The available ranges (n/cm^2 per second were: 100, 1000, 10000 and 100000 sensitivity could be chosen

Twelve series of experiments, using different electrolytes and different anodes, were performed between July 20 and September 1, 2005. Cell temperature, current and voltage were recorded each minute. The readings of the dosimeter

and of the neutron flux detector were also recorded each minute.

11) The NaI detector was accumulating counts during the electrolysis and then, for the same duration, after the electrolysis. The authors wrote "unfortunately, the spectra were never seen to be not significantly different from the background." But emission of neutrons, and production of tritium, were observed in some experiments. Numerical results (minute-by-minute readings of the neutron detector), are shown in their figures 3, 4, 5, 6 and 7. The reported 2 neutron/cm^2 per second background was exceeded in all cases. But the actual counts in the 7th series, for example, fluctuated between 20 and 75 while in the 9th series they fluctuated between 2000 and 2200.

12) Production of tritium, on the other hand, took place only in the 5th series. It was at the level of about three times the background. The authors conclude "if we were really recording neutrons, then the maximum production (in the 9th series) was ~10^6. Using the data from series 5, in which production of tritium was observed, we conclude that the ratio of the total number of tritons (10^{12}) over the total number of neutrons (10^7) was close to 10^5. This is typical for cold fusion experiments but not for thermonuclear reactions, where the ratio is close 1."

13) The motivation for these experiments was to check the hypothesis that neutrons are produced by reactions induced by erzions. With this in mind, the electrolyte in series 11, was changed to make such reactions impossible (according to previously postulated mechanism). The absence on the emission of neutrons in that series confirmed the hypothesis. In all other series the choice of the electrolyte was made to maximize, not to minimize, the mechanism based on erzion theory. In that sense, one can say that experimental results are consistent with the theory.

Appended on 1/18/08

14) Here is a brief summary of some terminology. Hopefully, it will be sufficient in the context of Yuri's investigations. It is based on what I knew before the conference in Catania and what I learned from Yuri during that conference.

a) Nucleons are not elementary particles; they are composed of particles called quarks. The up quark, u, has a charge of +2e/3 while the down quark, d, has a charge of -1e/3, where e is the magnitude of the charge of one electron.

b) A neutron consists of one u and two d; that is why its electric charge is zero. A proton consists of two u and one d; that is why its charge is +1e.

$$n = \{u,d,d\} \text{ <--- charge of n is } +2/3 -1/3 - 1/3 = 0$$

$$p = \{u,u,d\} \text{ <---- charge of p is } +2/3 + 2/3 - 1/3 = 1$$

c) Yuri has good reason to postulate existence of a massive stable "mirror aniquark," U^*. That elementary particle is rare; in comparison with common u and d quarks. On the average, only one U^* exists for every 10^{15} of common quarks. Combining with one common quark (either u or d), U^* becomes part of a neutral or a negative pair, as shown below. Such pairs are called erzions.

A neutral erzion E^o consists of U^* and u; it is bag of two quarks (a meson).

$$E^o = \{U^*,u\} \text{ <----charge of } U^* \text{ is } 0 - (2/3) = -2/3, \text{ equal but opposite of the charge of u}$$

A negative erzion E^-consists of U^* and d; it is a bag of two quarks (a meson)

$$E^- = \{U^*,d\} \text{<----charge of } E^- \text{ is } -(2/3) - 1/3 = -1, \text{ equal but opposite of the charge of p}$$

d) A negative erzion, according to Yuri, can form a bound with a proton. This produces a "bag of 5 quarks." The name of that structure is enion,

$$E_N. = \{U^*,u,u,d,d\}$$

The neutral enion can dissociate into a pair of particles, a negative erzion (charge -e) and a proton $\{E^-,p\}$ or a neutral erzion and a neutron $\{E^o,n\}$. In other words,

$$E_N \text{ --> } E^- + p + \Delta_1 \ldots \text{ or } \ldots E_N \text{ --> } E^o + n + \Delta_2$$

where Δ_1 and Δ_2 represent reaction energies. They are positive for exothermic reactions and negative for endothermic reactions.

e) Enions (E_N), negative erzions (E^-) and neutral erzions (E^o) are rare (because U^* are rare). But they can interact with common atomic nuclei, (A,Z).

15) For a given atomic nucleus, (A,Z) one can consider only six possible interactions: two with an enion, two a neutral erzion and two with a negative erzion. They are:

$$(A,Z) + E_N = (A+1,Z) + E^o + Q1 \text{ neutron is going from the enion, } E_N, \text{ into } (A,Z)$$
$$(A,Z) + E_N = (A+1,Z+1) + E^- + Q2 \text{ proton is going from the enion, } E_N, \text{ into } (A,Z)$$

$$(A,Z) + E^o = (A-1,Z) + E_N + Q3 \text{ neutron is going from the neutral erzion into } (A,Z)$$
$$(A,Z) + E^o = (A,Z+1) + E^- + Q4 \text{ proton is going from the neutral erzion into } (A,Z)$$

$$(A,Z) + E^- = (A-1,Z-1) + E_N + Q5 \text{ neutron is going from the negative erzion into } (A,Z)$$
$$(A,Z) + E^- = (A,Z+1) + E^o + Q6 \text{ proton is going from the negative erzion into } (A,Z)$$

where symbols Q1, Q2, Q3, Q4, Q5 and Q6 stand for energies released in six possible reactions. In the first two reactions the enion is changed into a neutral erzion while in the second the enion is changed into a negative erzion. In either case the unusual antiquarks, U^*, remain outside the newly formed nucleus. The same is true for the next two reactions (transformation of a neutral erzions into enion or into a negative erzion) The last two reactions (transformation of a negative erzion into an enion or into a neutral erzion) also do not consume the unusual antiquarks. These elementary particles always remains available to produce numerous reactions, one after another. That is why the U^* can be said to be a catalyst. It participates in reactions but it is not consumed in them.

16) Suppose the symbol A is used to describe the target nucleus (A,Z) and the symbol B is used to describe the product nucleus. Then the above six reactions can be described in the following way. The symbol Q represents the absolute value of the released nuclear energy (+ when the reaction is exothermic and - when it is endothermic).

$$A(E_N,E^o)B + Q$$
$$A(E_N,E^-)B + Q$$
$$A(E^o,E_N)B + Q$$
$$A(E^o,E^-)B + Q$$
$$A(E^-,E_N)B + Q$$
$$A(E^-,E^o)B + Q$$

Yuri Bazhutov would replace my Latin symbol E (see above) by the Cyrillic symbol (see below). What follows is a list of possible nuclear reactions (induced by enions and erzions) in light isotopes. Column 2 shows natural abundances of target nuclei while numbers in column 3 can be used to identify reactions.

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Table 1

^{1}H (Э-, Э^{0}) ^{0}n + 1,65 МэВ	(100 %)	(1)
^{2}H (Э-, Э$_{N}$) ^{0}n + 5,6 МэВ	(0,016 %)	(2)
^{2}H (Э$_{N}$, Э^{0}) ^{3}H + 0,1 МэВ	(0,016 %)	(3)
^{2}H (Э^{0}, Э^{N}) ^{1}H + 3,9 МэВ	(0,016 %)	(4)
Li^{6} (Э^{N}, Э^{0}) ^{7}Li + 1,1 МэВ	(7,5 %)	(5)
Li^{6} (Э^{0}, Э$_{N}$) ^{5}Li+ 0,5 МэВ	(7,5 %)	(6)
^{5}Li - > ^{4}He + ^{1}H + 1,7 МэВ		
Li^{6} (Э$^{-}$, Э$_{N}$) ^{5}He + 3,2 МэВ	(7,5 %)	(7)
^{5}He - > ^{4}He + ^{0}n + 1,36 МэВ		
^{7}Li (Э$_{N}$, Э$^{-}$) ^{8}Be + 9,5 МэВ	(92,5 %)	(8)
^{8}Be - > 2·^{4}He + 4,8 МэВ		
^{13}C (Э$_{N}$, Э^{0}) ^{14}C + 2,0 МэВ	(1,1 %)	(9)
^{13}C (Э^{0}, Э$_{N}$) ^{12}C + 1,2 МэВ	(1,1 %)	(10)
^{14}C (Э$_{N}$, Э$^{-}$) ^{15}N + 2,4 МэВ	(---)	(11)
^{14}N (Э$^{-}$, Э^{0}) ^{14}C + 2,3 МэВ	(99,6 %)	(12)
^{14}N (Э$^{-}$, Э$_{N}$) ^{13}C + 0,25 МэВ	(99,6 %)	(13)
^{14}N (Э$_{N}$, Э^{0}) ^{15}N + 4,7 МэВ	(99,6 %)	(14)
^{15}N (Э$_{N}$, Э$^{-}$) ^{16}O + 4,3 МэВ	(0,37 %)	(15)
^{17}O (Э$_{N}$, Э^{0}) ^{18}O + 1,9 МэВ	(0,038 %)	(16)
^{17}O (Э^{0}, Э$_{N}$) ^{16}O + 2,0 МэВ	(0,038 %)	(17)
^{18}O (Э$_{N}$, Э$^{-}$) ^{19}F + 0,2 МэВ	(0,2 %)	(18)
^{19}F (Э$_{N}$, Э^{0}) ^{20}F + 0,45 МэВ	(100%)	(19)
^{19}F (Э$_{N}$, Э$^{-}$) ^{20}Ne + 5,05 МэВ	(100 %)	(20)
^{23}Na (Э$_{N}$, Э^{0}) ^{24}Na + 0,8 МэВ	(100 %)	(21)
^{23}Na (Э$_{N}$, Э$^{-}$) ^{24}Mg + 3,9 МэВ	(100 %)	(22)
^{27}Al (Э$_{N}$, Э^{0}) ^{28}Al + 1,6 МэВ	(100 %)	(23)
^{27}Al (Э$_{N}$, Э$^{-}$) ^{28}Si + 3,8 МэВ	(100 %)	(24)

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17) Yuri thinks that some of these reactions might be responsible for production of neutrons in his experiments. He also thinks that formation of tracks in CR-39, such as those I reported in Catania, can be explained in terms of the above reactions.

18) After listing six possible reactions (see items 15 and 16 above), Bazhutov writes: “Enion, due its special quantum numbers, is strongly repelled by atomic nuclei. But, due to its electric dipole moment, it is also attracted to them.” At some distances these two tendencies cancel each other and a “stable long-lived state” can be formed, provided “the exothermic exchange reactions are absent. The bounding energy is about 1.5 eV for proton (Z=1) and about 60 eV for ^{208}Pb (Z=82). Atomic nuclei able to form bound states with enions (^{1}H, ^{4}He, ^{12}C, ^{16}O, ^{64}Ni, . . .) are called ‘donors’ .”

19) A little later, after mentioning the low concentration of “captured enions” (about one out of 10^{15} nuclei), Bazhutov continues: “When enions become free they react exothermically with atomic nuclei; the cross sections of such reactions are expected to be of several mega-barns. Sequences of reactions, as described above, are very fast, approaching billion per second, under the most favorable conditions. As usual, only exothermic reactions [see Table 1 above] are expected to occur, under ordinary temperatures. The table shows that exothermic reactions induced by negative erzions are relatively rare. The eleven stable isotopes, reacting with the negative erzion, are called converters. To produce a long sequence of nuclear reactions one must have a mixture of donor nuclei and converter nuclei. Deuterium happens to be the best converter. To produce neutrons one must have a mixture of hydrogen and lithium atoms.”

Appended on 1/30/08 What follows was not seen by Yuri Bazhutov. I will add his comments after receiving them.
20) To illustrate the idea of chains Yuri pointed to reactions 9 and 10 (see the above table). That was during our

Catania conversation. Note that C represents 1.1% of carbon atoms in a plastic material, such as CR-39. It is an example of a converter. An enion being neutral, approaches the ^{13}C nucleus, deposits a neutron into it (production of ^{14}C and a neutral erzion). That is reaction 9. The neutral erzion, created in that reaction, approaches another ^{13}C converter and triggers the reaction 10. That reaction is also exothermic; it results in the pickup of a neutron, from ^{13}C (creating ^{12}C and a neutral enion). This constitutes an element of a sequence of identical pairs of reactions. The sequence ends when the enion is either captured by a donor, such as ^{12}C, or escapes from the plastic material. The net result of reactions 9 and 10 is conversion of one ^{13}C into one ^{12}C, and release of 3.2 MeV of energy.

21) Similar long sequences of repetitive nuclear events can be based on reactions 16 and 17, or on reactions 3 and 4. To calculate Q values of reactions produced by enions and erzions one must know exact masses of these projectiles. How large are these masses? How were they determined? Not knowing the answers I can work backward, taking the posted energies (2.0 MeV and 1.2 MeV) for granted. Suppose the exact mass of an enion is X while the exact mass of the neutral erzion is Y. Then

$M13 + X = M14 + Y + Q9/c^2$ <-----(Q9=2.0 MeV in reaction 9)
$M13 + Y = M12 + X + Q10/c^2$ <--- (Q10=1.2 MeV in reaction 10)

where c is the speed of light. This is a set of two equations with two unknowns. Using the known nuclear masses (in atomic mass units, u):

M12 = 12.000000 u
M13 = 13.003354 u
M14 = 14.003074 u
1 u = 931.5 MeV (1 MeV = 0.001073537u)

22) This leads me to X=1.0017677 u and Y=0.000099343 u. My calculated rest mass on the enion is only slightly smaller than the rest mass of a proton.(1.0078252 u) while the calculated mass of the negative erzion is 5.4 times smaller than the rest mass of an electron, (0.0005389 u). The mass of the negative erzion can be calculated, for example, from the 5.6 MeV of reaction 2. Yes, I know that this is not the answer to my questions. The simple algebra tells me what enion and erzion masses were actually used to compose the table. What I want to know, however, is justification for these masses. Something is missing in my understanding of Yuri's explanations. What is it? My guess is that masses of erzions were assumed to be negligible while the mass of the enion was assumed to be 1.0017677 u, perhaps on the basis of some advanced QCD considerations.

23) I will try to learn more about quarks in the furure. And I will try to summarize what I learn. One thing is already clear; most quarkologists probably never heard about the U* antiquark. The accepted standard model quarks, and antiquarks, are listed at

http://en.wikipedia.org/wiki/Quark

Mainstream theoretical physicists probably treat the idea of another quark in the same way in which the idea of thermonuclear reactions at low temperatures is treated by mainstream nuclear physicists. In the final analysis, only experiments will show who is right and who is wrong.

Appended on 4/4/08 (to be posted later):
24) This is a message I posted at the Internet discussion list for CMNS researchers. " Some time ago Juri Bazhutov sent me a message saying that the last part of what I wrote above (appended on 1/30/08) has a mistake, and that rest masses of the enion and neutral erzion, are about 200 u each. (The u stands for what used to be abbreviated as amu (atomic mass unit); it is defined as 1/12 of the mass of C12. The mass of a proton, for example, is approximately one u. Note that E=m*c^2 becomes E=m*931.5, when E is in MeV and m is in u.)

Yuri is correct, I made a mistake; the mass of C14 should have been 14.003242, and not 14.003074. The mass I used

would be OK for N14 but not for C14. The very small print in the mass table was probably responsible for the mistake. I just corrected the mistake and recalculated the masses. But this did not bring me closer to Yuri's masses of approximately 200u. In fact, my values of Y is now negative.

How can this be interpreted? Masses of particles are always positive. My interpretation is that the suggested reactions 9 and 10 (From Yuri's table), are not consistent with masses attributed by him to the enion and to the neutral erzion. Please help me to bring clarity to this. I will update my webpage accordingly.

P.S.
Here are numerical calculations, where X is the mass of an enion and Y is the mass of an erzion)

Yuri's reaction 9:
13.003354 + X = 14.003242 + Y + 2/931.5 --> X + Y = 1.002035

Yuri's Reaction 10:
13.003354 + Y = 12.000000 + X + 1.2/931.5 --> X - Y = 1.002266

Solving these two equations with two unknowns I found that

X = 1.00215 u (rather than ~200 u)
and
Y= -0.000115 u (rather than ~200 u)

X is the rest mass of an enion and Y is the rest mass of a neutral erzion."

25) I calculated the X and the Y again but from another pair of reactions (reactions 16 and 17 in the same Yuri's table). The new values of X and Y turned out to be:

X=1.004219 u (rather than ~200 u). Thus is slightly larger than before.
and
Y = -0.002146 u (rather than ~200 u) The absolute value is 18.7 times larger than before.

That is strange; the masses of the enion and the erzion should be the same no matter which pair of reactions is used to calculate them from. Either what I am doing is wrong or reactions in Yuri's table are not consistent with each other. Once again X and Y are very different from ~200 u.

26) Suppose a totally different approach is taken to 24 exothermic reactions listed in Yuri's table. Let me rewrite four of them.

Reaction 9 ---> C13 + X = C14 +Y + 2.0 MeV
Reaction 10 ---> C13 + Y = C12 + X +1.2 MeV

Reaction 16 --->O17 + X = O18 + Y + 1.9 MeV
Reaction 17 ---> O17 + Y = O16 + X + 2.0 MeV

where X stands for the enion and Y stands for the neutral erzion, as before. Each of these reactions can be used to calculate the difference between the rest masses of X and Y.

According to reaction 9, the difference is
X-Y = C14 - C13 + 2/931.5 = 14.003242 - 13.003354 + 0.002147 = 1.002035 u

According to reaction 10, the difference is
X-Y = C13 - C12 - 1.2/931.5 = 13.003354 - 12.000000 - 0.001288 = 1.002066 u

According to reaction 16, the difference is
X-Y = O18 - O17 + 1.9/931.5 = 17.999160 - 16.999133 + 0.002040 = 1.002067

According to reaction 17, the difference is
X-Y = O17 - O16 - 2/931.5 = 16.999133 -15.994915 - 0.002147 = 1.002071

The differences obtained from the four reactions are nearly identical. The mass on the enion exceeds the mass of the neutral erzion by nearly one u. The results are consistent with X-Y values calculated yesterday. But yesterday I was able to calculate masses of X and Y, not only X-Y. Each mass was much smaller that ~200u. I am making no progress in trying to understand the situation.

27) On 3/24/08 Bill Collis wrote:
Ludwik I'm not sure I follow all your discussion about Erzions and Enions. One point I would make is that no data regarding reaction energies is going to give the absolute mass of any particle. Masses are relative. In the case of conventional nuclear reactions they are relative to C12. In the case of Erzions, we may arbitrarily choose the mass (or mass defect) of one of the three Erzions.

28) On 3/24/08 Ludwik wrote:
1) I am leaning on Yuri Bazhutov's Catania presentation available from

http://www.iscmns.org/catania07/program.htm

His paper can be download by scrolling down to the "Monday 15 October" and by clicking on the corresponding "pdf" link.

2) Yuri's paper lists 24 exothermic nuclear reactions. What were the masses of three exotic particles (enion and two erzions) that were used to calculate the Q values? I simply tried to answer this question. Each of Yuri's reaction gives an equation with two unknown. The three unknown masses -- I labeled them X, Y and Z -- can be found by solving three equations with three unknowns.

3) The values of X, Y and Z should be the same, no matter which three reactions are chosen to calculate them. And each mass should be positive. Right or wrong?

P.S. (after reading Bill's message):
OK, suppose the mass of X is declared to be 1.000000 by definition. Also suppose that Y and Z, according to a theory, happen to be one thousand times smaller, or larger, than X. How would you use this information to calculate the Q values of Yuri's 24 reactions? I think that this would be possible only if masses of X, Y, and Z were accurately expressed in familiar C12 units. I took Yuri's Q values for granted and calculated X, Y, and Z from them in C12 units. What was wrong with this?

29) On 3/25/08 Bill Collis wrote:
Ludwik, reaction energies, whether chemical or nuclear, are based on the energy difference between products and reactants. For conventional nuclear physics nucleons are treated as indivisible particles (we cannot weigh quarks) and we choose a single "anchor" to specify an absolute mass of C12.

This is not sufficient for Erzions because they cannot be specified in terms of nucleons alone. They cannot be converted into "normal" nucleons. Erzion Number is always conserved so hypothetical Erzion reaction energies can only give the difference between Erzion masses.

The first such difference, as proposed by Yuri, is the Erzion / Enion mass difference required to ensure low level tritium production without simultaneous 14.1 MeV neutron production:-

2H + zN --> 3H + z0

This reaction would have to be almost endothermic otherwise fast tritons would interact with deuterium. ;)

Similarly some reaction must be found to fix the negative Erzion mass. With these 2 parameters you can calculate any number of Erzion reactions.

Last year I presented a paper on this at ICCF13 using results from ENSAP software. If anyone would like a full list of ALL hypothetical Erzion reactions with naturally occurring isotopes I'll be happy to send it to them privately. For ICCF14, I'm working on a paper revising the Erzion masses with a view to further reducing the predicted radio-activity which Erzions would otherwise induce through interaction with ordinary matter. The goal is to explain heat and transmutation without lethal radiation, without Coulomb barrier using the ordinary laws of physics. That's quite difficult of course (but software helps!). If anyone would like to collaborate on this, just let me know.

30) On 3/25/08 Ludwik wrote (addressing Bill):
OK, Let us calculate the Q value (released energy) in the following
reaction

Na23 + A = Na24 + B

where A and B are exotic particles of some kind. The answer is Q = m*c^2, where m is the mass difference between the right side and the left side. I know the difference between the masses of two sodium isotopes. What I need is the mass difference between B and A, in the same units (such as kg or u). I assume your answer is Q=0.8 MeV, when A is an enion and B is the neutral erzion. Please show your arithmetic. Why do I assume that your answer is 0.8 MeV? Because that is what was reported by Yuri; it is reaction 21 in his Catania presentation at:

http://www.iscmns.org/catania07/program.htm

31) On 3/25/08 Bill Collis wrote:
Ludwik wrote: "OK, Let us calculate the Q value (released energy) in the following reaction

Na23 + A = Na24 + B

I assume your answer is Q=0.8 MeV, when A is an enion and B is the neutral erzion. Please show your arithmetic."

Following Yuri's logic and the reasoning given in my previous message, we can write:-

23Na + t = 24Na + d + 0.702 MeV

More precisely, in the case of Erzions being the catalytic intermediaries 2 reactions are possible:-

23Na+ EURO N -> EURO- + 24Mg +5.194 MeV
23Na+ EURO N -> EURO 0 + 24Na +0.708 MeV

The first reaction is favoured over the second by energy and spin considerations and may explain why we do not observe radio-active sodium in our experiments. Does this answer your question?

32) On 3/25/08 Ludwik wrote:
Bill wrote: "Does this answer your question? " Perhaps it does but I am still in the dark. My question was about the reaction

23Na + (enion) = 24Na + (neutral erzion) +Q

According to Yuri's table, Q for this reaction is 0.80 MeV. How was this calculated? Please describe assumptions behind Yuri's logic. Please show the arithmetic.
P.S.

I also got the Q=0.702 MeV for your first reaction. How can this number be used to calculate Q=0.80 MeV?
.
P.P.S.
In the previous message you wrote:

``. . . Erzion Number is always conserved so hypothetical Erzion reaction energies can only give the difference between Erzion masses. The first such difference, as proposed by Yuri, is the Erzion / Enion mass difference required to ensure low level tritium production without simultaneous 14.1 MeV neutron production:-

2H + zN --> 3H + z0

This reaction would have to be almost endothermic otherwise fast tritons would interact with deuterium. ;)

Similarly some reaction must be found to fix the negative Erzion mass. With these 2 parameters you can calculate any number of Erzion reactions..... "

a) What is the erzion number?
b) How much is ``almost" (in almost endothermic), and why?
c) The 23Na + t = 24Na + d + 0.702 MeV and the 23Na + (enion) = 24Na + (neutral erzion) +0.80 MeV reactions describe conversion of 23Na into 24Na (adding a neutron to 23Na). But why should the Q of the second reaction depend on the Q of the first reaction? I need a more detailed description of logic (and assumptions) used to calculate Q of reactions involving enions and erzions. Hints are not sufficient, at my zero-level familiarity with such particles.

33) On 3/27/08 Bill Collis wrote:
1) Ludwik wrote: "I am still in the dark. My question was about the reaction 23Na + (enion) = 24Na + (neutral erzion) +Q According to Yuri's table Q for this reaction is 0.80 MeV. How was this calculated?" I suggest you ask Yuri. It may be a misprint. My calculation, agrees with yours.

The assumptions [in Yuri's logic] are that endothermic nuclear reactions are impossible at room temperature and fast tritons would create unobserved 14.1 MeV neutrons in any heavily deuterated material.

2) [He also wrote] "Please show the arithmetic." See my previous posts.
3) [He also asked] "What is the erzion number? It's another way of saying that Erzions cannot be created nor destroyed.
4) [He also asked] "How much is ``almost" (in almost endothermic), and why? Almost means a few keV. The d+t reaction would be detectable if the tritons had more than about 10 keV.
5) [He also asked} "The 23Na + t = 24Na + d + 0.702 MeV and the 23Na + (enion) = 24Na + (neutral erzion) +0.80 MeV reactions describe conversion of 23Na into 24Na (adding a neutron to 23Na). But why should the Q of the second reaction depend on the Q of the first reaction?" The two Qs should be similar. As I say, this looks like an error. Or maybe Yuri is refining his model in some undocumented way. Ask him.

At this stage we can make many assumptions varying the two parameters which describe Erzion masses. When we find a pair which coherently explain CMNS observations, we may be closer to the truth. You don't have to be a nuclear expert to do this if you have the right software.

Ludwik (not posted):
At this stage I do not mind performing manual calculations. Software is useful in repetitive calculations. And I will wait for Yuri's input. I hope he is no longer sick, as he was last month. The idea that a mass cannot be associated with a particle participating in a reaction is not easy to swollaw. The answer to my question "what is the erzion mass number" does not add clarity. I wanted to know what physical quantity is represented by the erzion number EN. Suppose someone tells me that EN=3. How does this differ from EN=7, 13.5, or -2, etc. ?

34) Appended on 4/10/08 (to be posted later)
Two days ago I posted this message on the Internet list for CMNS researchers: "

Dear Bill, your slide #6, of Catania presentation, shows four reactions involving Yuri's erzions and enion. Let me use X for an enion, Y for the neutral erzion and Z for the negative erzion. With these labels your four reactions are:

Li6 + Z = X +He5 +3.48 MeV (A)
Li6 + Y = X + Li5 + 0.58 MeV (B)
Li6 + X = Y + Li7 + 0.99 MeV (C)
Li7 + X = Z + Be8 + 9.17 MeV (D)

Masses of nuclei involved in these reactions are know. They are:

5He --> 5.01222 u
5Li ---> 5.01254 u
6Li ---> 6.01512279 u
7Li ---> 7.01600455 u
8Be --> 8.00530510 u

The accuracy for the highly unstable 5He and 5Li (life-times of 10^-21 sec) are not as high as for the other three nuclei. But that is OK, I will take the Q values for granted. Suppose that x, y and z stand for masses of three exotic particles: enion, neutral erzion and negative erzion. Then, applying the energy conservation law to reactins (B) and (C) one has:

6.01512279 + y = x + 5.01254 + 0.58 / 931.5 (b)
6.01512279 + x = y + 7.01600455 + 0.99 / 931.5 (c)

1) Each of these equations tell me than the enion, X, is more massive (by about one mass unit) than the neutral erzion, Y. That is is an important conclusion. I suppose the Q values were artificially chosen to make the (x-y), calculated from (b) and (c), identical. Please confirm.

2) Let me do the same thing with equations (A) and (D).

6.01512279 + z = x + 5.01222 + 3.48 / 931.5 (a)
7.01600455 + x = z + 8.00530510 + 9.17/931.5 (d)

Each of these equations tell me than the enion, X, is more massive (by about one mass unit) than the negative erzion, Z. Once again, I suppose the Q values were artificially chosen to make the (x-y), calculated from (a) and (d), identical. The masses are knot known accurately enough for this to happen, except by accident. Please confirm.

3) In other words, erzions, neutral and negative, have about the same mass. Their masses are by about one C12 unit smaller than the mass on the enion.

4) According to one of Yuri's papers erion is a "single charged particle with mass -- M of about 200 GeV/c^2." If that number were exact then the mass of a negaitve erzion would be 200000 / 931.5 = 214.707461 u. In other words, the A would be 214 and the mass deffect would be 0.7074611 units. The value of A for the enion, according to my calculations above, would be close to 215. But the 200 GeV/c^2 seems to be only a very rough estimate based on cosmic-ray data. The bottom line is that erzions and enions are very massive particles. Enion and neutral erzion, like polyneutrons, are not repelled by atomic nuclei. That is a very important property.

5) Is my description acceptable? If not then what else should be added to introduce Yuri's particles to students and teachers?

References:
W. Collis at --- > http://www.iscmns.org/catania07/program.htm

Y. Bazhutov at ---> http://www.iscmns.org/catania07/program.htm

35) **Ed Storms wrote:**
This discussion is interesting, but I don't understand how it relates to LENR. Are these erzions proposed to be everywhere and can initiate nuclear reactions under certain circumstances? Why are they not easy to detect when material is vaporized and/or ionized? What keeps them from reacting at anytime in any material?

36) Bill Collis wrote:
"Ludwik your description looks OK but please remember we do not know what the absolute or relative masses of Erzions are. Erzions are a model system of neutron transfer catalysts. Another such model is John Fisher's polyneutron theory. The question at this time should be, "What properties should these catalysts have to explain CMNS observations".

Ed, yes Erzions are supposed to be everywhere! In most materials they do not react or react but once and so are never observed. More interesting effects occur in a limited range of composite materials, an Erzion picks up a neutron from one isotope and deposits it into another.

The neutral Erzion can react with a deuterium producing a proton and a heavier Enion. But the Enion cannot normally react further (but perhaps if it has a little more energy it can create tritium).

In pure Beryllium, Erzions should set up an exothermic chain reaction producing helium and radioactive 10Be! Can you see why? (Be9 can both accept and donate a neutron). The fact that nobody has detected 10Be suggests either they weren't looking or the model is wrong! (Fisher's initial polyneutron theory did not make this prediction.)

If Erzions are everywhere why are electrolytic cells good places to find them? I haven't really thought about this problem deeply, but maybe negative erzions somehow get trapped there and so accumulate."

37) Responding to Bill, Ludwik wrote (also on the private CMNS list**):**
``Your statement about not knowing `the absolute or relative masses of erzions' still troubles me. I have three concerns

1) Consider reaction 9 from Yuri's Catania paper:

X + 13C = 14C + Y + 2.0 MeV

This implies that

(x-y) = 14Cmass - 13Cmass) +2.0 / 931.5 =1.002034 u. <------ (E)

This does give the absolute difference between the masses of two exotic particles. If the value of (x-y) is not known then the Q of the reaction is also not known. But the Q=2 MeV is given. How was that Q determined?

2) Or consider reaction 10, also from Yuri's Catania paper:

Y +13C = 12C +X +1.2 MeV

This implies that

(y-x) = 12Cmass - 13Cmass) + 1.2 / 931.5 = -1.002066 u <----- (F)

If the value of (y-x) is not known then the Q of the reaction is also not known. But the Q=1.2 MeV is given. How was that Q determined?

3) Suppose I do accept the fact that the Q of each of these reactions is given. Then what is wrong with treating the (E) and (F) as two equations with two unknown? That would give me exact values of x and y. In other words, specifying

Q of reactions, induced by exotic erzions and enions, does not seem to be consistent with the statement that "absolute masses" are not known. On what basis were the very accurate value of Q chosen? Some assumptions were probably made. Please explain these arbitrary assumptions.''

Appended on 4/12/08

38) Responding to Ed, Bill wrote:

The beauty of catalysed neutron transfer reactions is that only a tiny number of catalyst entities need be present to make measurable excess heat. Being (mostly) neutral, their reaction cross sections are measured in Mega-barns because no gamma-ray emission is necessary to conserve energy (in contrast to neutron capture). This enhancement of about 1 million is due the fact that the nuclear force is 6 or 7 orders of magnitude stronger than the electromagnetic force and correspondingly faster. In turn this means that the mean free path of such neutral catalysts is about 1 millionth of that of a neutron. Consequently a 3 dimensional CMNS reactor may be 10**18 times smaller than a fission reactor! Instead of weighing 10**12 g scales are reduced to tiny hot-spots of 10**-6 g. Indeed individual hot spots may be caused by single catalytic particles. Of course these figures are all VERY approximate.

No let's see how these particles catalyze neutron transfer. In the case
of Erzions we may have simple cycles such as:-

EURO N + 6Li-> EURO 0 + 7Li +0.99 MeV
EURO 0 + 6Li-> EURO N + 5Li +0.58 MeV
5Li -> 4He + p +1.966 MeV

As you can see Erzions are regenerated and we get lots of He4 and protons (but no X-rays or gammas). However we all know that bulk Lithium alone does not get hot! We must explain why. One possibility is:-

EURO N + 7Li-> EURO- + 4He + 4He +10.85 MeV

Once the negatively charged Erzion is formed its reactivity is dramatically reduced (but not quite eliminated). Of course the situation is much more complicated in reality because our hypothetical catalysts may react with other isotopes too. Furthermore we do not know their masses (and therefore their reactivity). We need to make reasonable assumptions, test them against reality, and refine the parameters. I am making some progress in this direction and hopefully I'll report at ICCF14.

So how many catalytic neutral particles would you need to make say 1 watt? Dozens? Hundreds? So few that they would be undetectable by density or physical properties.

39) Responding to Bill, Ed wrote:

I agree, some kind of semi-neutral catalyst is required. Apparently, we now have hydrinos, Erzions, and polyneutrons to consider. Such a catalyst must be made either by an exothermic reaction, as with hydrinos, or be supplied fully formed by nature, as is the case with the latter two possibilities. I think we agree, creation of a catalyst, such as neutrons, by a endothermic process is not possible. How do we evaluate each of these possibilities?

As an answer, I suggest we need to consider two questions. What conditions make each catalyst reactive and what are the expected products. Both questions can be answered by experimental observation. In addition, these answers have to be provided with a minimum of arbitrary assumptions that can not be verified. At the present time, we already have some of these answers.

In the case of polyneutrons and Erzions, we need to assume that they are more or less uniformly distributed in nature. Therefore, the trigger that initiates a nuclear reaction must be created by a unique chemical or physical environment. Next, we need to ask why this environment is so rare and difficult to create. Why do the few environments used by CF researchers work and most other environments do not work? This question is easier to answer about hydrinos. This nuclear catalyst requires a chemical catalyst to form. Therefore, nuclear reactions can occur only when this rare chemical catalyst is present. Nevertheless, I find one fact rather troubling. In all of nature, why are these possible nuclear reacts not being catalyzed all the time in various opportune circumstances? The result would be an isotopic

distribution in nature that would be highly distorted, favoring the reaction produces being proposed by the various proponents of the different catalysts.

I suggest we need to discuss these broader issues rather than debating hypothetical reactions that might never occur. We know that certain nuclear products are produced. I suggest we need to discover why and how these products are formed. Other products might be produced, but until they are verified, their formation is simply an assumption having no value. Meanwhile, we know that tritium and helium result from the CF reaction. How are these elements formed and what are the required conditions? Why have these conditions not converted all of the deuterium to tritium then to He3 or to He4 during the life time of the universe? This question is easier to answer for hydrinos than for the other possibilities.

40) Responding to Ludwik, Bill wrote:
I answered your questions already, but here goes again! To calculate Q values for Erzion reactions we need to assign 2 parameters for mass differences between the 3 Erzions. In the case of the 2 reactions you cite above which do not involve the negative Erzion only 1 parameter is required. It is fixed (in Yuri's view) by the non observation of 14.1 MeV neutrons in tritium producing deuterated which implies in turn the absence of fast tritons from the reaction:-

$$X + d = Y + t$$

If the above reaction has an energy close to zero, the Q for reactions 1 & 2 can be calculated. Is this clear now? John Fisher used the same reasoning, quite independently, to fix poly neutron masses. However I am not convinced that this method is appropriate. After all, tritium has been produced in light hydrogen systems too.

41) Responding to Bill, Ludwik wrote:
Thanks for being patient with my slow learning. Let me follow your hint. I will continue using X, Y and Z for neutral enions, neutral erzions and negative erzions, resperctively (and x, y, z for their unspecified masses)

1) Let me try to calculate the Q value for the

$X + {}^{13}C = {}^{14}C + Y + Q1$ ---------- (9) Yuri's reaction #9

I want to verify that the answer is 2.0 MeV, as given by Yuri. You suggested I lean on the zero Q for the

$X + {}^{2}H = Y + {}^{3}H$ ---------- (0) Bill's reaction (assuming Q=0; to impose absence of 14.1 MeV neutrons)

This gives me two equations:

$x + 13.003354 = y + 14.0032419 + Q1$ -------- (9')
$x + 2.0141022 = y + 3.0160494$ ---------- (0')

The first equation gives me

$x-y = 0.999888 + Q1$

The second equation gives me

$x - y = 1.001947$

In other words,

0.999888 + Q1 = 1.001947 or Q1 = -0.0020592 u = 1.92 MeV

That is indeed close enough to Yuri's value 2.0 MeV; I am using masses from an old table. Let me do this again for Yuri's reaction #10.

Y +13C = 12C +X +Q2 --------- (10)

x-y = 13.003354 12.000000 - Q2 = 1.003354 - Q2------- (10')
x-y = 1.001947

In other words,
1.003354 - Q2 = 1.001947
or Q2 = 0.001407 u = 1.31 MeV.

This is again close enough to Yuri's value of 1.2 MeV. He probably used more accurate masses. Things become clear in my mind. Thanks for your help, Bill.

2) But one thing remains to be clarified. What is the second independent assumption? It must known to calculate the Q values of reactions involving a negative erzion, Z.
P.S.
3) I wander why Yuri is silent. In the last private message, about three weeks ago, he wrote that he was recovering from being sick. How are you now, Yuri?

42) Bill Collis (responding to Ed) wrote (on 4/19/08):
1) The questions you pose are relevant, but in my view they may not be the primary questions to resolve the issue! The first requirement is that any theory of CMNS explains (most of) the generally accepted observations. The second is that the theory must not make predictions which are not observed. Many theorists conveniently overlook this second requirement.

2) My approach is to assume some (not too many!) parameters (1 or 2 masses in the case of Erzions) and see what happens to their interactions with ALL known natural isotopes. I refine the parameters as needs be. It seems that all interactions of the E0 Erzion produce no penetrating radiation whatsoever! That's a miracle that makes me think the theory is worth looking into in more detail. Every theory I know purporting to "explain" heavy element transmutation fails at this hurdle.

3) In the early 90s there were many reports (Notoya, Bush & Eagleton etc.) of apparent transmutations by exactly 1 nucleon. Again, that is something expected by polyneutron or Erzion catalysis. I don't pretend for a moment that these models explain everything. I merely suggest that they are very testable, and therefore worthy of study.

4) If you want to analyse an extremely complicated series of transmutations it may be well to concentrate on very simple systems first before applying ones attention the generality of all CMNS experiments. For example how many theories attempt to explain the emission of neutrons from cold deuterium gas in a magnetic field (Mizuno)? Could it not be that our hypothetical catalysts have a magnetic moment and are trapped in a magnetic field? Could this also explain the successes of Letts, Piantelli and others?

5) I am not convinced that the Nuclear Active Site is a chemical catalyst. Rather, it may be an environment where specific nuclides permit and participate in a self sustaining series of reactions. This unusual combination of nuclides does not occur in nature and so CMNS reactions are not observable in natural systems.

6) You ask "Why have these conditions not converted all of the deuterium to tritium then to He3 or to He4 during the life time of the universe?". In order to achieve a sustained chain reaction the Erzion model requires:-

a) The presence of neutron donors like deuterium, Li6, Be9, C13, O17

b) The presence of neutron acceptor nuclei such as He3, Li6, B10.
c) The absence of negative Erzion precursors such as Li7, Be9, B10, N.
d) Some Erzions!

Of course what counts as an acceptor, donor or poison depends critically on the mass differences we assign to the Erzions themselves. This is why it is so important to discuss "hypothetical reactions that ... never occur" because they help us refine the model.

7) If you look at Physics as a whole, and at Cold Fusion in particular it is littered with untestable (and therefore unscientific) conjectures and models which barely survive because although useless (they make no predictions they cannot guide experiment), they cannot be demolished either. We are left with a set of models which are wrong in detail but leave open the possibility of improving that detail, and suggesting new experiments to perform. If this is unsatisfactory, it is nevertheless common reality of most science at the frontiers of knowledge.

8) I do not believe that hydrinos or mini-molecules explain CMNS. They might explain excess heat but not the observed nuclear transmutations. They predict unobserved nuclear reactions such as p+d->3He+gamma.

9) As you mention the Universe, consider this. Venus, Mars and Earth (in that order) have a higher D/H ratio than the interstellar medium which in turn has a higher ratio than the giant planets, Jupiter, Saturn, Uranus, Neptune. These last 4 radiate more energy than they receive from the Sun. Could that excess heat be cold fusion? If so one might presume that the power was greater in the past when deuterium concentrations were higher (assuming deuterium is the fuel and is being consumed). Well the 2 inner satellites of Jupiter have no atmosphere. Were these atmosphere blown away by nuclear heat some 5 billion years ago?

43) Ed Storms wrote:
[Referring to 1] I agree totally, Bill. However, I would add several other requirements. The theory must not violate any basic law and must show why the phenomena is so rare and difficult to initiate. In addition, a theory must explain how the Coulomb barrier is overcome and how the resulting energy is communicated to the observed world. These mechanisms must have universal application and not be uniquely available to the CF environment. Therefore, any proposed mechanism must be observed to be part of other phenomena besides CF. As you say, most theories overlook these requirements as well.

[Referring to 2] I agree, this hurdle is worth exploring. If no radiation is produced, how is the energy converted to heat? Can the resulted Erzion be detected? If not, the transmutation reactions would result in no detectable heat because all energy would go off with the invisible Erzion. How does the Erzion model fit with the Russian gas discharge work where heat and radiation are claimed?

[Referring to 3] Study is always worthwhile. However, there is always a competition between the time it takes for the study to be made and its potential success. If the model is too far from reality, potential success is too low to make the study worthwhile. We see this problem in most of the suggested theories. As you argue, it may be too early to render a judgment about Erzions.

[Referring to 4] Here, we get to the issue of what needs to be explained. Personally, I see no need to explain a single, unreplicated experiment, no matter how unusual the result. I suggest the effort should be directed to explaining the well known and well documented behavior. We have a lot of such information without being distracted by what could be a wild goose chase.

[Referring to 5] I agree, the NAE could take many forms. However, we have found a few universal features to be present. For example, oxygen can always be found in the environment. The local deuterium concentration is high, but not because it is dissolved in a metal lattice. I could suggest other less well documented features, but will avoid getting into the resulting debate, which is a subject for another time.

[Referring to 6]
The model, as I understand it, exploits the energy gain that results when neutrons are added to certain atoms resulting

in an unstable isotope. In addition, the Erzion also has to change mass for this process to be exothermic. My question is, What features about the environment must be unique to permit such reactions to occur? Why would a Erzion not immediately react with, say 6Li, just as soon as it has made contact? Some additional condition must be present to make such a reaction very rare. This condition is the "secret" that would make such a reaction reproducible once it is known and can be created at will.

[Referring to 7] True, but some theories are easier than others to improve. Some are so far from meeting the conditions we both agree are necessary that they are worth very little time. I suggest it is worthwhile to weed out such theories early in the discussion to avoid wasting a lot of time.

[Referring to 8]Yes, this reaction would be expected. However, the gamma may be eliminated by the same process that operates during the d + d = He4 reaction. To fit the hydrino model, we have to make an assumption about why this reaction does not occur. Making similar assumptions is not an uncommon requirement even in the Erzion model.:-)

[Referring to 9] I agree, cold fusion probably operates in such environments. Too bad we can't create a Jupiter atmosphere here on earth.

44) Bill Collis wrote:
{Bill, earlier] The questions you pose are relevant, but in my view they may not be the primary questions to resolve the issue! The first requirement is that any theory of CMNS explains (most of) the generally accepted observations. The second is that the theory must not make predictions which are not observed. Many theorists conveniently overlook this second requirement.

[Ed] I agree totally, Bill. However, I would add several other requirements. The theory must not violate any basic law and must show why the phenomena is so rare and difficult to initiate. In addition, a theory must explain how the Coulomb barrier is overcome and how the resulting energy is communicated to the observed world. These mechanisms must have universal application and not be uniquely available to the CF environment. Therefore, any proposed mechanism must be observed to be part of other phenomena besides CF. As you say, most theories overlook these requirements as well.

[Bill] We agree completely!

[Bill, earlier] My approach is to assume some (not too many!) parameters (1 or 2 masses in the case of Erzions) and see what happens to their interactions with ALL known natural isotopes. I refine the parameters as needs be. It seems that all interactions of the E0 Erzion produce no penetrating radiation whatsoever! That's a miracle that makes me think the theory is worth looking into in more detail. Every theory I know purporting to "explain" heavy element transmutation fails at this hurdle.

[Ed] I agree, this hurdle is worth exploring. If no radiation is produced, how is the energy converted to heat?

[Bill] When heavy charged fragments such as alpha particles or fission fragments pass through condensed matter, they lose energy principally by interaction with electrons. Because electrons weigh so much less (say 8000 times less in the case of alphas) they can only take a tiny proportion of the energy away. So we would expect heat to be removed by energetic electrons in the 100s-1000s eV range. There would be corresponding soft X-rays too with very little penetrative power.

[Ed] Can the resulted Erzion be detected?

It would be wise to determine its properties before attempting detection. He3 neutron counters might work.

[Ed] If not, the transmutation reactions would result in no detectable heat because all energy would go off with the invisible Erzion.

No. The Erzion is not so light and may be charged itself. Its mean free path in dense matter is of the order of microns

as I have already discussed. We expect "hot spots".

[Ed] How does the Erzion model fit with the Russian gas discharge work where heat and radiation are claimed?

See above.

[Bill, earlier] In the early 90s there were many reports (Notoya, Bush & Eagleton etc.) of apparent transmutations by exactly 1 nucleon. Again, that is something expected by polyneutron or Erzion catalysis. I don't pretend for a moment that these models explain everything. I merely suggest that they are very testable, and therefore worthy of study.

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[Bill, earlier] If you want to analyse an extremely complicated series of transmutations it may be well to concentrate on very simple systems first before applying ones attention the generality of all CMNS experiments. For example how many theories attempt to explain the emission of neutrons from cold deuterium gas in a magnetic field (Mizuno)? Could it not be that our hypothetical catalysts have a magnetic moment and are trapped in a magnetic field? Could this also explain the successes of Letts, Piantelli and others?

[Ed] Here, we get to the issue of what needs to be explained. Personally, I see no need to explain a single, unreplicated experiment, no matter how unusual the result.

Agreed.

[Ed] I suggest the effort should be directed to explaining the well known and well documented behavior. We have a lot of such information without being distracted by what could be a wild goose chase.

True.

[Bill, earlier] I am not convinced that the Nuclear Active Site is a chemical catalyst. Rather, it may be an environment where specific nuclides permit and participate in a self sustaining series of reactions. This unusual combination of nuclides does not occur in nature and so CMNS reactions are not observable in natural systems.

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[Bill, earlier] You ask "Why have these conditions not converted all of the deuterium to tritium then to He3 or to He4 during the life time of the universe?". In order to achieve a sustained chain reaction the Erzion model requires:-
1) The presence of neutron donors like deuterium, Li6, Be9, C13, O17
2) The presence of neutron acceptor nuclei such as He3, Li6, B10.
3) The absence of negative Erzion precursors such as Li7, Be9, B10, N.
4) Some Erzions!

Of course what counts as an acceptor, donor or poison depends critically on the mass differences we assign to the Erzions themselves. This is why it is so important to discuss "hypothetical reactions that ... never occur" because they help us refine the model.

[Ed] The model, as I understand it, exploits the energy gain that results when neutrons are added to certain atoms resulting in an unstable isotope.

No. If the products were unstable, we would have penetrating radiation in most cases - Bremstrahlung in the case of beta decay. Such radiation would be fatal (and not just to the theory!). We need to show, without any magic, that stable products are the only ones possible from stable precursors.

[Ed] In addition, the Erzion also has to change mass for this process to be exothermic. My question is, What features about the environment must be unique to permit such reactions to occur? Why would a Erzion not immediately react with, say 6Li, just as soon as it has made contact?

It is expected to.

[Ed] Some additional condition must be present to make such a reaction very rare.

Why? The trouble with pure lithium is that it generates negative Erzions which being charged cannot move in condensed matter. They have a half life of at least seconds so nuclear catalysis is slowed by at least 9 orders of magnitude. However if the layer of lithium were very thin, as it may well be on the surface of an active cathode, the energy of the reaction might be sufficient to send it somewhere else (such as the electrolyte)

[Ed] This condition is the "secret" that would make such a reaction reproducible once it is known and can be created at will.

When you find it, I'll come to Stockholm!

[Bill, earlier] If you look at Physics as a whole, and at Cold Fusion in particular it is littered with untestable (and therefore unscientific) conjectures and models which barely survive because although useless (they make no predictions they cannot guide experiment), they cannot be demolished either. We are left with a set of models which are wrong in detail but leave open the possibility of improving that detail, and suggesting new experiments to perform. If this is unsatisfactory, it is nevertheless common reality of most science at the frontiers of knowledge.

[Ed] True, but some theories are easier than others to improve. Some are so far from meeting the conditions we both agree are necessary that they are worth very little time. I suggest it is worthwhile to weed out such theories early in the discussion to avoid wasting a lot of time.

It would be nice if someone would write a critical review paper for JCMNS. A good starting point would be Preparata's review published in Fusion Technology.

[Bill earlier] I do not believe that hydrinos or mini-molecules explain CMNS. They might explain excess heat but not the observed nuclear transmutations. They predict unobserved nuclear reactions such as p+d->3He+gamma.

[Ed] Yes, this reaction would be expected. However, the gamma may be eliminated by the same process that operates during the d + d = He4 reaction. To fit the hydrino model, we have to make an assumption about why this reaction does not occur. Making similar assumptions is not an uncommon requirement even in the Erzion model.:-)

Not really. Assuming mass or energies is quite different from assuming new physics. What assumption do you make with the hydrino model? Is there a paper anywhere which discusses CMNS in terms of hydrinos? I ask because Mills himself is said not to believe in "Cold Fusion". Dufour has abandoned his hydrex / deutex model.

Ludwik:
It is evening of 4/21/2008. I just stoped appending messages to this unit. The last two messages (from Ed and Bill) were even more convoluted than the last one. I decided not to record them here. I have to stop somewhere; this unit is already too long. Then I uploaded the expended unit 341 to my website. That should not prevent me from being interested in what is going on. I might decide to compose another unit abou erzions at some later time.

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342) Speculations or truth?

Montclair State University, Montclair, NJ, 07055
1/16/08

1) The private Internet discussion list for CMNS researchers has been quiet in recent weeks. Then, on 1/12/08 an interesting message was posted by Ivan Ragland. Two days later another speculative message was sent to me in private. This shows that people are thinking about the ongoing CMNS controversy. These contributions are worth posting here. Perhaps someone will conceive and perform experiments to test the ideas. **Evan wrote:**

2) "Dear CMNS group, While things are quiet on the CMNS circuit it seems opportune to suggest an alternative structure of matter which might lead to new insight of cold fusion phenomena. I hesitate to make such introduction as no cold fusion theory is clearly established. However, it was cold fusion which caused me, *in the spirit of empirical equivalence*, to originally present an Alternate Model to Standard Model interpretation of the nuclear strong force. I have written extensively on the structure of matter and have two published papers on the subject. Recently, a third paper is accepted for presentation at the March Meeting of the American Physical Society. The abstract is appended herewith. In preparing for this meeting I solicit and welcome advice, interest, and questions of the CMNS group.

I am elderly, 81, a retired engineer, not a physicist, mathematician, or theorist. After a long career in computer design my interest is nuclear fusion. I bring to technology, success in things that, "*cannot be done*," and strong technical opinion. The Standard Model is a tragic wrong theory which for decades has failed the most prestigious of all sciences, high energy physics; string theory, nuclear fusion, M theory, super symmetry, TOE, ITER, NIF, and so on fail, while *all* sciences, solid state physics biology, medicine, communications, chemistry, computers, space, etc. flourish. Your advice, interest, and questions are appreciated." What follows was attached to the message:

3) "Alternate atomic model posits concentric electron and nucleon fields spinning together about an empty center. It is alternative to the generally accepted planetary system in which electron point particles orbit about a center clump of nucleon point particles. Introduced in 1992 as an alternative to the standard model of the nucleus it applies scientific space-time knowledge unknown when the standard model was conceived. Originally advanced in the spirit of alternative equivalence it evolved to model the entire atomic structure plus many features of space-time. Structural definitions assume space-time properties of: unidirectional expansion, special relativity, electrical field, magnetic field, spin field, gravity field, and space-time surface effect. Field effects are associated with Faraday lines of field force. Model properties feature symmetry and complementarily. Mass structures of the electron, proton,neutron, and protium atom plus the atomic and nuclear constituencies of all elements are developed. In addition the nuclear strong force is defined, the magnetic anomaly explained, etc. Model logic constructs the neutron as the complement of the hydrogen (protium) atom."

4) **My reply was**: "OK. I will tell you what I think.
a) I suspect that many people on this list, myself included, know the name "standard model" but never studied it, or used it as a tool to solve problems. In other words, most of us are not qualified to discuss this topic. If you want to criticize standard model, and offer a better alternative, then you should find a list with people prepared to deal with your ideas.

b) On the other hand, if you want to propose a new theory of atomic structure, starting from scratch, then this list is OK; it has a lot of people able to face your ideas. I would certainly try to follow you, at least as long as arguments are

scientific, and match my personal background. The rules of the game -- how to justify new statements in terms of what is already accepted -- are well known. In that way the CMNS field is not different from other fields of physical science.

c) Is your theory based on experimental facts or is it based on ad hoc assumptions? Please describe it briefly. In other words, you have to make the first move. Then wait for questions and comments about your theory. By the way, you will not be the first one to do this here.

5) **Two days later Evan wrote:**
"Thanks for your response. It is very helpful to me to know your thoughts. [a] I suspect you are right, many may know the name but have not studied or used the "standard model." It is my experience most scientists avoid 'standard model' complexity because they feel lack of expertise. Dr. Johnathan Allday writes, 'SM calculations are beyond us; [thus].simplifying assumptions have to be made." The AM is simple classical physics. I append descriptive material for both SM and AM with this reply.

[b] I do want to propose a new theory. I am sure you follow my reasoning and do value your expertise and comments. Also, I have confidence and respect for others on this list. Fortunately, young scientists are joining, plus, I hold highest professional regard for other scientists I know and have worked with since 1992. The rules are clear and, while my AM proposal may be controversial, it 's correctness can rescue physical agendas.

[c] It is based on facts, experiment, and assumption. The appended list of these is basis for evaluation of my written paper for the March meeting. My presentation is directed to the "*now"* of space-time. A description of the AM follows. Also, the paragraph entitled the crucial issue in the attached PDF is worth review. CMNS commentary and critique would be of value to me but I would not want to reflect any embarrassment upon the group. On the other hand CMNS participation might be worthy of consideration. I hope all this helps. Regards, Evan Ragland."

6) What follows is the attached piece:
"The logic of the Alternate Model constructs the neutron as the complement of the hydrogen (protium) atom;i.e., an electron captured inside a proton. The neutron's resultant internal field is the nuclear strong force. According to this model the nucleus is a concentricity of individual proton and neutron fields, each confined to a distinct shell location by wave length and angular and spin momenta. The structure conforms to the Pauli exclusion principle, the principles of quantum mechanics, and with the exception of the protium nucleus is bound together by neutron(s) internal field(s). The Alternate Model is plausible and easily visualized.

Nuclear Notes on March paper
01. electrons don't orbit; they ***nest***
02. nuclei don't clump; they ***nest***
03. electron & proton are fundamental; (soliton field energy packets)
04. protons ***don't*** decay; (probability 10^30 doesn't happen).
05. space is expanding: (in time)
06; space is a surface function; (as we know it)
07. ***spin*** is a property of space; (spin up, spin down)
08. space is a ***bubble***; (closes on itself, has no boundaries)
09. time has three states; (past, ***now***, and future)
10. ***now***, is a ***moment***; (between past & future)
11. the ***now*** of space-time is **C/r**; (**r** is radius of the electron & proton)
12. **E = mC2** is equivalence of energy (**E**) and mass (**m**)
13. decay from **C/2** is **e** (-e, frozen energy)
14. fill of **C/2** is **p** (+e, frozen energy)
15. **h** = e + p of like spins
16. **n** = e + p of unlike spins
17. **d** = p + n
18. **t** = d + n

19. **t** decays into **he3**
29. **he4** = **he3** + **n**
21. ***black holes*** are singularities “

7) My reply, on 1/14/2008:
You wrote that "the electron in spinning inside a proton." How can this be reconciled with HUP (Heizenbeg Uncertainty Principle, dx*dp~h). For a proton the dx is ~ 1 F. Introductory nuclear physics textbooks often use sizes of a nuclei (several F), and HUP, to explain why an electron cannot exist inside an atomic nucleus. Hydrogen is a small neutral atom and its electron is known to be outside the central nucleus.

8) Next day a carbon copy of another speculative message was sent to me by an old acquaintance (see item 268).
Dean Sinclair wrote: “ I've been out of contact for some time; but, thought you-all might find the following little essay which, I submitted to Helium.com as a "Debate" subject today, a little bit interesting. Can molecules be changed to atoms?

Yes. Under the right circumstances there is no reason why simple molecules cannot be transformed into atoms. Atoms combine into molecules and when one figures out the energetics, it can be shown that in many cases the atom version should be more stable than the molecule. Let us look at one case which is best explained by a molecule to atom transformation.

In 1998, two scientists, Stanley Fleischmann and Marvin Pons, announced that when they had carried out an electrolysis with a Palladium electrode and "Heavy Water," there was energy produced within the Palladium electrode, to the point, in one case of melting the electrode down. They ascribed this energy production to the fusion of Deuterium atoms to Helium 4 atoms, and called the process, "Cold Fusion."

The scientific community "went ballistic" at the term, "fusion." It is an item of faith in the scientific community that "fusion" can only take place at extremely high temperatures and pressures such as are found in the center of the Sun. In the almost twenty years that have passed since then a number of other workers have shown that the phenomenon reported by Fleischmann and Pons is real and have also reported other results of similar types. However, most of the establishment considers the area as "Kook science," rather than as a frontier between molecular and nuclear science where it probably should be considered. Unfortunately, explanations given for the phenomenon range from the fanciful to the bizarre.

The problems with this whole area stem from the separation of nuclear chemistry from molecular chemistry which started with Chadwick's discovery of the neutron in the 1930's. and the "romance" that enshrined as a fundamental particle of nature, an essential unit of every nucleus except of that of Hydrogen 1, This choice of the neutron as a basic building block of nature was in spite of the fact that it is energetically unstable to both the electron and proton as free entities and the Hydrogen 1 atom which can be considered as isomeric to it. The neutron was not given its proper place as an alternate state of matter which can come into existence under certain high-energy conditions. As a consequence of the neutrons "mis-placement" there began the high energy particle physics field searching for other "fundamental particles," which could be found by "atom-smashing." For some seventy years these scientists have discovered more and more "fundamental particles," which. like the neutron, should be properly classified as "alternate, high-energy states of matter.

All of this has led to such inanities as the Standard Model based upon these sets of "fundamental particles. " and the ideas of "Strong and Weak Nuclear Forces." The latter are ideas that arise directly from the ascribing to neutrons their existence as such in the nuclei of atoms. If. instead of adopting the neutron as a "fundamental particle," which had to be accounted for as such in nuclei, the neutron had been given its proper place and nuclei had been considered in the same way as molecules are, that is, as centers bonded by electrons moving in paths within and among them, perhaps we would have had a much more productive 70 years.

The point of all this is that. for the Fleischmann-Pons observations, there is a very simple probable explanation if one notes that both the Deuterium molecule, D:D, and the Helium 4 atom, He4, can be properly considered as being composed of four protons bonded by four electrons. Even the shapes are somewhat similar. The D:D unit can be

visualized as a "stretched tetrahedron," and the He4 nucleus as compact, perfect tetrahedron. The latter could be an instantaneous shape of a vibrational mode of the former. If there is a "receptor" available to drain off the excess vibrational energy of the "D:D form," the more stable "atom" structure can be taken on. Palladium with its plethora of energy levels is an ideal receptor for the excess energy of vibration.

What Fleischman and Pons almost surely observed was what could be called an "Iso-set isomerization" between the molecular and atomic forms of a set of protons and electrons. There are many such possibilities, some of which have apparently been observed in other "Cold Fusion" type experiments. Surely molecules can be transformed to atoms under the proper conditions. It's an area that should be wide open to open-minded scientists."

9) From Evan to Ludwik (1/16/08):
"I do not think it can be reconciled with the Uncertainty Principle as there is no action between the proton and electron constituents of the AM neutron. They spin together in a common wave packet about a common center.

Nuclear Physics by Irving Kaplan (pages 149-152) uses the UP to show a *free* electron in the neutron would approach velocity near 0.999 C which is contrary to experimental observations of emitted electron kinetic energies. Kaplan also suggests other reasons questioning the electron/proton hypothesis. The E/P hypothesis posited the electron constituent was *free* to *orbit* or actively relate to the proton constituent in the neutron. The AM holds the electron and proton are fundamental, electrostatic wave packets which spin as one in electron spin attitude.

The AM was conceived as a nuclear structure to keep protons from flying apart and neutrons from wandering away. It evolved from fundamental electron and proton structural definitions and their constituencies, the complementary protium (hydrogen) atom and neutron. It was never intended to revive the E/P hypothesis. Included herewith is additional information which may help explain the AM."

10) I do not want to argue about theoretical topics. But I must reply to Evan; his message was sent to me only. Both sets of ideas might be valid. Unfortunately, neither Dean nor Evan suggest experiments that could be performed to test the ideas. I am not prepared to speculate about their ideas.

11) Ludwik to Evan (1/17/08):
My theoretical background is too elementary to venture in that direction. It is safe to stick to what I learned from textbooks. Textbooks say that the Uncertainty Principle should be used to distinguish between what is possible from what is not. And "possible" does not mean "real." That is why the last word belongs to reproducible experimental data. It would be natural to abandon that principle, or to restrict its universality, to account for new experimental data.

Can beta radioactivity of neutrons (T~10 min) be used as experimental evidence that electrons live in them? Can beta radioactivity of tritium (T~12 y) be used as evidence that electrons exist in it? Can beta radioactivity of U-239 (T~23 min) be used as evidence that electrons exist in these nuclei? The answers are negative, according to most textbooks. Why should I reject these answers? In any case, I am not a good sounding board for speculating about standard model.

12) Closing message from Evan to Ludwik (1/21/2008):
I appreciate your offer to pose the question of, "Speculation or truth?" apropos the concepts of Dean Sinklair and myself. Sinklair's memo is impressive. It is *thinking outside the box,* representative of questions troubling physics today. And, I am honored by association with the two of you. However, I do not believe a URL has potential to answer the question. I agree most people are unfamiliar with the Standard Model and thus see no virtue in upsetting it. This whole matter is a hot potato. No one wants to become involved even to argue that concepts are wrong. One might lose the argument which would be worse than ignoring the issue. *No response* is symptomatic of science when the ground begins to change under prevailing dogma. Unsaid is:

"We know what we were taught, we are professional at what we do, there is much needed work in our specialties, just like everybody else we have mortgages, vacations, kids to put through college and we don't intend to, and won't become involved with challenge to prevailing theory."

That surely is understandable. Ludwik, if High Energy Physics continues to fail, as I expect, stagnation of *energy*

independence will trigger public demand for new high science. That may happen within this year. Many who work with nuclear are near disillusion with the Standard Model. Now is a time for patience. Thank you again for your interest, efforts, and proffer of help.
P.S.
Inasmuch as the new unit is already posted, herewith is copy of review items used in my estimate of URL potential to generate questions of Alternate Model validity. I am certain the nested AM [alternative model] a physically more accurate nuclear structure than the contrived point particle SM [standard model]. And, of course, it does so without imaginary particles (quarks, gluons) and force fields (colors). I am confident to answer all questions regarding my posited model. Although no harm is done, I will be surprised if the CMNS list generates much interest in different nuclear structure. . .

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344) What is going on in our CMNS field?

Ludwik Kowalski
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February, 19, 2008

The CMNS field continue to be active. What follows are pieces of information and comments which do not form a coherent pattern. Why are they are worth recording here? Because these pieces caught my attention; I found them interesting. The first item is a review of of a recently published book about cold fusion. It appeared in the *Journal of Scientific Exploration*, volume 21, no. 4, pp. 801-805. The author is Dieter Britz, he is from Denmark.

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1) The Science of Low Energy Nuclear Reaction: A Comprehensive Compilation of Evidence and Explanations about Cold Fusion by Edmund Storms. Singapore: World Scientific Publishing Co. Pte. Ltd., 2007. 312 pp. $71.OO (hardcover) ISBN-13978-981-270-620-1.

"Cold fusion" refers here to the claim that by simple electrolysis of heavy water with some electrolyte, the deuterium ions (deuterons) that accumulate inside palladium or some other metal can be made to fuse. This surprising claim was made by Fleischmann, Pons, and Hawkins (1989) and, almost concurrently, by Jones et al. (1989). Much has been said about this subject, which has become a scientific "affair" of sorts, with most mainstream scientists refusing to accept the reality of cold fusion, and a smallish band of researchers continuing work and indeed publishing their findings. The present author began to collect a bibliography (hereafter, the Bibliography) of the literature in 1989 (see http://www.chem.au.dk/~db/fusion), including only papers in refereed journals, and the present count is 1367. As many as 30 books have been published on the subject. Of these, not all are notable, some being hasty productions or having clear weaknesses, but some stand out. Some of these were written by proponents of the phenomenon, and some by skeptics. Most of the books were written by people who have not themselves worked on the phenomenon, some being science journalists or enthusiasts without a scientific background.

Two books written by adherents of cold fusion stand out in that they were written by people active in "normal" science, with solid research records, and who have themselves done research on cold fusion. One of them was by T. Mizuno (1997), an electrochemist, and the other by radiochemist Edmund Storms, whose book is the subject of this review. It must be recalled that the claim of cold fusion is astonishing to anyone with a smattering of knowledge of physics. The fusion of deuterons is resisted strongly by electrostatic forces keeping these positively charged particles apart. Fusion can be achieved by heating a plasma (hot fusion), and theoretical studies of the expected rate of fusion at low temperatures, as obtains in an electrochemical cell, predict rates so low that nothing will be detected. There are, then, good reasons for being skeptical of cold fusion claims. A book such as this one, however, makes a strong case for cold fusion, not only by the results presented, but also by the impression the reader gets of the writer. Storms is a working scientist, and this shows clearly in the book. He begins with a clear statement that he is convinced of the reality of cold fusion (preferring to call it low energy nuclear reaction or LENR). He is not-like some proponents of the phenomenon-oblivious to the weaknesses of many claims, that is, he shows the proper critical attitude of a normal scientist and will (mostly) not accept unsound evidence. Much shoddy work has been done in the field, but after eliminating this, some evidence remains that leaves the skeptic in a quandary; the phenomenon is unlikely to be real for many reasons, but there is much evidence nevertheless that it may be. Storms does his best to point out such

evidence based on his own work. The book contains an interesting account of his own work, and to a practicing scientist, he conveys a true picture of science as it is done: it rings true to someone who is a working scientist.

The book is divided into ten chapters and six appendices. It begins with an overview, a brief history of the field and (Chapter 3) a description of Storms's own work. The book's strength lies here, and the scientific reader will feel at home. Chapter 4 outlines what is known or believed; Chapters 5 and 6 focus on where cold fusion might occur and how perhaps to affect or even initiate it by means of experimental parameters. Chapter 7 considers fusion products and their detection, and, in the last chapters, Storms looks at some theories, the future of the field, and, in Chapter 10, provides a brief summary. Each chapter has its own bibliography, with Chapter 4 having a massive 646 citations.

The claims by Fleischmann, Pons, and Hawkins and of Jones et al. immediately engendered an embittered controversy between the "believers" and the "skeptics", with arguments and evidence not always strong on either side. In general, one should be wary of blank pronouncements by scientists that something is impossible, and as Storms points out, although there is as yet no good theory of cold fusion, the experimental evidence is not easy to dismiss (in his view, it proves the phenomenon real beyond doubt). As mentioned above, the Bibliography has over 1300 entries. Compared with other fields that appeared around the same time, such as high temperature superconductivity, however, this is not a large number. The Bibliography also shows that the publishing rate rose sharply initially, but fell roughly exponentially thereafter, similar to the polywater affair (Franks, 1981), a curve that has been likened to the course of an epidemic (Bennion & Neuton, 1976). Many journal editors flatly refused papers on cold fusion, and some referees probably rejected papers not so much on the basis of the evidence presented, but on the basis that cold fusion simply must be an error. It must also be said that many cold fusion papers submitted (and published) have been of poor quality and deserved to be rejected. Storms agrees with this. The result has been that in recent years, although some work is still appearing in mainstream serious journals, most work now appears in enthusiast journals, where refereeing probably is not very strict. If these articles were counted (which they are not in the Bibliography), then several thousand papers have been published. Storms himself has published some of his work in these journals, no doubt tired of unfair rejections by the normal journals.

When two deuterons fuse (against a strong electrostatic barrier), there are three branches along which the reaction can proceed, two of them the most probable. One branch leads to tritium and a proton, the other to helium-3 and a neutron, and the third branch, normally occurring with a frequency of only 10^-7 that of the other two, to helium-4 and highly energetic gamma emission. In all cases, the end result is particles carrying extra energy corresponding to lost mass to be dissipated in various ways. This all happens in "normal", that is, hot, fusion. Skeptics argue that all emissions must be present, while proponents point to one or two of them as proof, or they say that the process may not be the fusion of deuterons, but some "hitherto unknown nuclear process" (Fleischmann Pons, & Hawkins, 1989). They insist that experimental observations are paramount, and the lack of theory will be made up later. Storms takes this view, hence his use of "LENR".

Storms' own work concentrates on tritium production and "excess heat". Storms was ideally placed at the Los Alamos National Laboratory (LANL) to detect tritium. When a solution of an electrolyte in heavy water is electrolysed at length, deuterium gas is given off but also some tritium, contained as an impurity in the heavy water (and perhaps also in the palladium). Storms is careful to account for these sources. His findings are interesting: excess tritium appears first in the electrolyte, not in the effluent gas, indicating that it is produced at the palladium electrode. Storms was careful to perform control experiments aimed at eliminating environmental effects (at LANL, there might occasionally be tritium in the air) and convincingly shows that they were indeed accounted for. Similar results were obtained by Will, Cedzynska, & Linton (1993), equally competently obtained, and the skeptic is hard put to reject these results.

Excess heat is measured with a calorimeter. There is current passing through an electrochemical cell, with a voltage across it, producing an input power, and the electrolysis reaction absorbs some of this power at a well-known rate. Effluent deuterium and oxygen might carry off some heat, and heat is both radiated and conducted away from the cell. To keep track of all this is not trivial, and Storms became expert in calorimetry over the course of years, as problems were found, some of them by skeptics and addressed by him, carefully doing control experiments to assess the magnitude of interference effects. One of them is the way the temperature inside the cell is measured. There may be temperature gradients inside the cell, and this problem can be overcome by a better calorimeter design, as was done by Storms. If in the accounting of the known input and the measured output powers there is an excess, this indicates the

presence of some phenomenon producing heat. One problem is the irreproducibility of the effect; only some electrodes show it and not all the time. This is one of the strong points that skeptics make. A new effect must be reproduced by others, and this has not been done to the skeptics' satisfaction, even after almost 20 years. Reproducibility can only be achieved, however, when we understand all the factors at work, which is not the case here, so irreproducibility itself does not invalidate cold fusion. Needless to say, Storms tried many different variables: various sources of the palladium, how best to vary current with time, how best to load the palladium with deuterium, what loading degrees are needed, and surface treatment of the metal before the experiment. The results do not yet give a very clear picture.

So the book makes a good case for cold fusion. There are some weaknesses. Some of the figures are poorly done, and the text is often awkward. Some expert criticism of Storms' calorimetry (Shanahan, 2006) is not mentioned, and there is some imbalance in attribution: to some extent, The work by Jones's team is reemphasized in favor of that of Fleischmann and Pons's team. Storms is not good especially with foreign names, mangling some of them, and Kirk Shanahan, one of his staunch critics, appears as "Kurt Shanahan" in the Index. Storms suggests the fusion of up to six neutrons with some nuclei, which will surprise many. The term "enthusiast" applies to Storms. The word "amazing" is seen six times in the book, and his critical attitude does seem to lapse at times. For example, he appears to accept what has been called "biological fusion" (Kervran, 1972), which is even less likely to be real than cold fusion, and even suggests that it might be behind spontaneous human combustion; and he also accepts claims by Mills & Kneizys (1991) of electron orbitals of the hydrogen (or deuterium) atom below the ground level, although here Storms appears to be a little skeptical, admitting that there is a lack of theory (which can equally well be said of cold fusion). In Chapter 9, he writes "... the skeptics went to war- a war they have now lost". In the Preface, he writes that cold fusion has now been proved. Many would disagree and remain unconvinced. So, the book is not n neutral on the subject. Nevertheless, these weaknesses are comparatively minor and do not detract from the major message of the book, the rather solid experimental evidence of some exotic process taking place, from a careful and self-critical researcher.

What then is the bottom line? This writer is still agnostic with respect to cold fusion because even a thorough worker like Storms has not succeeded in demonstrating the effect at will. This is not to say that we can dismiss cold fusion but simply that we must wait for evidence so convincing that even skeptics must accept it as real. If it indeed is real, then it is subject to parameters that as yet elude most workers in the field. Other newly discovered phenomena have been irreproducible for some time (albeit rarely for 18 years as here) and this alone does not prove it to be false. We shall have to wait and see. The Storms book certainly is recommended reading, for both skeptics and proponents.

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2) Faradic efficiency thread on CMNS list for researchers (Inserted on 3/5/08)
I am neither chemist nor electrochemist. But two messages under this thread are worth recording. One was from our electrochemistry and calorimetry expert, Mike McKubre and one from the author of the above review, Dieter Britz.

3) On 3/4/08 Mike wrote: This debate is diverging. I suggest we propose a new rule to encourage convergence. Let's call it: QUANTIFICATION.

A lot of the more subtle condemnation of cold fusion is disguised - as in politics - behind unspecified doubts concealed in the implication that electrochemistry is "too complicated", contains "unknown or mysterious" variables, somehow an "art not a science" --- you know, sort of like witchcraft. IT IS NOT! Let us role back this deception, I suspect unsuspectingly practiced by some on this list. The way forward is NUMBERS!

Under this rule new material can be entered into the discussion if it is clear the the proponent has made numerical estimates of the effect he proposes. I am not picking on Dean, Dieter or Scott but am going to use their recent input as examples of how things might change (I argue for the better) with the influence of such a rule.

Lithium forms several substitutional alloys with palladium. The reaction enthalpies can easily be found by anyone with access to a library or internet. Let's take (for example) the most famous experiments of F&P and calculate how much enthalpy could be released (you need the volume of electrolyte times the Li concentration, the putative phase formed and the Li heat of solution times the volume of Pd). If that number is small compared with the excess energy observed then we can stop. If not then we need to dig a little deeper and quantify the extent to which Li can deposit in aqueous electrolysis and how far it can diffuse into the Pd*. I am not going to do it for you - but I recommend the exercise.

Scott asked about electrolysis current below the formal breakdown potential. We have gone round this loop before and I took some trouble to answer it in terms of electrochemical kinetics and thermodynamics. I don't want to discourage debate but I do want to encourage learning. To make the question and answer quantitative one needs to look at the solubility and diffusion of molecular D2 and O2 in the solution (water) and recognize that - if the solution contains no dissolved D2 or O2 then the breakdown of water can occur at an (unspecified) low voltage. Ficks first law, the diffusion coefficient and the diffusion layer thickness will get you near-quantitative answers. The answer to this question will also help with the answer to the next.

I was surprised to see Dieter as a proxy for Shanahan's straw man make a Hillary-like** statement that he "I don't know whether this happens or not, but that is his argument." Why don't you know? If you don't know, why introduce the unsupported argument of another? You have all the expertise to do the experiment and access to a good library. Information on the catalyzing power of Pt and Pd under a monolayer or more of water is readily available as this is very important in fuel cells and heterogeneous catalysis. I am cheating a little bit here as my electrochemistry group in 1989 was studying fuel cell electrode kinetics, and happened to have been situated in the middle of a world famous group studying all sorts of heterogeneous catalysis. With a simple walk across the hall I was able to learn that the catalyzing power of wet Pt or Pd was about the same as pyrex (*i.e.* nil). Even so we still measured and studied the effect and ran closed cells to avoid it.

But the information is out there for all. If you don't know how to frame the calculation then you are probably not going

to understand (or therefore accept) the answer. We need to commit to work to make progress, not endlessly answer 1989 questions. As proponent and so far sole supporter of the QUANTIFICATION rule I pledge to not introduce new matter except with quantification and not respond to new matter on this topic unless it is quantitatively bounded. . . .

4) On 3/5/08 Dieter wrote:
McKubre wrote "This debate is diverging. I suggest we propose a new rule to
encourage convergence. Let's call it: QUANTIFICATION. A lot of the more subtle condemnation of cold fusion is disguised -as in politics - behind unspecified doubts concealed in the implication that electrochemistry is "too complicated", contains "unknown or mysterious" variables, somehow an "art not a science" --- you know, sort of like witchcraft. IT IS NOT! Let us role back."

I hope you are not suggesting that I have some sort of agenda, other than providing information where I can. I do not. I am neutral on the subject, and would indeed be pleased if CNF turned out to be a real effect. But it has to be proven, not just assumed because it's nice thought.

He also wrote "Lithium forms several substitutional alloys with palladium. The reaction enthalpies can easily be found by anyone with access to a library or internet. Let's take (for example) the most famous experiments of F&P and calculate how much enthalpy could be released (you need the volume of electrolyte times the Li concentration, the putative phase formed and the Li heat of solution times the volume of Pd). If that number is small compared with the excess energy."

That is not necessary, because we know that very little of that Li is deposited; this has been measured to be at most 1 atomic% in a very thin layer near the Pd surface. No sausage there. There have been suggestions that the redissolution of the Li might cause heat bursts, but this will not happen during electrolysis, so that is not on either.

He also wrote "I was surprised to see Dieter as a proxy for Shanahan's straw man
make a Hillary-like** statement that he "I don't know whether this "

I am not a straw man for anybody, mate. I was just thinking of all possible side reactions that might take place, in the framework of faradaic efficiency, and this was one of the suggestions. You are echoing somebody else here, who insists that I have this anti-CNF agenda. I repeat, I do not.

I like people to get their facts straight. I don't have them all myself, but when I have some, or some ideas, I point them out. That makes me unpopular on both sides of this debate. I got into Morrisons's bad books by demolishing his "cigarette lighter effect" which he thought at one time would explain excess heat. My agenda is for people to believe what they believe, for the right reasons. I do think I have made it clear the last week or so, that I do not believe that faradaic efficiency less than 100% is the cause of excess heat.

5) On Mar 5, 2008, Mike McKubre wrote:
"Dieter Britz wrote: I am not a straw man for anybody, mate. I was just thinking of all possible side reactions that might take place, in the framework of faradaic efficiency, and this was one of the suggestions. You are echoing somebody else here, who insists that I have this anti-CNF agenda. I repeat, I do not."

I am not sure I qualify as your mate, but hopefully no hard feelings. I am happy to consider all plausible reactions. What distinguishes plausible from possible is quantification. Actually I was not echoing any other comment that you have an anti-CNF bias. I have never heard that and have seen no evidence of it. In fact I believe the opposite and am trying to extract maximum benefit for the community.

He also wrote "My agenda is for people to believe what they believe, for the right reasons. I do think I have made it clear the last week or so, that I do not believe that faradaic efficiency less than 100% is the cause of excess heat."

That last clause was not clear - at least to me. From someone with your background and credentials that conclusion is significant. What I objected to was you raising Shanahan's dead horse - without doing the experimental or theoretical walk through - apparently (now) when you did not believe it was relevant to our experiments. Of course you are free to

contribute whatever you want - it will all be good. My point is that divergence promotes confusion.

6) On March 6 Dieter wrote:
In this case, somebody asked, in effect, what is faradaic efficiency, and I was trying to think of all possible effects that might reduce that. Then someone mentioned oxygen being reduced at the cathode, and hydrogen oxidized at the anode. Some appear to believe that this is what Kirk Shanahan proposes, and to counter this I added what he actually does propose. Even had I been aware that this has been experimentally tested and discounted (which I was not), I would have felt bound to mention it, to avoid this misunderstanding of what Kirk is saying. I like this forum because it seems to me that we can have rational arguments here, without excess heat. I hope it stays like that.

7) My comment (3/6/08)
Messages keep popping up under this thread. That is not my field. But I am happy to see disagreements among subscribers to the CMNS list. That is a sign of health. How different is it from what I often hear from people who say that they already know (since early 1990's) that “cold fusion” is pseudoscience. These people do not want to “waste time” on reading reports from ongoing experiments.

8) CR-39 work
My numerous attempts to get information about recent CR-39 findings (of SPAWAR team and of those who cooperate with SPAWAR) were not successful. The seem to prefer not to share preliminary results on this list (or with me via private messages), as they did so generously before Catania conference. Why is it so? But I am corresponding privately with Richard Oriani. He is making progress in trying to publish a paper on his most recent results (tracks originating deep inside CR-39). I am affraid that sharing what I know here might interfere with the process. But I do plan to write about this (with Richard's permission) later.

9) ICCF14 The next CMNS conference will take place this summer in Washington DC. The exact date will probably be announced shortly. I expect the ICCF14 to be dominated by experimental reports from Iwamura et al. (evidence for transmutation) and from Storms et al. (evidence for nuclear activity during glow discharge). Also by the report from Boss and from Oriani (evidence for nuclear particles resulting from a process induced by electrolysis).

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345) My comments on Storms' new book

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April 4, 2008

A published review of Ed Storms' book can be seen in unit 344. This unit consists of my own observations. I agree with Dieter Britz, the author of the review, that the book is recommended reading, for both skeptics and proponents of so-called "cold fusion." Storms also uses this term "for the sake of consistency and habit." Most scientists are not familiar with more recent terms, CMNS and CANR. For that reason the term cold fusion seems to be appropriate.

The list of references alone has a tremendous historical value; no one but a scientist who participated in cold fusion research from the very beginning could compile such list of publications, and to categorize them logically. The publications belong to two categories: those which demonstrate excess heat and those which demonstrate other manifestations of nuclear processes due to chemical (atomic and molecular) activities. Both kinds of research are important, according to Storms. But investigations of excess heat are more urgent because nuclear reactions producing excess heat are numerous, and because they might lead to practical applications. Referring to excess heat Storms writes (on page 50) "More important, the Fleischmann and Pons observation was unique because the high rate might allow their process to become a competing source of energy and a thread to conventional theory, rather than being simply a scientific curiosity. A mere scientific curiosity can be accepted without a battle; an economic and intellectual thread cannot."

Chapter 4, entitled "What is Known or Believed" is a very good summary of all kinds of cold fusion research, first excess heat, then production of tritium, production of helium, transmutations and emission of nuclear particles. I was familiar with many publications mentioned in that chapter. But reading the summaries and comments made by Storms was very useful and eye opening, especially when common features were emphasized. What follows is a paragraph from a message I posted on the Internet list for CMNS researchers. Unfortunately, this was done in the form of a side comment, under a different thread. That is probably why only one person responded to my message.

On 3/28/08 I wrote: "Why do we say that excess heat discovered in 1989, and confirmed in hundreds of replications, is due to a nuclear activity? Perhaps it is due to an unknown gravitational phenomenon, to an unknown magnetic phenomenon, to an unknown electric phenomenon, to an unknown chemical phenomenon, etc. ? I am familiar with only one answer -- production of ^{4}He. Production of each atom of ^{4}He is associated with production of about 23 MeV of excess heat. Uncertainties about the 23 MeV could be reduced in better experiments. In my opinion, experiments in which generation of ^{4}He and generation of excess heat take place are worth performing. Hopefully they will show that in at least one kind of experiment excess heat seems to be nuclear. Am I wrong that production of ^{4}He is the only case in which nuclear ashes mach excess heat? " The person who responded wrote: "The Fleischmann-Pons effect clearly involves a nuclear reaction. Of course, other sources of heat might be produced at the same time, but the main heat is clearly from a nuclear reaction."

That was certainly not clear in 1989. The only reason for nuclear interpretation, as far as I recall, was a theory according to which the pressure of deuterium ions, loaded into palladium, as calculated by Fleischmann, was high enough to allow fusion of individual deuterons. He did not write anything, at that time, about cold fusion being totally different from hot fusion. He and Pons performed an experiment whose purpose was to justify nuclear origin of excess heat by neutrons, presumably emitted from their electrolytic cell. The results of that experiment, by the way, were later shown to be in error. Neutron emission, according to Storms (page 78 of his book) is seldom found even though over

500 papers have described efforts to detect them." In section 4.4.2 Storms describes several experiments in which amounts ^{4}He was measured. These experiments continue to be the only evidence of nuclear origin of excess heat in cold fusion.

Let me now focus on topics of general interest; as described at the end of Chapter 4. Two common aspects of many electrolytic excess heat experiments are emphasized by Storms -- the dependence of the rate, at which thermal energy is generated, on the mean density of deuterium in palladium and its dependence on the current density. But experiments are not reproducible on demand. Why is it so? Because no one knows what other conditions must be satisfied. Thermal energy is released "when a lucky combination of conditions has been created." A special term, NAE (nuclear active environment) is used to identify a material in which such conditions are satisfied.

On page 13 Storms writes: " Some people are simply unable to evaluate observations with an open-mind because, to them, truth is only defined by theory. If theory and observations are in conflict, theory wins." That attitude is called faith-based science; it is not reality-based science, "the kind of science we are taught to respect." The relation between experimental facts and explanatory theories is profound and worth discussing. Absence of a theory linking NAE with what has already been learned about nuclear phenomena is deplorable but it does not mean that a rational explanation of NAE will not be found. In Section 4.2 Storms writes: "Indeed absence of conventional nuclear products of any kind [in 1990], except perhaps tritium, made the high rate claimed for heat production suspicious in many people's eyes. While this was a legitimate concern, rejection did not wait until the proper measurements were made. Only now, 18 years later have the nuclear products been detected on a sufficient number of occasions and with sufficient care to be credible."

In my opinion, cold fusion would not be rejected prematurely if the initial announcement of the discovery of excess heat were not accompanied by then-unjustified claim of its nuclear origin. The architect of rejection, John Huizenga, is described (on page 15) as "a competent scientist and teacher. ..... He was not about to change his mind, especially while so many questions remained unanswered." I had a chance of discussing nuclear topics with Huizenga -- at least two decades before the discovery of cold fusion -- and I agree that he was an extremely competent nuclear scientist. People like him feel that defending science from unreasonable claims is one of their moral obligations. The claim of nuclear origin of excess heat became reasonable only when production of helium, at the rate of approximately $2.65*10^{11}$ atoms per joule of thermal energy, was discovered and confirmed by qualified electrochemists. The inverse of this number, 23.5 MeV per atom, happens to be $E=m*c^2$, where c is the speed of light and m is the difference in mass between one ^{4}He atom and two ^{2}H atoms. This seems to support the original idea that ^{4}He results from fusion of two deuterons. But, as explained in Section 8.3.1, other nuclear mechanism to produce helium are also possible. The history of cold fusion would certainly be different if generation of excess heat and production of helium were announced at the same time.

It is interesting that Ed Storms is well aware that "In this business, too much willingness to be open-minded can be a danger." This observation, made on page 28, refers to his refusal to cooperate with an alchemist Joe Champion. I do not think that the pyrotechnical method of turning other elements into gold, suggested by Joe, can be qualified as cold fusion. The idea of associating one mystery with another mystery, especially when connections are not clear, can indeed be counterproductive.

The following hypothetical situation can be considered after reading section 4.8. Suppose that a team A makes a claim that excess heat has been observed and that it is due to accumulation of deuterium (from the D_2O in the electrolyte) in a palladium cathode. This is not very different from the claim made in 1989 by Fleischmann and Pons. But their situation was complicated by secondary factors, such as competition with Steve Jones, pressure from university administrators, and involvement of lawyers. That is why I prefer to deal with hypothetical situations. Note that the A team makes two claims, one is generation of excess heat and another is a mechanism by which this happens.

Suppose that B, who is not an experimentalist, makes the following comment. "If your explanation is correct then generation of excess heat should not take place when the D_2O is replaced by the H_2O. Perform the experiment and report the result." The team A returns to the laboratory performs an experiment and find that excess heat is also produced when the H_2O electrolyte is used. After learning about this B proclaims: "Aha, your theory is not good."

That would be a reasonable kind of thinking, considering what was known at that time. Another possible proclamation, made by B, could be: “Your claim about excess heat must be rejected because it conflicts with your own explanation. Not too many deuterons are present in the new electrolyte and excess heat due to deuterons is impossible.” That would be a totally unreasonable conclusion; the only way to refute generation of excess heat would be to perform an experiment and to show that excess heat is not generated.

This, however, as pointed out by Storms, is not simple because preconditions for success, in obtaining excess heat, are not known. The control experiment performed by B is likely to different from the experiment performed by A? That is indeed a very important point. It means that replications cannot be trusted, even when all researchers are highly qualified. In my opinion, that fact alone is sufficient to say that cold fusion is still protoscience. A field in which something occurs only “when nature is in a good mood” should not be called science. Absence of a generally accepted theory can be tolerated in a new field of science. But absence of reproducibility, when experiments are performed by qualified researchers, is not acceptable. Science has too much to loose from not insisting on “reproducibility on demand.” To be admitted to the prestigious club called science, cold fusion must offer at least one reproducible on demand experiment in which a nuclear process results from a chemical process.

Protoscience, like science, should be financially supported. The amount of money allocated to any field of research should be based on its potential benefits, both scientific and practical. I agree with Ed that “For a theory to be useful, the logical consequences of the model must be consistent with observations, including what is not observed, and with well established physical laws.” The issue of an acceptable theory and the issue of reproducibility on demand are intimately linked. Theoreticians need reliable experimental data, to validate predictions, and experimentalists need reasonable predictions to guide them. That is why I think that an acceptable theory is likely to emerge once experimental data become reproducible on demand.

In Chapter 8, Ed wrote that “incorrect assumptions in theories are equivalent to experimental errors in observations. Both lead to false conclusions and distract from general acceptance.” I like this comment. What he probably had in mind are systematic, rather than random, experimental errors. Random errors in experiments are unavoidable and fluctuation of results by a small percentage is usually acceptable in a new investigation. In other words, differences of up to about 10%, between results reported by several teams, would not interfere with the idea of reproducibility on demand, at least in some kinds of investigations.

Appended on 4/5/08 (some speculations):
According to section 7.9, most helium is produced when alpha particles slow down. If this is true then a lot of alpha particles must be emitted. SPAWAR people did say that the track density in CR-39 was very high in their experiments. But no convincing evidence was presented that tracks were due to alpha particles, or to other nuclear projectiles. Let me assume that heat is generated at the rate of 0.5 W. This translates into 0.5 J/s or to $3.12*10^{12}$ MeV/s. The energy cost of producing one helium atom was reported as 23.5 MeV. The corresponding rate of helium production, as indicated earlier, would be $3.12*10^{12} / 23.5 = 1.33*10^{11}$ atoms per second.

Note that fusion of two deuterons, originally believed to be the mechanism by which nuclear energy is released, does not call for emission of alpha particles. Recall that the name “cold fusion” was chosen to describe such mechanism, at common temperatures. In section 8.3.1 Storms reminds us that the “d-p fusion should be more common than d-d fusion.” But no evidence of d-p fusion was found. On that basis one must conclude that the d-d fusion is not a dominant mechanism by which nuclear energy is released.

What are other possibilities? That question is also answered in section 8.3.1. One of the possibilities is fusion of a deuteron with ^{6}Li. This produces a highly unstable ^{8}Be nucleus which is known to instantly decay into two alpha particles. The kinetic energies of each of these particles is 5.6 MeV energy. Note that the coulomb barrier for the d-Li fusion is much higher than for the d-d fusion. How many alpha particles would be emitted per second via the d-Li mechanism at 0.5 W? The answer is $3.12*10^{12} / 5.6 = 5.57*10^{11}$. Four other mechanisms, for producing alpha particles are described in the same section. In section 8.3.2 Storms describes mechanisms that can possibly be responsible for production of much heavier elements. One of them is fusion of deuterons (for example, one after another) with an atomic nucleus of palladium. This would produce an excited compound nucleus undergoing fission. Is it conceivable

that Cu and Fe are fission products?

My doctoral dissertation (1963) was to study fission resulting from bombardment of uranium, bismuth and gold with protons of 156 MeV. Later I participated in several projects in which fission of lighter nuclei, induced by heavy ions (after they fused with targets at various energies), was studied. On the basis of what I know, I would say that the probability of fission would be negligible in comparison with other mechanisms of de-excitation, such as emission of neutrons and protons. Note that the coulomb barrier for the d-Pd fusion, as mentioned by Storms, is very much higher than for the d-d fusion. That is another obstacle. Trying to explain presence of transmutation products by ideas taken from familiar nuclear physics does not promise to be successful. Something totally different is needed to explain experimental results. Polyneutrons? Erzions? Hydrinos? Crazions? Sanions? Smartions? Whatelsions? I predict that the answer will be found not later than one or two decades after a reproducible on demand protocol is offered to generate transmutation products. Yes, many of us will not be around to verify this prediction.

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346) Information for users of CR-39

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April 6, 2008

0) Chemical composition and some proprties of CR-39 are shown at:
http://tinyurl.com/yp6ld5
Last page shows that density is 1.31, refractive index is 1.498, and that 89-91% of light is transmitted.

1) CR39 detectors are likely to play an important role in CMNS research. With this in mind, all researchers should use the same etching conditions, and the same calibration curves. Otherwise it would be difficult to directly compare our experimental results. A calibration curve is a relation between the track diameter and the particle energy (for perpendicular exposures and for a chosen etching protocol). Calibrations experiments were performed by Russian scientists: Andrei Lipson, Alexei Rusetski and Eugeny Saunin, as reported at the 8th International Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals (Catania, October 13-18, 2007).

Their standard etching times (in 6N NaOH at 70 C) are 7hrs, 14 hrs, 21 hrs and 28 hrs. The layers of the CR-39, etched away during these times, can be calculated from the known bulk etching rate of 1.32 microns per hour. Thus etching for 28 hours will make the CR-39 chip about 2*37=74 microns thinner. Note that the range of a 5 MeV alpha particle in CR-39 is approximately 30 microns. Longer etching can be used to reveal tracks due to alpha particles produced inside the CR-39 material.

2) The experimental data for the calibration curves are available for protons between 1 and 2.5 MeV, and for alpha particles between 5 MeV and 13 MeV. What follows are equations which fit experimental data points. Note that x is the energy in MeV while y is the corresponding diameter in microns. Also note that diameters might depend on some difficult-to-control parameters, such as additives added during manufacturing, time of storage, conditions of storage (air versus vacuum), etc. etc. For that reason, relative diameters might be more informative than absolute diameters.

Alpha Particles (5 to 13 MeV)
Etching for 7 hours: $y=12.364*x^{-0.2169}$
Etching for 14 hours: $y=30.556*x^{-0.3733}$
Etching for 18 hours: $y=81.763*x^{-0.6785}$
Etching for 21 hours: $y=145.04*x^{-0.8321}$

Protons (1 to 2.5 MeV)
Etching for 7 hours: $y=7.1395*x^{-0.3363}$
Etching for 14 hours: $y=9.6093*x^{-0.5057}$
Etching for 18 hours: $y=12.354*x^{-0.6143}$
Etching for 21 hours: $y=15.611*x^{-0.7608}$

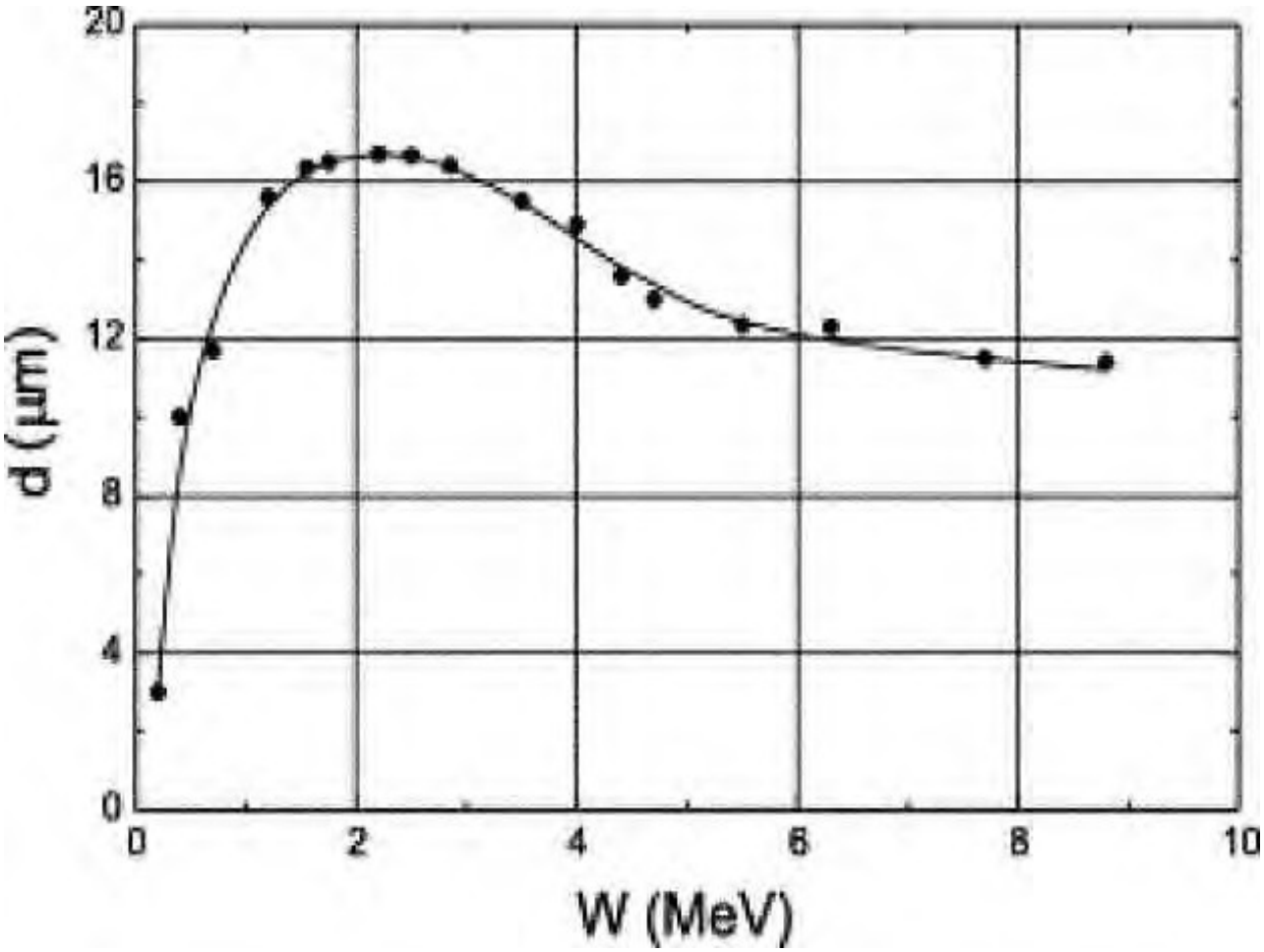
The plots of these eight functions, together with actual data points, can be seen at

http://www.iscmns.org/catania07/RussetskiAanalysisof.pdf

3) It is well known that most alpha-radioactive substances emit particles with energies between about 5 MeV and 8

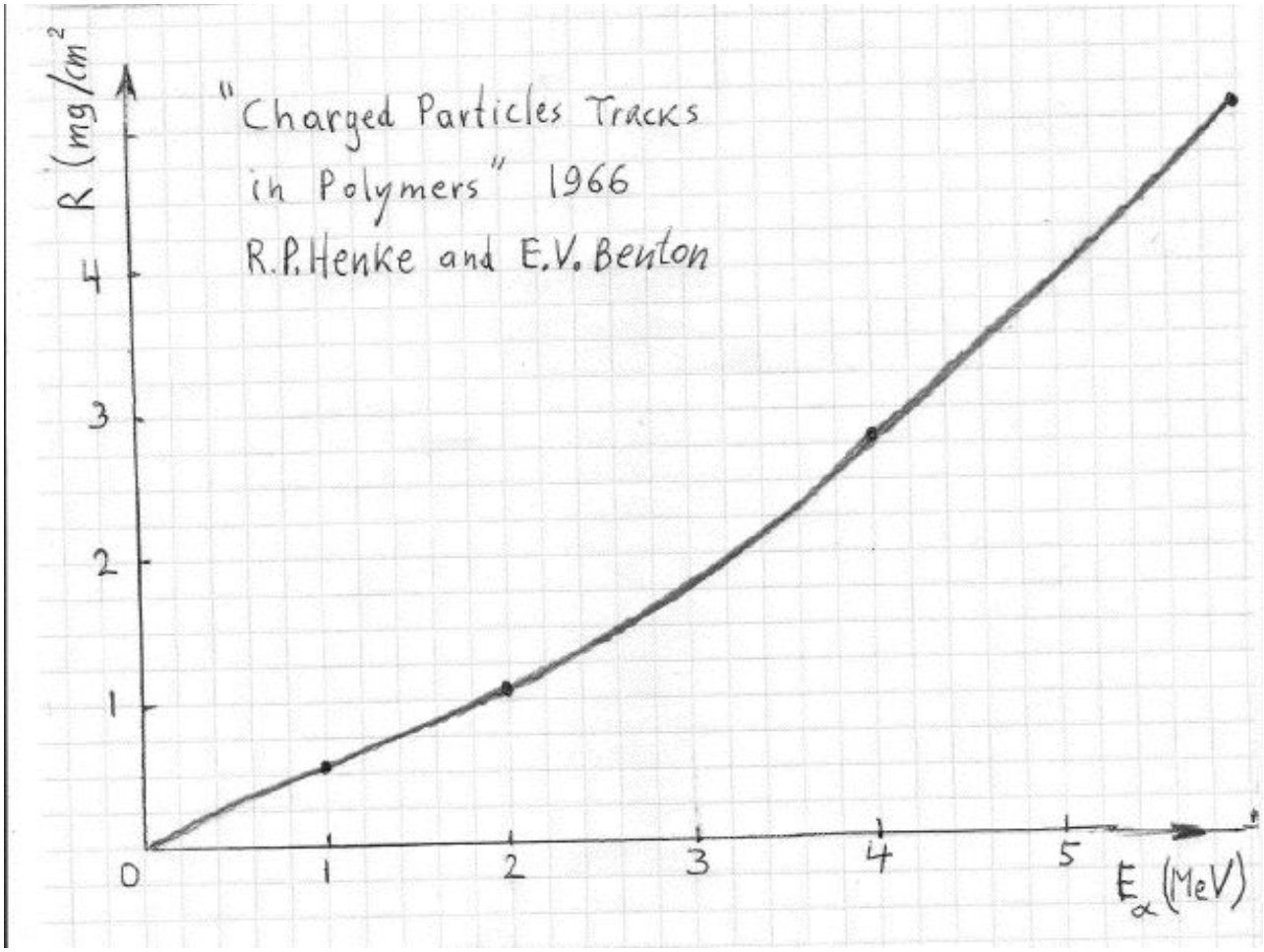
MeV. For these particles, the diameters, after 7 hrs of etching (in 6 N NaOH at 70 C), should be between about 8.7 and 7.9 microns, for nearly perpendicular incidences. For oblique incidences, tracks are expected to be more or less elliptical and the y would refer to the width of an elongated track. A track whose diameter, or width, is significantly smaller than 7.9 microns could not be attributed to an alpha particle from a natural contaminant. But it can be due to a triton, or to a neutron colliding with a proton. Likewise, a track whose diameter, under the same etching conditions, is larger than 15 microns cannot be attributed to an alpha particle from a natural contaminant. But it can be attributed to a fission fragment, for example, from spontaneous fission of uranium.

4) It is important to keep in mind that these equations should not be used outside the energy ranges specified above. Diameters do not continue to increase with decreasing energies. At some points diameters are known to decrease, rather than to increase, as illustrated in the figure below. That figure, due to Dorschel (1995?), is a calibration curve for alpha particles with data points for energies smaller than 5 MeV. The etching conditions were probably different from those used by Russian scientists. Perhaps the concentration of the NaOH, or etching temperature, or etching time, were higher than those chosen by Lipson et al. I am saiying this because Dorschel's diameters at 5 MeV and 8 MeV are 13 microns and 11.5 microns, respectively. These values are nearly 50% higher than those calculated from the first equation above. Extrapolations of the equations toward lower energies are likely to result in large systematic errors.



This curve was posted by a researcher on our Internet discussion list. But I also found it (in a slightly different form) in C. Brun, M. Fromm, M. Jouffroy, P. Meyer, J.E. Groetz, F. Abel, A. Chambaudet, B. Dorschel, D. Hersmdorf, R. Bretschneider, K. Kander, and H. Kuhne; "INTERCOMPARATIVE STUDY OF THE DETECTION CHARACTERISTICS OF THE CR-39 SSNTD FOR LIGHT IONS: PRESENT STATUS OF THE BESANCON DRESDEN APPROACHES," Radiation Measurements, vol 31, (1999), pp 89-99.

5) The next figure shows the range-energy relation for alpha particles in polymers (plotted by me a long time ago) is shown below.



6) To convert ranges from mg/cm^2 to microns one must know the density of a particular material. The density of CR-39 is 1.3 g/cm^3. That is why the range of 5 MeV alpha particles is close to 30 microns. For a material of density 1.0 the number of microns would be ten times larger than the number of mg/cm ^{2}. That is a good rule of thumb for crude estimates, even when densities are slightly different from unity.

7) Ranges of alpaha particles in air (at 20 C and 1 atm) are shown in the table below.

Here is a table showing how energies depend on distances between the CR-39 and the source of 5.5 MeV alphas, in air at 1 atm and 20 C. This table was constructed on the basis of 1966 data. I am certain that these data are reliable, but I will try to produce a similar table on the basis of more recent data.

Energy (MeV) ..... Range(cm)
1.0 . . . . . 0.50
2.0 . . . . . 1.00
3.0 . . . . . 1.68
4.0 . . . . . 2.60
5.0 . . . . . 3.63
6.0 . . . . . 4.78
7.0 . . . . . 6.02
8.0 . . . . . 7.35
9.0 . . . . . 8.77

Suppose an 241Am source (initial energy is 5.5 MeV) is used to calibrate a detector. How does the energy of alpha

particles depend on the thickness of air between the source and the detector. The answer is shown in the next table. It was produced on the basis the above data.

distance .....energy

0.5 cm..... MeV
1 cm..... MeV
2 cm..... MeV
3.5 cm..... MeV
3.75 cm..... MeV
3.9 cm..... MeV

Here are two usedul equations to calculate energy from distance, or vice versa.

E =5.416 - 0.5646 * d - 0.1790 * d^2

d = 4.092 - 0.31705 * E - 0.07847* E^2

Each of them is a reasonable fit for the data in the above table.

Appended on 4/28/08
8) Looking for something else I found an interesting paper. The title is "Variation of alpha-particle track diameters in CR-39 as a function of residual energy and etching conditions." The authors are A.H. Khayrat and S.D. Durrani. The paper was published in February 1999 (Radiation Measurements, vol. 30, Issue 1, pages 15-18). Can the track diameter be used for [rough] energy spectrometry [in the energy region between ~1 MeV and ~ 5 MeV]? The authors explore conditions under which this should be possible. They used a source of alpha particles whose initial energies were 5.5 MeV. Energies of particles entering the detector, E, were controlled by the amount of air between the source and the detector. These energies were actually measured by using a calibrated surface barrier detector, connected to a multichannel analyzer. Collimating tubes were used to make sure that angles of incidence were small. In other words alpha particles were entering detectors nearly perpendicularly. The etching solution was 6 M NaOH at 70 C.

9) The bulk etching speed, Vb, in CR-39, was measured by using a 252Cf source of fission fragments. Presumably, it is well known, that for fission fragments the track diameter, D, is directly proportional to the etching time (D=2*Vb*t). In other words Vb was determined by measuring D and t for several tracks. The track etching rate, Vt, was determined by authors by measuring the etch-cone length, Le, after the etching tile t. The used relation was Le= (Vt-Vb)*t . The Le seems to be the depth of the track. But the method for measuring it is not specified. Ranges of alpha particles of different energies in CR39, were calculated using a computer program based on old Henke and Benton's data. How do ranges plotted in their Figure 2, differ from those I plotted above? The answer is in the following table (numbers read from their Figure 2):

Alpha energy . . . Range in CR-39

5.5 MeV .....33 microns
4.0 MeV .....20 microns
2.4 MeV .....10 microns
1.6 MeV .....7 microns
0.8 MeV .....3 microns
0.0 MeV .....0 microns

10) Figure 3, in their paper, shows how the values of dE/dx (in MeV per g/cm^2) depend on energies of alpha particles.

Alpha energy..... dE/dX

5.5 MeV 800
4.0 MeV 1000
2.4 MeV 1400
1.6 MeV 1780
0.8 MeV 2000
0.4 MeV 2200
0.1 MeV 1900
0.0 MeV 0

11) It is convenient to think about dE/dx as an indicator density of ionization, or as degree of damage created by the particle in the CR-39 material. Note that dE/dx reaches a maximum when the particle is nearly stopped. The fact that dE/dx decreases rapidly when particles slow down can easily be explained. At the end of their ranges alpha particles catch two electrons (one after another), and turn into neutral atoms of helium. The dependence of dE/dx on E is after called Bragg's curve.

Note that If diameters of tracks were proportional to dE/dx, at corresponding energies, then diameters of tracks due to alpha particles of ~ 1 MeV would be two times larger that diameters of tracks due to alpha particles of 5.5 MeV. The authors did measure diameters of alpha particles at different energies and results are shown in their Figure 5. According to that figure, the diameter for 5.5 MeV particles was found to be close to 4.5 microns while diameter of 1 MeV particles was found to be close to 7 microns. For 3 MeV particles diameters are close to 6 microns while diameters below 1 MeV quickly decrease to zero, just like the values of dE/dx. It is important to emphasize that these results were obtained with etching times of 2 hours. The ratios of diameters at longer etching, for example, at 5.5 MeV versus 1 MeV, are usually much smaller, as illustrated by Dorschel's calibration curve (see my Figure at point 4 above).

12) The main point of this study is to show that it is possible to estimate energies of alpha particles on the basis of track diameters, provided etching times are very short. The slope of their curve in Figure 5 (track diameter versus the energy above 1 MeV) is approximately 0.6 microns par MeV. Suppose a CR-39 chip is exposed to alpha particles of 1.5 and 5.5 MeV. Then diameters of tracks due to low energy particles will exceed diameters of tracks due to high energy particles by 4*0.6 = 2.4 microns. This is measurable, under high magnification. Yes, the energy resolution offered by CR-39 detectors is not as high as that offered by silicon detectors. But ability to distinguish low energy alpha particles whose energies differ by two ot three MeV might be appreciated in applications in which only CR-39 can be used.

13) Why did I not say anything about Figure 4, and text describing it? Because this part is not clear to me, except for they way of etching. Instead of etching for only two hours, the chips were etched for times t=R/Vt, where R is the range of alpha particles. In other words chips exposed to alpha particles of higher energies were etched longer than chips exposed to alpha particles of lower energy. The dependence of diameters on energies of alpha particles is plotted as curve (a) in Figure 4. The rate at which diameters increase with energy is high, about 10 microns per MeV, for E>4 MeV. But how much of the slope is due to longer etching times (at higher energies) and how much (if anything) is due to other factors? The authors are trying to answer this question but I cannot understand them.

Appended on 4/29/08

14) According to my Unit #347, SPAWAR team works under the hypothesis that nuclear tracks, produced during their electrolysis experiments, are due to alpha particles of ~1 MeV. Referring to this I posted the following message on the Internet discussion list for CMNS researchers.

" Here is an idea based on that paper of Khayrat and Durrani. What would I do to confirm Pam's hypothesis -- that tracks produced during electrolysis are due to alpha particles of about one MeV?

a) First I would try to replicate the experiment of Khayrat and Durrani (using alpha particles of at least three energies: 5.5, 3 and 1 MeV and etching for two hours only). Suppose my results also showed that tracks due to alpha particles of ~1 MeV are nearly two times larger than those due 5.5 MeV.

b) In that case I would start testing the hypothesis. First one corner of a CR-39 chip would be irradiated by alpha particles from 241Am. Then I would conduct a codeposition experiment, using that chip, as described in SPAWAR last paper (*). But instead of etching the chip for 9 hours, I would etch it for 2 hours.

(*) Mosier-Boss, P., Szpak, S., Gordon, F., Forsley, L, "Use of CR-39 in Pd/D Co-deposition Experiments," European Physical Journal, Applied Physics , Vol. 40, p. 293–303, (Dec. 13, 2007)

c) After that I would compare sizes of tracks due to 241Am with sizes of tracks created during electrolysis. Suppose sizes of tracks created during electrolysis were nearly twice as large as those due to 241Am. That would be a confirmation of their hypothesis.

Does this make sense? I am assuming that tracks, attributed to nuclear particles, are nearly always produced in at least one kind of SPAWAR experiment. That seems to be the case, according to their publications."

15) In subsequent message, on the same forum, I wrote: "Suppose the following question is asked. Why are tracks due to ~ 1MeV alpha particles nearly twice as large as tracks due to 5.5 MeV particles, after 2 hours of etching, while at much longer etching (for example, ~7 hrs) sizes of tracks are nearly the same (according to Dorschel)?

I already tried to answer this question.But here is a simpler answer. It is well known that the degree of damage created by an alpha particle in CR-39, is higher near the end of the latent track. The range of alpha particles of ~ 1 MeV is close to 4 microns while the range of 5.5 MeV particle is close to 30 microns. Two hours of etching is enough to reach the bottom of a latent track of a 1 MeV particle but not enough to reach the bottom of the latent track of a 5.5 MeV particle. The highly damaged region, of the 5.5 latent track, is not etched and that is why the track is not as large as it would be after etching for 6 or 9 hours.

This probably captures the essence of the explanation without addressing many details. I would very much like to know if simulated tracks, after 2 hours virtual etching, agree with experimental data of Khayrat and Durrani."

18) About etching rates, Vb (often assumed to be 1.2 microns per hour) and Vt
Also from: C. Brun, M. Fromm, M. Jouffroy, P. Meyer, J.E. Groetz, F. Abel, A. Chambaudet, B. Dorschel, D. Hersmdorf, R. Bretschneider, K. Kander, and H. Kuhne; "INTERCOMPARATIVE STUDY OF THE DETECTION CHARACTERISTICS OF THE CR-39 SSNTD FOR LIGHT IONS: PRESENT STATUS OF THE BESANCON DRESDEN APPROACHES," Radiation Measurements, vol 31, (1999), pp 89-99. According to authors, Vt, the etching rate along the track, can be calculated from the easily measurable Vb, the bulk etching rate (both in microns per hour).

Vt = [1 +0.00155 * (LET-100)] * Vb <----- for 7 M NaOH at 60 C

and

Vt = [1 +0.00267 * (LET-100)] * Vb <----- for 8 M NaOH at 70 C

And what if LET (Linear Energy Transfer) is less than 100? In such cases Vt = Vb. The LET, in these formulas, is in MeV/cm. The coefficients 0.00155 and 0.00267 would have to be changed if LET were expressed different units, such as MeV per micron or MeV per (mg/cm^2). This is obvious because the term between square brackets must be dimensionless, like the Vt / Vb ratio.

LET is a parameter describing ionization density, dE/dx , in a given material. The general rule is that LET increases when the particle energy decreases, except when the kinetic energy becomes smaller than about 1 MeV/amu. For very low energies LET decreases when energies become smaller. In principle, the value of LET, in any given material, and for any given particle, such proton, deuteron, triton or alpha, can be calculated when its kinetic energy is given. The LET for alpha particles of 6 MeV, in CR-39, is said to be 3000 MeV/cm (in the caption of Figure 5, of the above reference). Note that LET=3000 MeV/cm is the same as 3000 / 1300 = 2.31 MeV per (mg/cm^2); 1300 is density of CR-39 is mg/cm^3. LET can also be expressed in terms of number of primary ions per unit length. It is well know, for

example, that it takes about 30 eV of energy to create a pair of primary ions in air. Thus 3000 MeV/cm = 300 MeV/mm, implies that 10^7 ions are formed along the path segment of 1 mm. Some references provide LET in terms of MeV*cm^2/mg, instead of MeV/cm.

Approximate LET values can be calculated from the range-energy data. Let me illustrate this by using the already shown data for air, at 20 C.

Energy (MeV).....Range(cm)
1.0 . . . . . 0.50
2.0 . . . . . 1.00
3.0 . . . . . 1.68
4.0 . . . . . 2.60
5.0 . . . . . 3.63
6.0 . . . . . 4.78
7.0 . . . . . 6.02
8.0 . . . . . 7.35
9.0 . . . . . 8.77

Looking at the last two rows, we see that a layer of air of 8.77-7.35 = 1.42 cm reduces the energy of alpha particles by 9 - 8 = 1 MeV. In other words the average rate at which energy is lost is 1/1.42 = 0.70 MeV/cm. I can assign this value of LET to alpha particles of 8.5 MeV. Likewise, the first two lines allow me to estimate LET of alpha particles of 1.5 MeV. The value is 1/0.5 = 2 MeV. In the same way LET = 1.09 MeV/cm at 3.5 MeV, etc. The LET versus E curve, with 8 data points, can be plotted on the basis of the above table. Note that density of air is about 1.3 mg/cm*3, Thus LET = 2 MeV/cm is the same thing as LET = 1.54 MeV*cm^2/mg. The values of LET for CR-39 can also be estimated (in MeV/micron and then in MeV*cm^2/mg) on the basis of the range-energy data for CR-39 (shown earlier in this unit).

Appended on 5/11/08
a) I found an interesting paper in Review of Scientific Instruments (78, 013304, 2007, pages 1-14). The title is “Study of saturation of CR39 nuclear track detectors at high ion fluence and of associated artifact patterns.” The authors are: by S. Gaillarda, J. Fuchs, N. Renard-Le Galloudec and T. E. Cowan.

b) Is it possible that our so-called "chopped meat" structures (in SPAWAR type experiments) results from extremely high fluence of nuclear particles? Below is a brief summary of what is new and interesting to me.

c) A CR-39 chip is exposed to an Am-241 source (activity 4 micro-curies) for 20 seconds, from a distance of 2 mm. Then the chip is etched for 30 minutes only. The source diameter was 4 mm and the track density was found to be essentially gaussian, along the diameter of the irradiated spot . Tracks were tiny and their overlappings were rare. The track density, in the center, turned out to be 0.010 tracks per square micron. Most tracks were confined to a circle whose diameter was about 9 mm, as shown in their Figure 2.

d) The purpose of this preliminary step was to learn how to control fluence (particles/cm^2) in the central region by changing the exposure time. Note that track density of 0.010 per square micron translates into 10^6 tr/cm^2. For ten times shorter exposure the fluence would be 10^5; for ten time longer exposure, it would be 10^7 particles/cm^2.

e) Experiments with a mylar filter were performed to show that CR-39 is not at all sensitive to X-ray photons, or to photoelectrons they produce. This was done by using a mylar filter that was thick enough to stop all alphas but negligibly thin for ~60 keV photons emitted by Am-241).

f) Saturation is defined as a situation in which overlapping of tracks is common, as illustrated in Figure 3c and 3c.’ Saturation can be caused by excessive fluence (particles/cm^2). For short etching times (when the layer of the etched away CR-39 is thinner than the range of particles) saturation starts to play a role when fluence exceeds ~10^6 particles per cm^2. The range, R, for alpha particles of 5.5 MeV, is 30 microns. Thus, assuming Vt=3 microns/hr, one can say that the etching time is short when it does not exceed R/Vt=30/3=10 hrs. I think that the term “saturation” is

appropriate for some regions of SPAWAR data. During the 2007 Catania conference, two scientists referred to these regions as “chopped meat.”

g) Figure 5 (pit diameters along the y axis and fluence along the x axis) is presented to show when tracks are saturated and when they are directly countable. Commenting on gray and white regions (on that figure), the authors write: “The valid region, shown in gray [lower left corner], can supply quantitative information (in terms of fluence), since the track diameter is below the track saturation diameter and the particle fluence remains low. In the zone left in white, the detector is saturated, since the tracks are too big and/or the fluence is too high. CR39 cannot be relied upon anymore to extract any quantitative information, but if carefully processed and analyzed, qualitative information (in terms of angular distribution of the ion beam) may still be extracted. Figure 5 is valid for any type of particle, since it represents the track diameter as a function of fluence. . . .

On the other hand, in the region limited by the dotted counter, which represents a zone for which the detectors are saturated, qualitative information can only be obtained in terms of beam distribution. Outside of these two zones, the detectors are in saturation and therefore do not provide trustworthy data anymore. At this point, optical ring structures start forming. The region with the dashed contour corresponds to a highly saturated zone, in which bull’s eye structures appear. The region limited by a dot-dash contour corresponds to a region for which a clumping pattern (see below) forms in the central region of the detector (including the white ring region).”

h) Figure 6 shows about 100 small pictures obtained when etched CR-39 chips were placed on the flatbed scanner whose resolution was 1200 dpi (21 microns between pits). That seems to be an appropriate way of examining saturated regions. Each picture is identified in terms of two known parameters, the fluence (between 5*10^6 and 5*10^10) and the etching time (between 18 min and 78 min). It would be desirable to see that, for a given etching time, an optical parameter, such as mean grayness, were proportional to the fluence. Unfortunately, this is not the case. The dependence of the mean grayness on the fluence is not even monotonic. This is illustrated in Figure 8.

i) Another technique used to study saturation regions was Atomic Force Microscopy. Such instruments are described, for example, at

http://en.wikipedia.org/wiki/Atomic_force_microscope

j) The second half of the paper reports on what was observed via microscopic examination of saturated regions. Some of the figures resemble what I observed in the examination of etched CR-39 detector from our The Galileo Project experiment. But my etching time was 6 hours while this paper is based on experiments in which etching times were less than 1.5 hours. This, however, does not exclude a possibility that fluences in SPAWAR-type experiments are much higher than what one measures by counting tracks in the non-saturated regions? Perhaps fluences were high enough to be compatible with reported amounts of helium produced diring excess heat experiments.

P.S.

k) According to http://en.wikipedia.org/wiki/Fluence,

“In physics, fluence [F1] or integrated flux is defined as the number of particles that intersect a unit area.” But the authors seem to use this term differently. Perhaps their fluence, F2, should be multiplied by a large factor, M, to obtain F1. Suppose they identify F2 with the number of tracks per cm^2, in the preliminary experiment (when tracks do not overlap). If this is so, then particles whose angle of incidence are larger than 22 degrees are ignored. This would make F2 considerably smaller than F1. To estimate the order of magnitude of M, defined as F1/F2, let me assume that the source diameter is large (much larger than 4 mm). In that case the source area could be treated as a set of point sources. For each point source, the factor M is nothing else but 4*Pi (total solid angle) divided by the solid angle A, within the half-angle of 22 degrees. In this case:

A=2*pi*[1-cos(22 degr)] = 6.28*0.0728 = 0.45 stereoradian.

Within my approximation, M =12.56 / 0.45 = 27.5. Thus if F2 is specified as 10^8 p/cm^2, then F1= 2.7*10^9 p/cm^2. This is for an ideal situation (well defined geometry, etc.)

l) What we are interested in is the total number of particles emitted during an experiment. Some of these particles are emitted in the direction of the cathode. They are not intercepted by the CR-39 detector. The source itself is not thin and a very large fraction of particles, emitted toward the detector, is absorbed in the source. Likewise, a very large fraction of particles, emitted toward the CR-39, is absorbed in the electrolyte. For that reason, an estimation of the total number of particles, emitted during an experiment, is not going to be easy. Only Monte Carlo simulations can help us in this task, even if F2 could be accurately determined, for example, on the basis of the mean grayness. But reliability of such simulations cannot be higher then reliability of assumptions one must make. One think is clear; the estimated number of tracks produced during electrolysis is likely to be a tiny fraction of one percent of all particles emitted during a SPAWAR-type experiment.

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347) Interesting New SPAWAR Results

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April 8, 2008

1) Introduction
A report presented by PA. Mosier-Boss, S. Szpak and F.E. Gordon, entitled "Pd/D Co-Deposition: Excess power Generation and its Origin," can be downloaded from http://www.lenr-canr.org/acrobat/MosierBosspddcodepos.pdf It was presented at the meeting of American Chemical Society. In this unit I want to share my own notes and comments based on this interesting report, and on a subsequently published paper. First the authors describe the electrolytic method for fast loading of deuterium into palladium. They have been using this method for many years. What evidence do we have that excess heat, measured by authors, has nuclear origin? They say that "amounts hundreds of times greater than can be delivered by any known chemical reaction/process, constituted proof of its nuclear origin. To verify that the heat generation is indeed nuclear in origin, nuclear ash needs to be detected."

The term "nuclear ash" stands for production of nuclear products in the amount matching excess heat. As I wrote in Unit 345, only helium can be considered a "nuclear ash." Production of tritium, at the rate of about 5000 atoms per second, studied by the authors, is a convincing indication that a nuclear process was going on. But, even that rate was not sufficiently high to produce measurable excess heat.

Another indicator of a nuclear process due to electrolysis was production of elements, such as Al and Mg in some spots on their cathode. These elements were identified on the basis of characteristic X-rays emitted under the bombarded by electrons. Such elements, the authors wrote, "could not be extracted from cell components and deposited at discrete sites. Furthermore it is thermodynamically impossible to electrochemically plate out metals such as Al and Mg from aqueous solutions."

2) Why am I mostly interested in what the SPAWAR team wrote about data gathered with CR-39 detectors? Because I already used their CR-39 protocol, as described in units 319 and 337. That protocol consisted of wrapping the cathode wire around a CR-39 chip, etching the chip after the experiment, and observing tracks that were produced during the electrolysis. My results were nearly identical to theirs. But my conclusion was that most of the observed tracks were too large to be due to alpha particles. The SPAWAR team did not agree with that conclusion. See item #10 in

http://newenergytimes.com/news/2007/NET21.htm#apsreport

3) In a discussion on the Internet list for CMNS researchers Ludwik wrote:
The ACS report does not allow one to compare tracks produced during electrolysis with tracks due to alpha particles from a radioactive source, under identical etching conditions. Fortunately, such comparison became possible on the basis of information they published in European Physical Journal, Applied Physics (Vol. 40, p. 293–303, December 2007). The title of that paper is: "Use of CR-39 in Pd/D Co-deposition Experiments" and the authors are Pamela Mosier-Boss, Stanislaw Szpak, Frank Gordon and Larry Forsley. Let me also mention that some pictures to which the paper refers are in the online supplement downloadable from:

http://www.epjap.org/index.php?option=article&access=standard&Itemid=129&url=/articles/epjap/olm/2007/12/ap07222/ap07222.html

Figures S1 and S2, shown below, were copied from that supplement.

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Figure S1
Picture of CR-39 tracks (after 9 hours of etching) due alpha particles from 241Am. Macroscopic magnification was reported as 1000.
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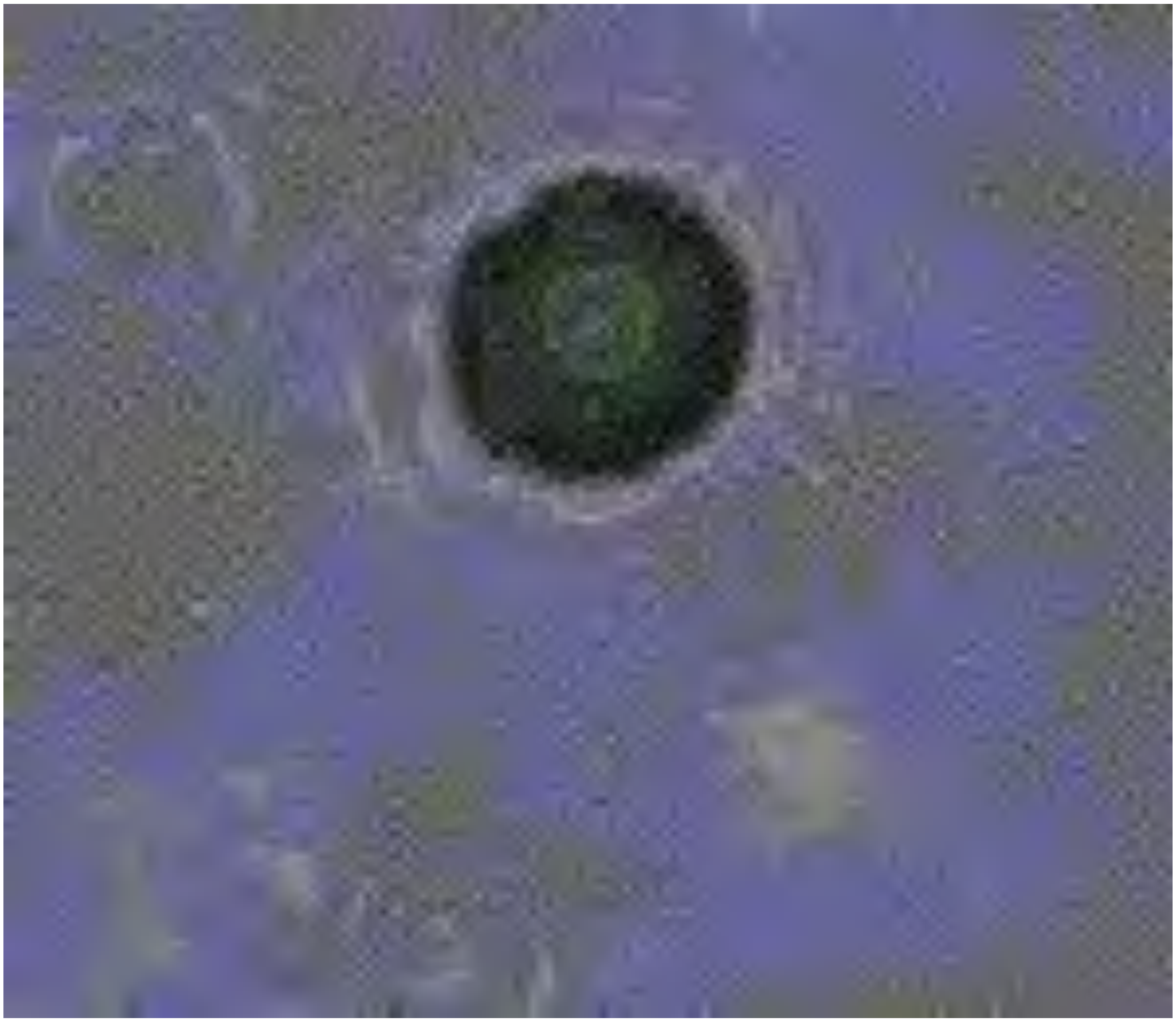

Figure S2
Picture of a CR-39 track (after 9 hours of etching) due an particle emitted during electrolysis. Macroscopic magnification was reported as 1000.

= =

Macroscopic magnifications under which these pictures were taken was 1000. On that basis I assumed that scaling factors were identical. The photographs shown in the supplement also showed the same tracks after 12, 16 and 20 hours of etching. Figure 3 shows how the widths of tracks changed with time of etching. The size of a track is defined as its diameter, when the track is circular, or as its width when the track is more or less elliptical. The length of the major axis depends on the angle of incidence, its width does not depend on that angle.

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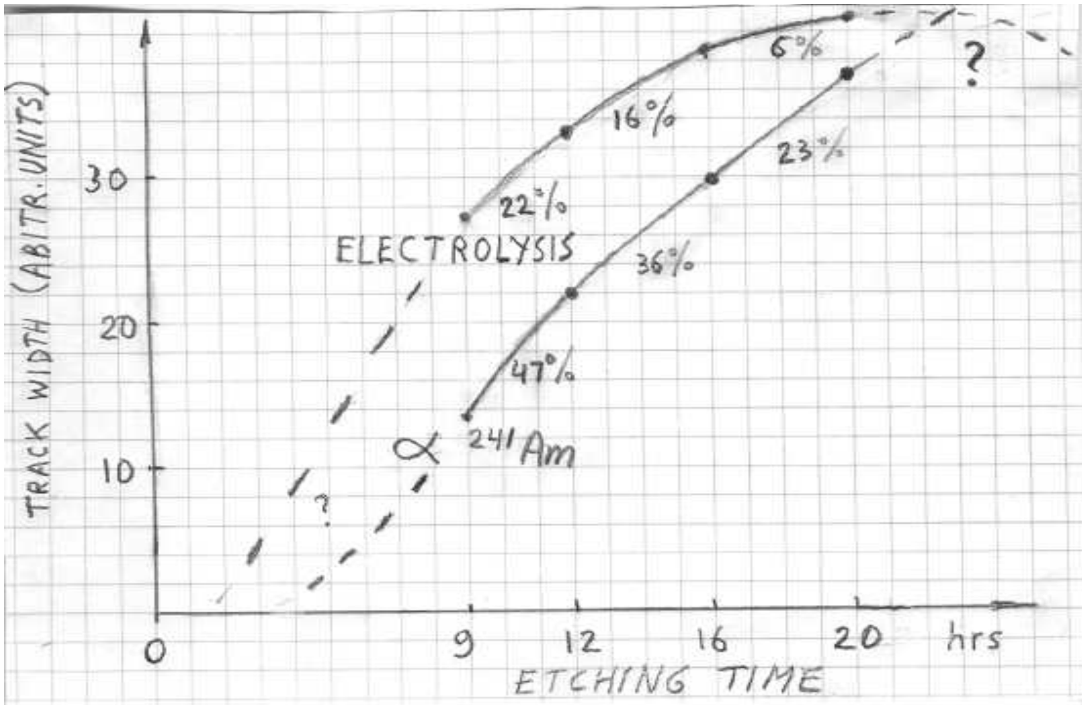

Figure 3

Depenedence of the track sizes on time of etching. For example, what does the 23% represent? The total width of an alpha track, was 30 units after 16 hours of etching and 37 units after 20 hours of etching. The ratio, 37/30=1.23, shows that the increment was equal to 23% of the final size.

= =

In what follows I will explain why, in my opinion, the large track shown in Figure S2 should not be attributed to an alpha particle of ~1 MeV, as tentatively assumed by SPAWAR team. Figure 3 and Figure 4 will be used to justify my position.

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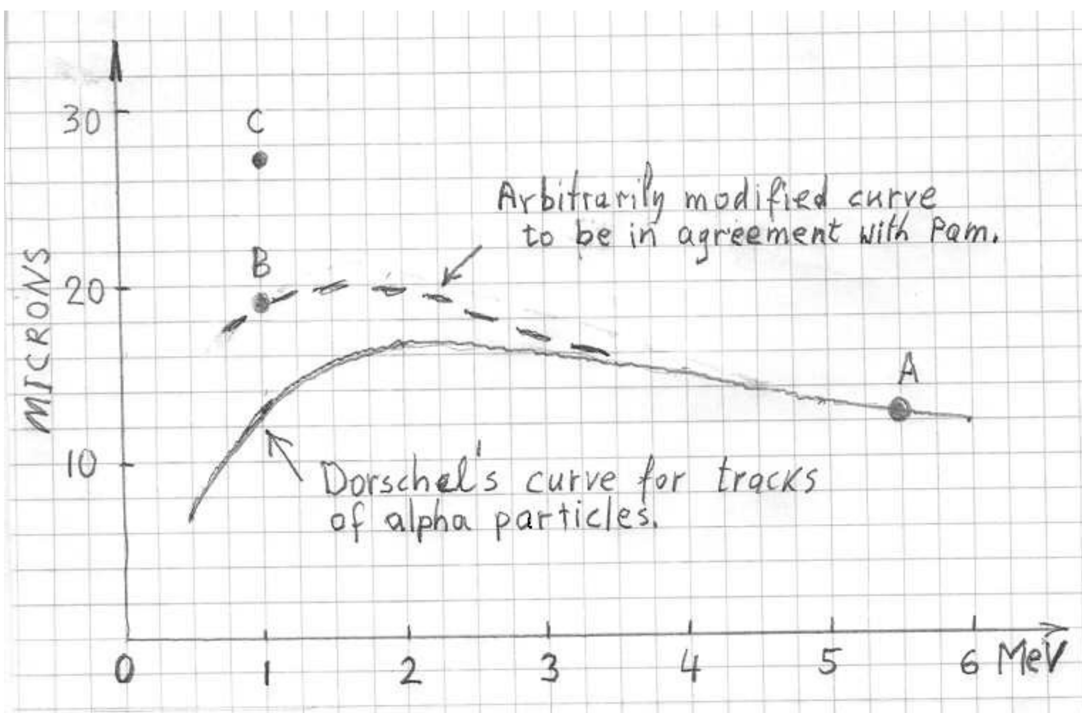

Figure 4

Dorschel's calibration curve for alpha particles (smooth line) versus a hypothetical calibration curve (dashed line). (See, for example, C. Brun, M. Fromm, M. Jouffroy, P. Meyer, J.E. Groetz, F. Abel, A. Chambaudet, B. Dorschel, D. Hersmdorf, R. Bretschneider, K. Kander, and H. Kuhne; "INTERCOMPARATIVE STUDY OF THE DETECTION

4) I am glad that SPAWAR people [Pamela Mossier-Boss et al.] showed exactly the same pits after different etching times. This is not always easy when the pit density is very high. Comparing widths of pits in Figure S1 with the diameter of the top circular pit in Figure S2, I see that pits due to alpha particles are smaller that pits due to particles emitted during the electrolysis. That is roughly consistent with my report at the APS meeting. The conclusion was that particles emitted during electrolysis must be different from alpha particles. Figure S1 and S2 (and Dorschel's calibration curve in Figure 3) seem to confirm this conclusion. . . .

5) Let me be more quantitative. (*) Tracks in Figure S1 are due to alpha particles of 5.5 MeV (241Am). (**) According to Dorschel, the maximum width of tracks due to an alpha particle can be no more than 25% larger than the width due to alpha particles of 5.5 MeV. (***) Zooming on Figures S1 and S2 I found that the width of the top track in Figure S2 is ~2.2 times larger than the mean width from seven tracks in Figure S1 (due to 5.5 MeV alphas). That is why I am claiming that the top track in Figure 2 cannot possibly be attributed to an alpha particle. (****) I will assume that the size of the top track in Figure S2 is typical; sizes of tracks produced during my electrolysis also excedded sizes of tracks due to alpha particles by a factor larger than two.

SPAWAR photos in Figures S1 and S2 also show that the top track in Figure S2 seems to be shallower than tracks due to alpha particles. I discovered this when I plotted the dependence of the track's widths on etching time (see the two curves in Figure 3). The upper curve is for the top track from Figure S2 while the lower curve is for the alpha particle track from Figure S1. The percentages indicate increments in widths between consecutive data points, as explained in the caption below the figure. The arbitrary units, along the vertical axes were millimeters, on my screen (plus or minus one), after zooming.

6) In general the width of a track (or the diameter when the track is circular) is known to increase with the time of etching. But what should one expect after very long etching? My expectation was that tracks should start disappearing; less deep tracks should disappear before more deep tracks. Consider, for example, alpha particles of 2 MeV (range ~8 microns in CR-29). Their tracks should disappear after about 7 hours. But tracks due to 5 MeV alpha particles (range ~30 microns in CR-39) should disappears after about 25 hours. These etching times were calculated from the known bulk etching rate of CR-39, about 1.2 microns per hour.

7) Ludwik wrote (not posted):
How can long etching times (deep pits) be reconciled with the idea that tracks created during electrolysis are due to alpha particles of ~ 1MeV? SPAWAR team, as far as I know, is currently working under such assumption (hypothesis).

8) Ludwik wrote:
a) According to Dorschel's calibration curve, the track widths for 1 MeV alphas should be smaller than the tracks of 5.5 MeV alphas. But the width of the top track in Figure S2, is twice as large as for a track of an alpha particle of 5.5 MeV in Figure S1. Something is not right somewhere. Do you agree?

b) I think that the top track in Figure S2 might be due to something much heavier, and more energetic, than an alpha particle. It might be a track of a fission fragment. I know, from literature, that CR-39 tracks due to FF are about two times larger that for alphas of several MeV. But that is not the only reason for suspecting fission fragments. Another part of my consideration has to do with the depth of tracks, rather than with their sizes seen from above the chips. The top track in Figure 2 keeps growing after after 20 hours. This indicates that its depth is at least 1.2*20=24 microns. How can such depth be attributed to an alpha particle whose range in CR-39 is known to be about 4 microns? Alpha particles of ~1 MeV are probably dissolved in hot NaOH after about four hours of etching.

c) Let me share another observation. According to Figure 7 (also from the online supplement, but not shown here),

widths of tracks due to particles created during electrolysis are significantly smaller than the widths of tracks due to alpha particles. Doesn't this conflict with conclusions reached from the photos in Figure S1 and S2?"

9) Pamela discovered one of my mistakes.
I assumed, in comparing sizes of tracks in Figures S1 and S2, that identical magnifications imply identical scales. She remesured diameters and informed us that the diameter of the top track in Figure S2 was 7.5 microns while the diameter of an alpha particle track, in Figure S1, was typically 5 microns. Responding to this I wrote: "OK, the ratio of track widths, after remeasuring, becomes 1.5 instead of 2.2. That is a big difference. With the ratio of 1.5, one can no longer ignore the question of how the widths of tracks were determined by Pamela and by Dorschel. Systematic errors, or differences in how the widths were defined by Pamela and Dorschel, become significant. Such things can indeed be responsible for the interpretational disagreement between Pam and myself."

10) So let me accept Pamela's interpretation -- the top track on the Figure S2 is due to an alpha particle of ~ 1 MeV. This is still not consistent with the Dorschel's curve. Figure 4 shows how Dorschel's curve (smooth line) was arbitrarily modified (dashed line) to be in agreement with Pam's hypothesis. Note that differences between the two curves are more pronounced in the very low energy region, that is in the region in which tracks become more and more shallow. Point A shows Dorschel's diameter for alpha particles of 5.5 MeV. Point B corresponds to the diameter which is 1.5 times larger than the diameter of an alpha particle of 5.5 MeV The dashed curve would have to pass through the point B to justify Pam's hypothesis. Why am I saying this? Because the diameter of her top track in Figure S2 is said to be 1.5 time larger than the width of a track due to an alpha particle of 5.5 MeV. Point C corresponds to the diameter which is 2.2 times larger than the diameter of an alpha particle of 5.5 MeV If the scale correction was not made by Pamela, then the dashed curve would have to pass through the point C, in order to accept her interpretation.

11) I am not going to argue about whether or not my arbitrary correction of Dorschel's curve (the dashed line) makes sense. Why? Because I have no information about the accuracy of his data. Neither do I know what curve is built into the simulation program used by Pam. The region of very low energies is difficult to explore. Is the error mostly on Dorschel's side or on SPAWAR's side? I do not know. The whole point of this exercise is to show what I have to do in order to accept Pamela's interpretation. Point B is much closer to the reported curve than the point C. That is why I am now willing to give Pam all the benefit of the doubt, as far as my first argument is concerned.

12) Ludwik wrote:
But my second argument, against her tentative hypothesis, seems to remain valid. Why was it ignored so far? That argument was based on the fact that the diameter of the top track in Figure S2 was still growing after 20 hours of etching. How can this experimental fact be reconciled with Pam's interpretation? The range of alpha particles of 1 MeV in CR-39 is only about 4 microns. This, as I already indicated, is much less than the layer of CR-39 that is expected to be dissolved in hot NaOH after 20 hours of etching.

13) Michel Jullian wrote:
. . . I may have the answer to your second objection to Pamela BTW. When I read your sensible suggestion that experimenting would resolve the dilemma it occurred to me that such experimenting might have been done, so I did this quick Google search:

http://www.google.com/search?num=50&hl=en&safe=off&q=alpha+0.5..1-mev+depth+etch+time+cr-39&btnG=Search

and found this 2007 paper, authored by Nikezic and Yu among others:

http://www.cityu.edu.hk/ap/nru/pub_j167.pdf

If I am not mistaken, it says that 1MeV alpha tracks are indeed visible after 15h (cf table 1) etching in a 6.25 N aqueous solution of NaOH at 70 C (cf abstract), in the form of 3 to 4 μm diameter hemispherical pits (cf table 2 and fig.3)

14) Ludwik wrote:

I printed this paper, after reading it with great interest, about two weeks ago. But the snip of information to which you refer did not catch my attention. I was mostly intereseted in the method by which profiles of tracks were deterined. Looking at what is shown on page 267, I agree with you that Pamaela's hypothesis is much more defendable than I thought. I am happy to withdraw my second objection. What is good for SPAWAR team is good for all of us. We are not competitors. My suggestion is that they replicate the experiment of Nikezic et al., if this has not already been done. Thanks for sharing; together we are stronger than each of us, individually.

15) Ludwik wrote (4/24/08):
a) I already suspected that Dorschel's calibration curve, on which my first objection was based, had a systematic error in the low energy region, as shown by the dashed line in Figure 4. But now I am even more inclined to accept this. Either something is wrong with his data or something is wrong the the SPAWAR hypothesis that alpha particles of ~ 1MeV are emitted from cathodes during electrolysis. Do you agree?

b) Unfortunately, I cannot find the reference from which Figure 4 was taken. It was posted on this list by Haiko, about one year ago. Perhaps he will share the reference in which the calibration method is described. Were his alpha particles from an accelerator or were they from a radioactive source? If he used a source then energy was probably controlled by absorbers. Was he the only one to calibrate CR-39 for alpha particles below 2 MeV? If not then how did his results compare with results of others? I wish my ability to find information by googling was better. Please help.

c) And here is a new question. Is it reasonable to think that helium ash, produced during excess heat experiments, comes from alpha particles detected by SPAWAR team? This question is worth addressing. Suppose that excess heat is generated at the rate of 1 W. How many particles must be emitted each second? We know that each alpha particle contributes 23.6 MeV of energy (3.78 pJ). Thus the number of particles emitted per second must be 1 / 3.78*10^-12 = 2.64*10^11.

Let me make a rather unrealistic (and too optimistic) assumption -- tracks are uniformly distributed over the CR-39 area of 1 cm^2. It is easy to show that the average distance between tracks of particles would be d(cm)=1/sqrt(N), where N is the surface density, in tracks per cm^2. Thus, N=2.64*10^11 gives d= 1.94*10^-6 cm or nearly 0.02 microns. Most tracks would be on top of each other, even if the CR-39 was exposed to the cathode for only one second. An exposure lasting 100 seconds would produce 100 times more tracks and the mean distance between tracks would be sqrt(100)=10 times smaller (0.002 microns). Yes, estimations of N are not likely to be easy.

d) But let us ignore this, for a moment. Suppose three SPAWAR type experiments were performed and excess heat was measured. The results were 100, 10 and 1 Joule. Suppose that CR-39 chips were used in these experiments. Wouldn't it be desirable to estimate values of N for each of these experiments. Yes, indeed. If excess heat is due to cold fusion of two deuterons then each track would be associated with production of 23.6 MeV (3.8 pJ) of heat, and of one atom of helium. But, as shown above, experiments in which excess heat is measurable would produce chips with highly overlapping tracks. A new method of measuring N must be invented. Any idea?

e) Yes, I know, we are not yet ready for experiments of that kind. A less ambitious short-term goal, in my opinion, should be to offer a protocol for a reproducible-on-demand demo of a nuclear activity triggered by a chemical process. How close are SPAWAR people from being able to offer a persuading protocol to mainstream scientists? How reproducible their tracks have been so far? I know that different kinds of experiments were performed at SPAWAR, and many results were published. By "persuasive protocol" I mean a protocol for a single, "most reproducible experiment, demonstrating tracks of nuclear particles."

Appended on 4/29/08

14) According to my Unit #347, SPAWAR team works under the hypothesis that nuclear tracks, produced during their electrolysis experiments, are due to alpha particles of ~1 MeV. Referring to this I posted the following message on the Internet discussion list for CMNS researchers.

" Here is an idea based on that paper of Khayrat and Durrani. What would I do to confirm Pam's hypothesis -- that tracks produced during electrolysis are due to alpha particles of about one MeV?

a) First I would try to replicate the experiment of Khayrat and Durrani (using alpha particles of at least three energies:

5.5, 3 and 1 MeV and etching for two hours only). Suppose my results also showed that tracks due to alpha particles of ~1 MeV are nearly two times larger than those due 5.5 MeV.

b) In that case I would start testing the hypothesis. First one corner of a CR-39 chip would be irradiated by alpha particles from 241Am. Then I would conduct a codeposition experiment, using that chip, as described in SPAWAR last paper (*). But instead of etching the chip for 9 hours, I would etch it for 2 hours.

(*) Mosier-Boss, P., Szpak, S., Gordon, F., Forsley, L, "Use of CR-39 in Pd/D Co-deposition Experiments," European Physical Journal, Applied Physics , Vol. 40, p. 293–303, (Dec. 13, 2007)

c) After that I would compare sizes of tracks due to 241Am with sizes of tracks created during electrolysis. Suppose sizes of tracks created during electrolysis were nearly twice as large as those due to 241Am. That would be a confirmation of their hypothesis.

Does this make sense? I am assuming that tracks, attributed to nuclear particles, are nearly always produced in at least one kind of SPAWAR experiment. That seems to be the case, according to their publications."

15) In subsequent message, on the same forum, I wrote: "Suppose the following question is asked. Why are tracks due to ~ 1MeV alpha particles nearly twice as large tracks due to 5.5 MeV particles after 2 hours of etching while at much longer etching (for example, ~7 hrs) sizes of tracks are nearly the same (according to Dorschel)?

I already tried to answer this question.But here is a simpler answer. It is well known that the degree of damage created by an alpha particle in CR-39, is higher near the end of the latent track. The range of alpha particles of ~ 1 MeV is close to 4 microns while the range of 5.5 MeV particle is close to 30 microns. Two hours of etching is enough to reach the bottom of a latent track of a 1 MeV particle but not enough to reach the bottom of the latent track of a 5.5 MeV particle. The highly damaged region, of the 5.5 latent track, is not etched and that is why the track is not as large as it would be after etching for 6 or 9 hours.

This probably captures the essence of the explanation without addressing many details. I would very much like to know if simulated tracks, after 2 hours virtual etching, agree with experimental data of Khayrat and Durrani."

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348) A message from a student

Ludwik Kowalski
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April 21, 2008

1) Three days ago I received a message from an American college student. He wrote: “Dear Dr. Kowalski, My name is X and I am a student at Y. I am impressed with your website. You seem to have been investigating ideas in this field for several years.

I've read some introductory material from lenr-canr.org and read some of your numbered items (#1, #2, #3, #4, #22, #24, #25 and several others). I already have the seminal Fleischmann, et. al. paper but I'm writing to you hoping that your years of experience may help guide me (and other interested students) in finding those papers which you think are essential reading (studies).

The cf information available from various sources seems voluminous. I have tried to just starting reading whatever I find but I think a more directed approach would be more fruitful. Would you be willing to identify a few such studies that I should probably start with? I've already read some of the history, "pathological science" and surrounding controversial stuff and have not yet been dissuaded. I'm skeptical but still wanting to find the science, i.e., credible evidence. I'd like to focus on the studies and thought I'd turn to you for some advice. Any direction would be helpful, X”.

The message is authentic but I am deliberately using X and Y because no permission was asked to quote the message. That student is probably not the only one to wishing to learn about the ongoing CMNS controversy. Here is how i responded to this message (with the CC posted on the Internet list for CMNS researchers: “Dear X, It is difficult to advice without knowing your background. Here is what one of my colleagues suggested:

"Ed's reviews and summaries were most helpful when I first sought
information about CF:

http://www.lenr-canr.org/PDetail10.htm#2937

There's even a student's guide:

http://www.lenr-canr.org/PDetail10.htm#2944

And CF for Dummies:

http://www.lenr-canr.org/PDetail10.htm#2954

And the book of course."

Ed's book is an extensive review of the field. But the level of presentation is advanced. The review of that book, written by a Danish chemist, Dieter Britz, can be found at:

http://pages.csam.montclair.edu/~kowalski/cf/344britz.html

On the other hand, do not believe everything you might find on the Internet. Some people like to mix serious scientific issues with deceptive advertising, as in:

http://www.apfn.org/apfn/free_energy.htm

I strongly suspect that the device described at that unit never existed. The article was probably written to attract naive investors. Serious cold fusion researchers, like Ed Storms, are making progress and trying to understand how excess heat was produced by Fleischmann and Pont. The original claim for excess heat has been confirmed in more that 100 experiments. But, in most cases the rate of generating such heat was close to 1 watt. There are other indicators, in addition to excess heat, that nuclear processes of some kind can indeed be triggered by chemical processes. What we need is at least one "reproducible on demand" demonstration of this. Technological advances would probably be made quickly after basic science is understood. Keep your eyes open on this developing field, no matter what your long-term plans."

3) Shortly after sending the reply, I read a message posted by Steve Krivit. It contained a link to the list of books about CMNS that he compiled.

http://newenergytimes.com/Books/books.htm

I wish I saw this link earlier; it should be given to anyone interested in CMNS topics. References to papers which are likely written to promote fraudulent claims are also worth showing to students. Here is a quote from an article entitled "Inventor claims discovery of free energy," written by Kevin Smith (in 2002). The entire article can be downloaded from <http://www.apfn.org/apfn/free_energy.htm>

"In a demonstration for Reuters, a prototype -- roughly the size of a dishwasher -- was run for around 10 minutes using four 12-volt car batteries as an initial power source. Emitting a steady motorized hum, the machine powered three 100-watt light bulbs for the duration. A multimeter reading of the batteries' voltage before the device started up showed a total of 48.9 volts. When it was switched off, a second reading showed 51.2 volts, indicating that, somehow, they had been reimbursed. The machine went on to run for around two hours while photographs were taken, with no diminution in the brightness of the light bulbs, which remained lit during a short power cut. 'The draw on the batteries was estimated at more than 4.5 kilowatts. With any existing technology the batteries would have been drained flat in one and a half minutes,' the inventor said."

A knowledgeable student would probably notice that something is wrong with the last sentence. One of the parameters of a car battery is Ah, usually 50 Ah or more. Suppose that four 12 volt batteries are connected in parallel. What is the total current needed to support three 100 W light balls, of matching voltage? The answer, based on Ohm's law, is I=300/12 = 25 A. Each battery would contribute 25/4=6.25 A. Thus, in two hours, each battery would use only 6.25*2=12.5 Ah. This is only 1/4 of its assumed capacity, 50 Ah. On what basis should one expect a battery to "have been drained flat" under such conditions? The same question would be asked if batteries were connected in series, supporting three 36 volt light bulbs (of 100 W each). I see nothing unusual for using four car batteries to power three 100 W light bulb for 2 hours. The draw on the batteries powering three 100 W light bulbs is 300 W, not 4500 W.

Another claim, that most likely was a fraud, is mentioned in Unit #279, at this website. Let me end this unit by quoting from what I wrote in unit #236, at this website. "Getting more out of a physical system than is put into it has been a persistent dream of humankind. Those who have tried to do this honestly -- by inventing their own 'perpetual motion' machines -- have invariably failed. The only people who have come out ahead in his area are those who claimed success and then got others to pay to see it. That it seems, was not difficult to do. In 1812 Charles Redheffer traveled through Philadelphia and New York, charging a dollar admission (a dollar was a lot of money in these days) to see his perpetual motion machine made up of wheels, gears, and pulleys which kept moving continually with no apparent source of energy. He did very well until someone discovered a little man in a back room turning a crank.

Another method of making money is to 'invent' such a device and then get people to invest in it. So gullible are some people, and so anxious to get something for nothing, that they will put money into all kinds of money-making schemes." The iESi company was certainly not the first instrument to get money from greedy people; and most likely

not the last one. Even people who studied science are not always immune to preposterous claims. They know that many ideas that turned out to be good were initially rejected as nonsensical. The germ theory of disease, for example, was ridiculed when it was first introduced by Pasteur. Today bacteriology is an essential part of medical science.

Or think about Wright brothers' idea of airplanes. It was ridiculed by those who believed that only balloons "lighter than air." could fly. But the brothers did not give up and their persistence paid off. How can one deny this today when heavier than air machines fly all over the world? Dismissing an idea which seems to be in conflict with what is already known may or may not be justified. That is why scientists are often reluctant to reject unreasonable claims. Sometimes it pays off and sometimes it leads to disappointments.

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349) How are visible tracks created in CR-39

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April 30, 2008

Introduction
Being retired often means that one is able to turn a hobby into a full time occupation. Fortunately, I am still healthy enough to do this. So let me describe what I think about the CR-39 track-formation process. Ideas presented here are closely linked with contents of units 346 and 347. They represent an attempt to to develop my own simulation of etching. I will begin with a very simple model and try to make it more realistic, one step at a time. For the time being, each of my models is for latent tracks perpendicular to the surface of the CR-39 chip. What is a latent track? It is cylinder, containing damaged CR-39 material (broken molecules, etc. etc.)

I will use the following notation:

E <--- (in MeV) kinetic energy of the incoming alpha particle
L<---(in microns) the length of the latent track created by that particle.
Vt <---(in microns per hour) the etching rate in the damaged CR-39 material. The Vt is caller track etching speed.
Vb <---(in microns per hour) the etching rate in the CR-39 material undamaged by the particle. The Vb is called bulk etching speed.

Model 1 (not realistic):
Let me make some assumptions:
a) The damaged material is in a cylinder (centered on the latent track axis) of very small diameter, centered on the latent track axis.
b) The damaged material is uniformly distributed within the latent track cylinder.
c) Bulk etching speed, Vb, is zero while the track etching speed, Vt, is 4 microns/s.
d) L=16 microns

Since Vb=0 , the track diameter does not change with the time of etching. The dry track will consist of an air cylinder whose length is y = Vt * t, where t is the etching time smaller than 4 hours. (the time to dissolve all damaged material, t' = L / Vt = 16 / 4 =4 hours). Nothing changes after 4 hours of etching.

Model 2 (Vb > Vt or Vb=Vt):
No track will be seen because the CR-39 material, surrounding the latent track cylinder, will be dissolved faster than the damaged material. The invisible latent track will disappear in 4 hours if L=16 microns and Vb=Vt=4 microns per second.

Model 3 (starts to be realistic):
Assumptions are the same as in Model 1, except that Vb (smaller than Vt) is no longer zero. In order to produce conical tracks I will assume that vertical etching (direction parallel to the latent track) proceeds with the constant speed Vt while horizontal etching proceeds with the constant speed Vb. This crude approximation is probably valid for etching times that are not too long; experimentally determined track profiles (1) were found to be conical, for of not-overetched tracks. A track can be said to be overetched when the etching time is longer than t'= R / Vt, where R is the range of particles in CR-39. For alpha particles of 5.5 MeV, R is about 30 microns. Thus, t' is 10 hours, when Vt=3 microns per second. Profiles of overetched tracks, according to (1), tend to be come spherical and their radii seem to

increas at the Vb rate. This observation is part of Model 3. Fot t > t', the diameters of tracks are assumed to grow at the same rate as for lower t (see below).

Figure 1 shows the expected evolution of the visible track of a 5.5 MeV alpha particle. Consecutibe profiles refer to etching times of 2, 4, 10, 30 and 40 hours, as indicated. These profiles correspond to Vb=1.2 and Vt=3.6 microns per hour. Note up to t=t'=10 hours, all tracks are conical. The half-angle, A, at the tip of each cone, 18.4 degrees; the tangent of that half-angle is nothing else but the Vb / Vt ratio. The hight of a cone, y, for any given t < t' (t'=10 hrs) is given by

y = (Vt - Vb) * t. = 2.4 * t

The corresponding diameter, D, at time t, is given by

D = 2 * y * tan(A). = 1.6 *t

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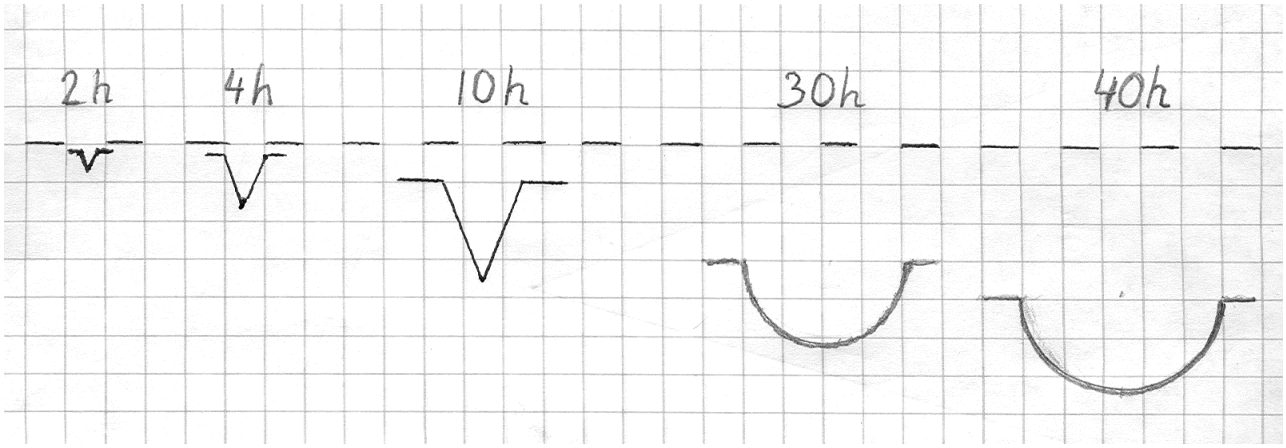

Figure 1
Cross sections of track profiles after different etching times. The dashed line shows where the top surface of the CR-39 chip was before etching. Two horizontal segments, at each profile, show where the CR-39 surface is after etching. The bottom of the 10 h cone coincides with the end of the invisible latent track. Note that after 40 hours of etching the latent track layer is totally disolved. The right-side pit, located below that layer, is growing radially at the rate of 1.2 microns per second.

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That observation made me think about SPAWAR results described in my Unit 347. http://csam.montclair.edu/~kowalski/cf/347new.html According to Figure 3, in that unit, the measured diameters of tracks produced by alpha particles from 241Am became 40 microns after about 22 hours. According to the above formula, D after 22 hours is 35 microns. That is not very different from 40. I consider this to be a good indicator that my model is reasonable. But it is too eraly to start playing with Vt and Vb, in order to get a perfect agreement.

* *

Model 4

Damaged material, surrounding a latent track, is probably not uniform. Material closer to the latent track axis is likely to be more damaged than material further away from the axis. Likewise, material closer to the end of a latent track is likely to be more damaged than material closer to the beginning of the track. I am saying this because I know how density of ionization is distributed along the path of an alpha particle in air. That topic has been studied by Bragg, more than one hundred years ago. In the Model 3, the Vt was assumed to be constant; in this model I will assume that it changes along the latent track.

I will simulate evolution of a track due an alpha particle of E=5.5 MeV (R=32 microns). Simulation steps will be 15

minutes each. The Vb will remain constant but Vt will be assigned a predetermined value for each step. In my simple computer program, the Vb will always be 1.2 microns per second. But my set of Vt values, for consecutive steps, will be treated as a set of adjustabl parameters. This should provide great flexibility. I would be able to see the cross section of a track at any desired etching time. To test the program (to discover and correct hidden errors) I will compare its output with Model 3. The results should be nearly identical, provided Vt=constant=3.6 microns/hour (as above), and time steps are very short, for example, one minute each. That is my plan. The results will be shown later.

Model 5
This model will be similar to Model 4 but it will have an additional assumption. The value of Vt will also have a radial dependence (larger values near the latent track axis and smaller values at larger distances from the axis.

Another thing worth doing is to perform experiments in which CR-39 chips are bombarded with alpa particles of several energies. The chips would then be etched for different times, for example, 2 hrs, 4 hrs, 8 hrs, 16 hrs and 32 hours. Plotting diameters of tracks versus etching time will produce curves to be compared with simulations. Yes, this is a big and ambiguous project. If all goes well, I might be able to publish the results. The project has nothing to do with CMNS; but results might be useful to CMNS researchers.

Appended on 5/1/08
Several things became intuitively clear to me, after I tried to compose an algorithm for Model 4. Let me enumerate them.

a) Strongly overetched tracks will have semi-spherical profiles, as in Model 3.
b) Suppose t' again stands for the etching time needed to dissolve the CR-39 layer in which the latent track was located. Profiles of tracks etched during times t shorter than t' will not be exactly triangular; they will resemble profiles of inverted Eiffel towers. But that is not a very big difference.
c) Time t' in Model 4 will be shorter than what it would be if the value of Vt were not increasing with the depth of the pit. The difference between t' of Model 4 and t' of Model 3 increases when the rate at which the Vt changes with the depth became larger.

I do not think that the overall results of Model-3 simulations will be very different from the results of Model-3 simulations. The diameters of strongly overetched pits will keep growing at the same rate close to 2 * Vb=2.4 microns per hour. The pits produced during the etching times shorter that t' will no longer be proportional to t. But consequences of this will be hard to measure. One thing is clear, Model 4 will not allow me to explain why diameters of tracks created during codeposition stop growing when the etching time is increased.

The same is likely to be true for the Model 5. Here too the diameters of profiles, of strongly overetched pits, should be increasing linearly at the rate close 2.4 microns per hour. Some nuances will probably be different at $t < t'$ but they will be difficult to notice. Intuition tells me that Model 5 will also not going to explain the experimental fact reported by SPAWAR team -- diameters of tracks, created during codeposition, stop growing when the etching time is increased.

Considering all this I decided not to commit myself to this project. What I learned so far gives me enough confidence to make the following observations.

a) Diameters of tracks due to alpha particles from 241Am increase with the etching time as expected. I know that I can simulate the reported curve; it is shown in Figure 3 at:

http://csam.montcllair.edu/~kowalski/cf/247new.html

b) The diameters of tracks created during codeposition, shown in the same Figure 3, stop growing when the etching time exceeds 20 hours. This experimental fact is not likely to be explained by my model. In fact, I suspect that the model of Nikezic and Yu, used by Pamela, also does not explain this experimental fact. But this is only a suspicion. Perhaps I am not aware of some important effects. Perhaps latent tracks created by alpha particles of ~ 1 MeV are very different from latent track due to common alpha particles. Something is not right somewhere. What is it?

References:

1) F.M.F. Ng, K.Y. Luk, D. Nikezic and K.N. Yu; “Determination of alpha-particle track depths in CR-39 detector from their cross-section and replica heights; Nuclear Instruments and methods in physics research B 263 (2007) 266-270.

2) A.H. Khayrat and S.D. Durrani; “Variation of alpha-particle track diameters in CR-39 as a function of residual energy and etching conditions." Radiation Measurements, vol. 30, Issue 1, (1999) pages 15-18).

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350) My notes based on SPAWAR paper

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May 3, 2008

INTRODUCTION
This unit contains notes I made for myself while rereading the SPAWAR paper (published in EPJAP, December 2007). I do not know why that paper is only listed in our CMNS library at

http://www.lenr-canr.org

instead of being downloadable from it. Perhaps it has something to do with copyright. So here is the reference again:

Mosier-Boss, P.A., et al., *Use of CR-39 in Pd/D co-deposition experiments.* Eur. Phys. J. Appl. Phys., 2007. **40**: p. 293-303.

The supplementary part of that paper, that was published online, is available (if you pay, or belong) from:

http://www.epjap.org/index.php?option=article&access=standard&Itemid=129&url=/articles/epjap/olm/2007/12/ap07222/ap07222.html

Part of what I wrote has already been written in Unit 347 but it is now better organized and better formulated, I hope. The bottom line is essentially the same -- I do not think that tracks created during SPAWAR-type experiments are due to alpha particles. That was the conclusion reached on the basis of my own experiment (as reported at the March 2007 APS meeting).

INTRODUCTION AND CONCLUSION (THEIR SECTIONS 1 AND 4)
1) The paper's introduction describes the CR-39 detection methodology and shows that authors are aware how it was used in several fields. They give credit to CMNS researchers who also detected unexplained particles, for example, 11-16 MeV alphas and 1.7 MeV protons (Lipson), huge clusters of tracks during light water electrolysis (Oriani and Fisher), and emission of particles when deuterium gas was diffusing through Pd (Li).

2) Here is the last sentence from the introduction: "Evidence is presented that shows that these **pits are tracks caused by the emission of charged particles** and that these tracks are not due to either radioactive contamination; or from the electrolysis of heavy water; or from chemical reaction with D_2, O_2, or Cl_2."

3) In the concluding Section 4, the authors wrote: "**The experiments are reproducible.....**Quantitative analysis shows that there are three populations of pits (0.1–0.5μm, 0.9–4.0μm, and 4.1–12μm) and that the pits can be either perfectly circular or elliptical in shape. These **features are consistent with those observed for nuclear generated pits**. In this communication, it has been shown that the pits formed during Pd/D co-deposition are **not due to radionuclide contamination** of the cell components nor are they caused by impingement of gas bubbles on the surface of the CR-39. Since electrochemical plating of $CuCl_2$ did not result in pits, production of pits in a Pd/D co-deposition experiment **cannot be attributed to chemical attack** of the surface of CR-39 by either D_2, O_2, or Cl_2

present in the electrolyte."

THE MAIN BODY OF THEIR PAPER (SECTIONS 2 AND 3)

4) Is the description of the protocol sufficient to guide someone? I think it is. In the simplest case the cathode was an Ag or Au wire (wrapped around the CR-39 chip) while the anode was a wire made from Pt. A nickel screen has also been used as a cathode on which the Pd/D was deposited.

5) Figure 2 shows the cathode surface from experiments without magnetic field (a) and with magnetic field (b). **The Lorenz force in the magnetic field is said to be responsible for structural surface differences. Electric field is also said to produce structural changes.**

6) By Lorenz force they certainly mean q*(vXB), where q is the charge of the ion, on which the force is acting, while vXB is the cross product of the ion's velocity, v, and the field strength, B. The field on the order of 2500 Gs, probaly measured with a gaussmeter, was created with two strong neodymium magnets, outside the cell.

7) The value of B between two magnets (inside the cell) is certainly not strongly affected by plastic walls from which the cell is made (see Figure 1) or by presence of the electrolyte. But the same is not true about the electric field, E. That field was created by applying 6000 volts to two copper plates outside the cell. The distance between plates was close to 3 cm. In the vacuum (or in air) the field strength would be only slightly smaller than 6000/3=2000 V/cm. But presence of electrolyte (a conductor) and plastic walls (dielectric) probably reduces E to less than 0.002 V/cm. How can such field affect the co-deposition process?

8) It is interesting that, in the case of a Ni cathode (Figure 3d), pits appear "only when either an external electric or magnetic field is applied." Nickel is known to be a ferromagnetic material. Is this significant? I do not know. The authors report that "the overall size and shape of the pits are similar to nuclear tracks reported by Oriani and Fisher [10], Li [9], and Lipson et al. [11, 12]."

9) The picture in Figure 3a shows the CR-39 but the picture of Figure 3b shows the photographic film. The film was protected from the cathode by mylar. Dark spots on the photographic film are believed to be due to X-rays emitted during electrolysis. Figure 3d shows copious CR-39 tracks. Some dark circles have diameters of about 4 to 8 microns.

10) **I am puzzled by Figure 3a**. Commenting on it, the authors conclude: "It is therefore possible, that the hollows observed in the CR-39 detector, Figure 3a, are the results of damage due to the soft X-ray emission." The density of ionization due to an X-ray photon is known to be very low, in comparison with that due to an alpha particle. What X-ray dose would be needed to produce a detectable damage in CR-39 material? Intuition told me that the dose must be very large. But how large?

Intrigued by this question I went to the reference 25 (Amemyia et al.,"Soft X-ray imaging . . . ", Nuclear Instruments and Methods B, vol 187, 2002, page 361) and found the answer. The dose has to be "more than kGy." The unit of dose, Gray, translates into 100 rads. In other words, 1kGy is 100000 rads. My recollection is that about 50% of experimental rats die shortly after receiving the whole-body dose of 500 rads. This is only 0.5% of one kilogray. Fortunately the danger to SPAWAR researchers was not as great as one might think. First the X-rays were highly localized; each hollow was about 1 mm, as shown in Figure 3a. Second, soft X-rays would be stopped inside the cell (or in the skin of a rat). Third, the film shown in Figure 3b would be much darker without the mylar film. How thick was the mylar film?

11) **Was the CR-39, shown in Figure 3d, also protected from the cathode by mylar?** Several CMNR researchers suggested (about a year ago, after phase 1 of The Galileo Project) that in some experiments CR-39 should be separated from the cathode by a mylar film of about 5 microns. Such film is commercially available; it is thin enough to transmit alpha particles. The film would protect the CR-39 surface from suspected chemical reactions at the cathode. This topic is discussed in Section 3.2, named "Nuclear tracks vs. chemical damage." The main argument is that tracks, as shown in Figures 5, are not different from tracks due to alpha particles from radioactive sources (Figure 4). Tracks due to chemical corrosion would be "lighter in appearance and irregular is shape." This is a powerful argument for the central CMNS claim -- a nuclear process of a new kind is triggered by electrolysis.

By the way, I also observed alpha-particle-like tracks produced during the co-deposition electrolysis. This was during the phase 1 of The Galileo Project. But I was troubled by the fact that tracks created during electrolysis were usually more than 2.5 times larger than tracks due to alpha particles from my 241Am source.

12) At the end of section 3.2 the authors refer to experiments in which CR-39 chips were etched incrementally. They conclude: “The incremental etching result support the conclusion that the pits observed in the CR-39 detector as a result of the Pd/D co-deposition process are nuclear in origin.”

13) The rest of the paper describes additional experiments. One learns, for example, what happens when KCl is used instead of LiCl, when light water is used instead of heavy water, when cathodes made from different metals are used, etc. etc. Was one particular setup found to be much better that the setup described in the original protocol, nearly two years ago? Are authors ready to offer the world a protocol for a simple and convincing demonstration of a nuclear activity caused by a chemical activity? I hope they are. I would be happy to participate in the new phase of The Galileo Project.

COMMENTS ON INCREMENTAL ETCHING AND ON RATIOS OF DIAMETERS

14) Let me comment on results from incremental etching experiments, mentioned in point 12 above. These results confirmed my old conclusion -- tracks created in SPAWAR-type experiments cannot possibly be due to alpha particles. That is what I reported at the APS meeting in Denver. The recent EPJAP paper offers a chance to return to this topic on the basis of new SPAWAR results.

15) Two photos below are from Figures S1 and S2, of the online version of SPAWAR paper.

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Figure S1
Picture of CR-39 tracks (after 9 hours of etching) due alpha particles from 241Am. Macroscopic magnification was reported as 1000.

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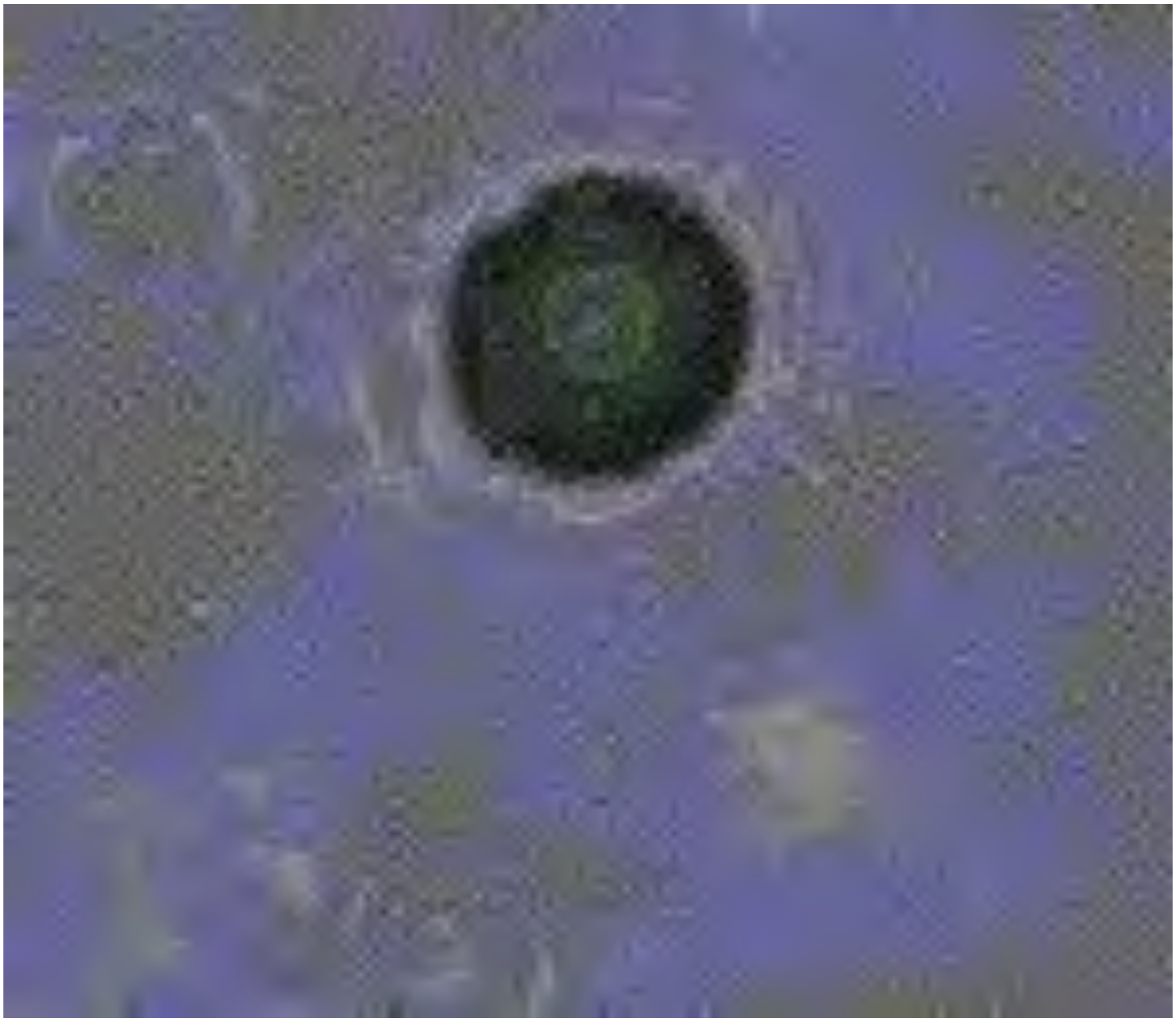

Figure S2
Picture of a CR-39 track (after 9 hours of etching) due an particle emitted during electrolysis. Macroscopic magnification was reported as 1000.

= =

Macroscopic magnifications under which these pictures were taken was 1000. The photographs taken from the supplement also showed the same tracks after 12, 16 and 20 hours of etching. Fortunately, the same chip was used to record alpha particles of 5.5 MeV (near the CR-39 corner) and particles emitted during electrolysis (in the CR-39 center). Comparing sizes of these two kinds of tracks would be much more difficult if etching conditions were not identical. Figure 3 shows how the widths of tracks changed with time of etching. The size of a track is defined as its diameter, when the track is circular, or as its width when the track is more or less elliptical. The length of the major axis depends on the angle of incidence, its width does not depend on that angle.

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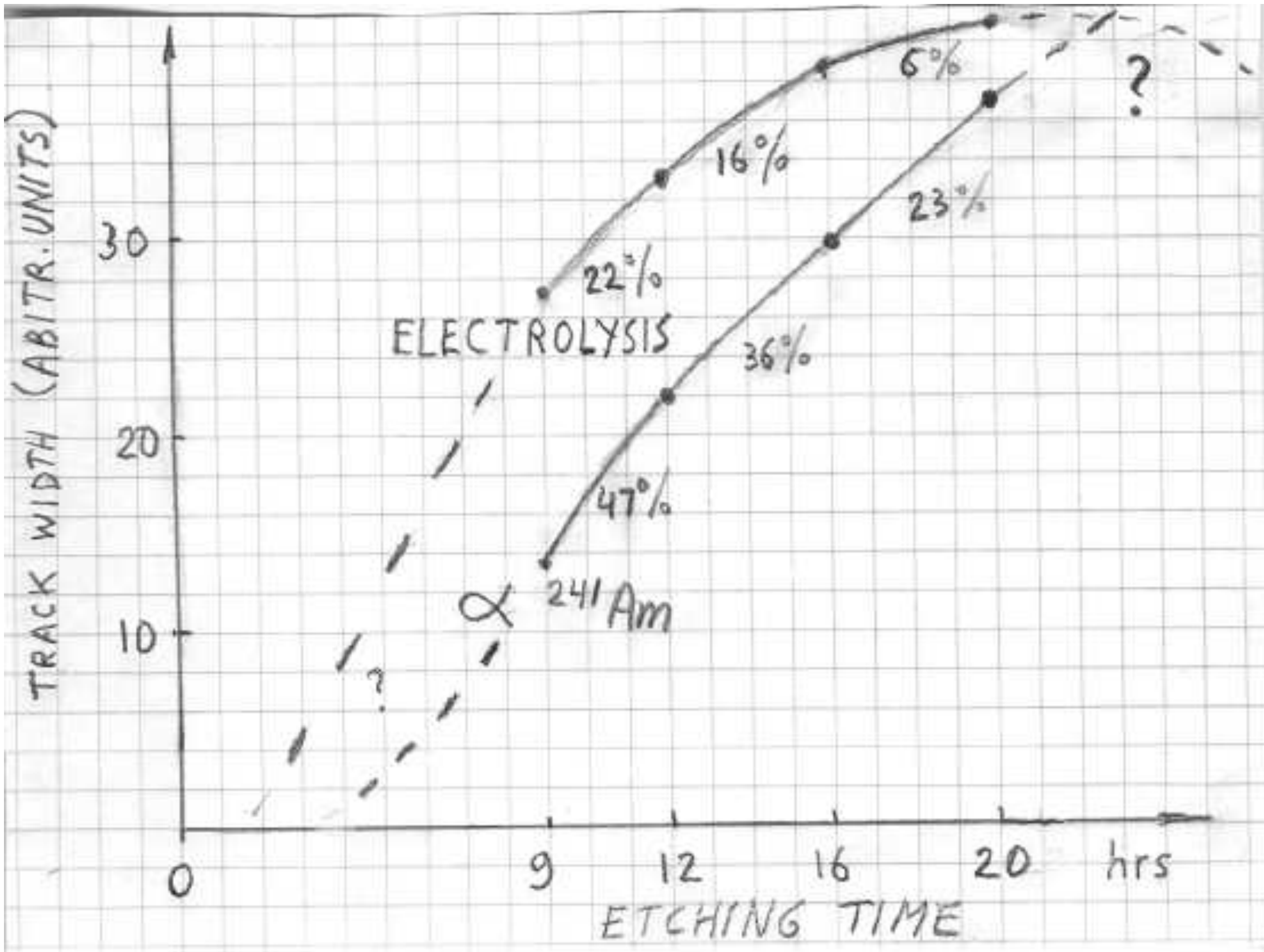

Figure 3
Depenedence of the track sizes on time of etching. For example, what does the 23% represent? The total width of an alpha track, was 30 units after 16 hours of etching and 37 units after 20 hours of etching. The ratio, 37/30=1.23, shows that the increment was equal to 23% of the final size.
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16) In what follows I will explain why, in my opinion, the large track shown in Figure S2 should not be attributed to an alpha particle of ~1 MeV, as tentatively assumed by SPAWAR team. Figure 3 and Figure 4 will be used to justify my position.

One thing becomes obvious at once; sizes of tracks created during electrolysis tend to stop growing, after 20 hours of etching, but tracks due to alpha particles from the radioactive source keep growing. This difference seems to be very important. According to recognized CR-39 experts, such as Nikozic and Yu (see reference in my unit 346), diameters of overetched tracks due to nuclear projectiles keep growing infinitely, at the 2*Vb rate. The value of Vb depends on the etching conditions (temperature and molarity of NaOH); Vb is usually between about 1 and 2 microns per hour. SPAWAR experimental curve for alpha particles seems to be consistant with the continous growth of the diameter. The average rate of growth, between 9 and 20 hours, is close to 2 microns per hour; this corresponds to Vb=1. Note that the etching temperature of the SPAWAR etching solution was said to be between 65 and 72 degrees C. Drifts in the temperatur could be responsible a slow chage of Vb.

But how to explain the second experimental fact? Why does the diameter of the upper curve tend to stop growing after 20 hours? I do not know how to answer this question. But one thing is clear; the second experimemental fact is not consistent with the idea that tracks produced during electrolysis are due to alpha particles emitted from the Pd/D deposit on the cathode. That argument has nothing to do with the argument I used before to justify the same conclusion.

17) Let me now return to the old argument; it was based on comparisons of track sizes. Would the same argument be

defendable in light of new experimental data? I think it would, considering what I wrote in Unit 346, at this CF website. A quick look at tracks in Figure S1 and S2 shows that their sizes are very different. At first I thought that the diameter of the track in Figure S2 was more than twice as large as the width of a track in Figure S1. Both photos were taken at the magnification of 1000. That would be close to what I found more than one year ago. But I learned, from one of the authors, about additional zooming factors. The true diameter of the pit in S2 was close to 7.5 microns while the true width of tracks in S1 was close to 5 microns. The question becomes: is the ratio of diameters equal to 1.5 consistent with my claim? It was easier to denend the claim on the basis of my own experimental data, when the ratio of diameters was close to 2.5. But now I am better prepared to adress the question. Before I was guided by intuition, now I can be quided by experimental data of those who studied how diameters of alpha particle tracks depend on their energies.

The figure below shows experimental data of that kind. It was found in a paper (1) published by two teams of researchers, one from France and another from Germany.

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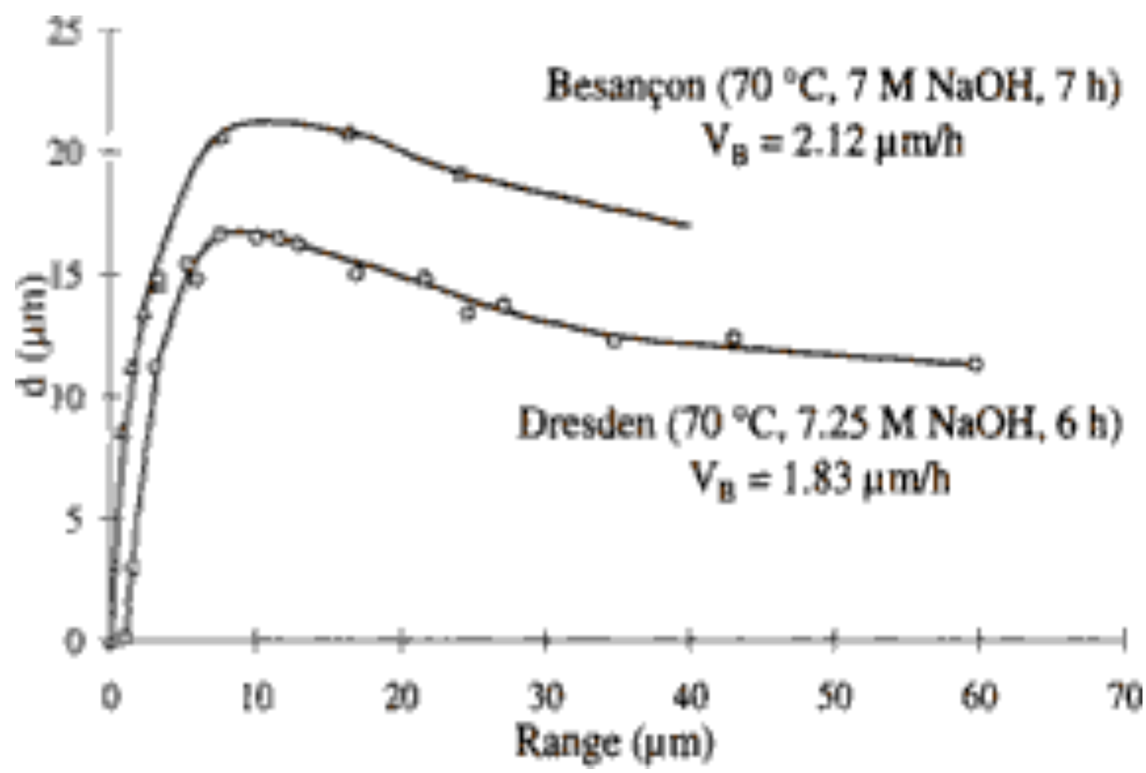

Figure 4
Diameters of etched alpha particle tracks in CR-39 versus their ranges. Even though the etching conditions are not the same for the two teams, the shape of the curves are very similar.

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18) Slightly different etching conditions in two laboratories explain differences in absolute diameters. But positions of maxima (at R~10 microns) are nearly identical. Also nearly identical are ratios of diameters, for example diameter at R=10 and at R=25 microns. In what follows I will use the lower curve because is covers a wider range of experimental data. To convert ranges in CR-39 into energies I will use the following data:

Alpha energy . . . Range in CR-39

5.5 MeV ....33 microns
4.0 MeV ....20 microns
2.4 MeV ....10 microns

1.6 MeV .....7 microns
0.8 MeV .....3 microns
0.0 MeV .....0 microns

The Dresden curve shows that the diameter of tracks af 5.5 MeV is about 13 microns. The diameter at the maximum, corresponding to 2.4 MeV, is about 15.2 microns. That shows that the diameter of an alpha particle track can exceed the diameter of an alpha particle of 5.5 MeV by no more than a factor of 15.2/13=1.2. That is considerably smaller than the factor 1.5 reported by the SPAWAR team, suggesting that the track in Figure S2 should not be attributed to an alpha particle. But the difference between 1.5 and 1.2 is not enormous. How reliable are Dresden data? The precision in measuring diameters was certainly very high, otherwise the data point would fluctuate more widely around the smooth curve. I read the paper and I am satisfied with the method by which energies were determined. My guess is that the uncertainty about the 1.2 ratio is smaller than +/- 0.05 (which is 4%). The uncertainly associated by the 1.5 factor is probably close to 16%. To calculate that value I simply assumed that the uncertainty in each diameter is 0.5 microns.

19) These considerations lead me to a conclusion that the difference between the ratios of diameters (1.5 versus 1.2) cannot be taken too seriously, in the context of the new paper. Also note that the 1.5 ratio was calculated on the basis of a questionable assumption. I assumed that the diameter 7.5 microns (from SPAWAR Figure S2) is typical for particles created during electrolysis while the diameter 5.0 microns, taken from their Figure S1, is typical for alpha particle. Taking this for granted was probably a mistake. To correct this mistake let me refer to Figure 7 of the new SPAWAR paper. According to the scatter plots, shown in that figure, the mean value D1 (width of an alpha particle track) is close to 10 microns while the mean value of D2 is close to 6 microns. These mean values give the D2/D1=0.6. It is two times smaller (not 1.5 times larger) that the maximum expected ratio of 1.2 (for alpha particles). The difference between 0.6 and 1.2 is likely to be more significant than the difference between 1.5 and 1.2.

20) I wish the actually-calculated mean values were reported. That would probably reduce the uncertainty in 0.6 to less than 5% (because each scatter plot is based on more than 300 data points). My estimated mean values are likely to be less precise. But they seems to be sufficiently precise to take the difference between 0.6 and 1.2 seriously. I expect mean values to be reported at the upcoming cold fusion conference, ICCF14, in Washington DC. At present I am mostly disturbed by discrepancy between the new data and my own results described at

http://csam.montclair.edu/~kowalski/cf/319galileo.html

These results showed that mean sizes of tracks, created during electrolysis, were much larger than sizes of tracks due to alpha particles. This was consistent, more or less, with the old SPAWAR results, also presented at the 2007 APS meeting. But new SPAWAR results show that the opposite is true. Tracks created during electrolysis are now much smaller than tracks due to alpha particles. I hope that this (apparent ?) contradiction will be explained at the ICCF14. For the time being I will stick to my old results. They show that the mean D2/D1 ratio was slightly larger than 2.5. The difference between 2.5 and the expected 1.2 ratio was certainly highly significant. My ratio and SPAWAR new 0.6 ratio cannot be correct at the same time.

21) Another reason for tentatively rejecting the SPAWAR idea -- tracks created during electrolysis are due to alpha particles -- has to do with the “chopped meat” structure of tracks produced during electrolysis (but not tracks due to alpha particles). I suspect that SPAWAR people also saw the “chopped meat” regions; but the article does not mention this. Ignoring troublesome issues, let me ask the following question. Which of two arguments is more convincing, the one based on long etching times or the one based on ratio of diameters? I think that the first argument is more convincing. It is also more powerful. The second argument would be about a hypothetical claim (that co-deposition electrolysis tracks are due to alpha particles), the first argument is against the actual claim made in SPAWAR paper (that tracks are due to nuclear particles).

23) P.S. By criticizing the SPAWAR paper I am trying to be useful. Yes, I know that my help might not be needed; they have been studying tracks for more than two year. But, hopefully, my critical comments might be useful to some readers. I would be very happy to applaude the SPAWAR success. That would be a great scientific contribution. The topic is too important to give up, after encountering first difficulties. But one must be careful in forging ahead.

Reference:

1) C. Brun, M. Fromm, M. Jouffroy, P. Meyer, J.E. Groetz, F. Abel, A. Chambaudet, B. Dorschel, D. Hersmdorf, R. Bretschneider, K. Kander, and H. Kuhne; “INTERCOMPARATIVE STUDY OF THE DETECTION CHARACTERISTICS OF THE CR-39 SSNTD FOR LIGHT IONS: PRESENT STATUS OF THE BESANCON DRESDEN APPROACHES,” Radiation Measurements, vol 31, (1999), pp 89-99.

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351) High Voltage Electrolysis Updates

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August 26, 2008

1) Introduction
About three years ago I was deeply involved in replicatios of Mizuno-type of excess-heat experiments (high voltage electrolysis). Several units on my CF website (see point 2f below) are based on this research. My involvment ended about two years ago, when I became one of participants of The Galileo Project (replications on SPAWAR-type experiments) organized by Steve Krivit.

But Pierre Clauzon, and his French colleagues, continued exploring the excess heat claim made by Mizuno and his coworkers. The purpose of this unit is to summarize recent results of French researchers, and to append descriptions of their future results, when they become available.

2) Current Status
a) During a private conversation in New York City, Pierre Clauzon updated me on his high voltage electrolysis research and mentioned additional experiments he hopes to conduct in the future. About one year ago we met in Pierre's laboratory in Paris. We performed the experiment together (using the old watt-meter) and the COP was again significantly larger than 1.0, for example 1.3 at 350 volts. The difference between 1.3 and 1.0 is statistically significant, considering reproducibility of the results.

b) New results, similar to those reported at ICCF12, were reported by Pierre in Sochi. But the claim of significant excess heat, reported in Yokohama and Sochi, is now withdrawn because an important systematic error was discovered. Pierre told me that measurements of the input energy were incorrect. The instrumental systematic error was due to unsuspected limitations of the Unigor-390 watt-meter. That instrument is reliable up to frequencies of 0.1 MHz while current fluctuations apparently contained much higher frequencies.

c) In new experiments, this instrument was replaced by D6000 Norma Goerz wattmeter; it is said to be reliable up to the frequency of 2 MHz. New experiments were performed by using a "boiling water calorimeter." The principle of measuring heat energy is the same as before (from the amount of steam produced during electrolysis) but the cell is surrounded by boiling water, rather than by air. The size of the cell, in these experiments was 1 liter. In these experiments excess heat was insignificant at all voltages, between 150 V and 350 V.

d) But that was not the end of the story. In the most recent high voltage electrolysis experiment, however, a significant excess heat was again discovered. The D6000 watt-meter was used in the latest experiments. The COP (coefficients of productivity) at 250 V were 1.09, 1.00, 1.10 and 0.97. At 300 volts, the COP values were 1.06, 1.08, 1.04 and 1.01. These results are very preliminary. They were obtained in an electrolytic cell (actually a dewar) whose volume was 5 liters. Pierre thinks that mechanical perturbation of plasma, in his small cell, were interfering with generation of excess heat.

e) In other words, the tentative "no excess heat" conclusion (see point 2c above) remains uncertain. Hopefully, the situation will become clear next month (after another sequence of experiments with the large dewar cell).

f) Older webpages:

http://pages.csam.montclair.edu/~kowalski/cf/252clauzon.html
http://pages.csam.montclair.edu/~kowalski/cf/253project.html
http://pages.csam.montclair.edu/~kowalski/cf/255project.html
http://pages.csam.montclair.edu/~kowalski/cf/258sampling.html
http://pages.csam.montclair.edu/~kowalski/cf/259simplified.html
http://pages.csam.montclair.edu/~kowalski/cf/260latent.html
http://pages.csam.montclair.edu/~kowalski/cf/261etiC.html
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http://pages.csam.montclair.edu/~kowalski/cf/266scared.html
http://pages.csam.montclair.edu/~kowalski/cf/267austin.html
http://pages.csam.montclair.edu/~kowalski/cf/270iccf12.html
http://pages.csam.montclair.edu/~kowalski/cf/271slaughter.html
http://pages.csam.montclair.edu/~kowalski/cf/285corresp.html
http://pages.csam.montclair.edu/~kowalski/cf/286colorado2.html
http://pages.csam.montclair.edu/~kowalski/cf/288chemistry.html
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http://pages.csam.montclair.edu/~kowalski/cf/301negative.html
http://pages.csam.montclair.edu/~kowalski/cf/302numbers.html
http://pages.csam.montclair.edu/~kowalski/cf/303energy.html
http://pages.csam.montclair.edu/~kowalski/cf/307corresp.html

3) Appended on July 2, 2007
Our formal presentation at ICCF12 (Yokahama, Japan, October 2005) can now be downloaded from the www.lenr-canr.org library. The link is
http://lenr-canr.org/acrobat/KowalskiLsearchingfa.pdf

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Nuclear or not nuclear: how to decide?

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Fourteen's International Conference on Cold Fusion
Washington, D.C, August 10-15, 2008

Introduction

The first version of this paper was devoted to my participation in The Galileo Project. More specifically I wanted to explain why, in my opinion, alpha particles, or less massive nuclear projectiles, could not be responsible for copious pits discovered by the SPAWAR team. I wanted to justify this conclusion not only on the basis of my own experimental data but also on the basis of SPAWAR results published in (1). It occurred to me, after writing the first version, that my manuscript could be published in a mainstream journal.

Not counting on success, I decided to submit the paper as soon as possible. Surprisingly, my paper (2) was accepted; it will probably be published in September or November of 2008. This presentation briefly summarizes my investigations of SPAWAR results and addresses some topics of general interest.

Investigations of SPAWAR results

In 2007 I was one of several researchers who used SPAWAR protocol, distributed by Steve Krivit, and reported the results (3) at the APS Winter Meeting (Denver, 2007). These results, and results reported by other researchers, demonstrated that dominant pits discovered by SPAWAR researchers are reproducible. No one disputed this fact. As far as I know, everyone who used the protocol observed copious pits on the face of the CR-39 detector that was in contact with the silver cathode during electrolysis.

But disagreements emerged about how to explain these pits. Are they due to nuclear projectiles created during electrolysis or are they due to something else? If copious pits are due to nuclear projectiles then what kind of projectiles are emitted? Can they be alpha particles and protons, as suggested by SPAWAR researchers (4)?

Taking the nuclear origin of copious pits for granted, I convinced myself that SPAWAR-type pits could not possibly be due to alpha particles, or to less massive projectiles, such as protons. That conclusion, based on my own experimental data, was reported in (3). In the paper to be published (2) I show that SPAWAR's experimental data, reported in (1), lead to the same conclusion.

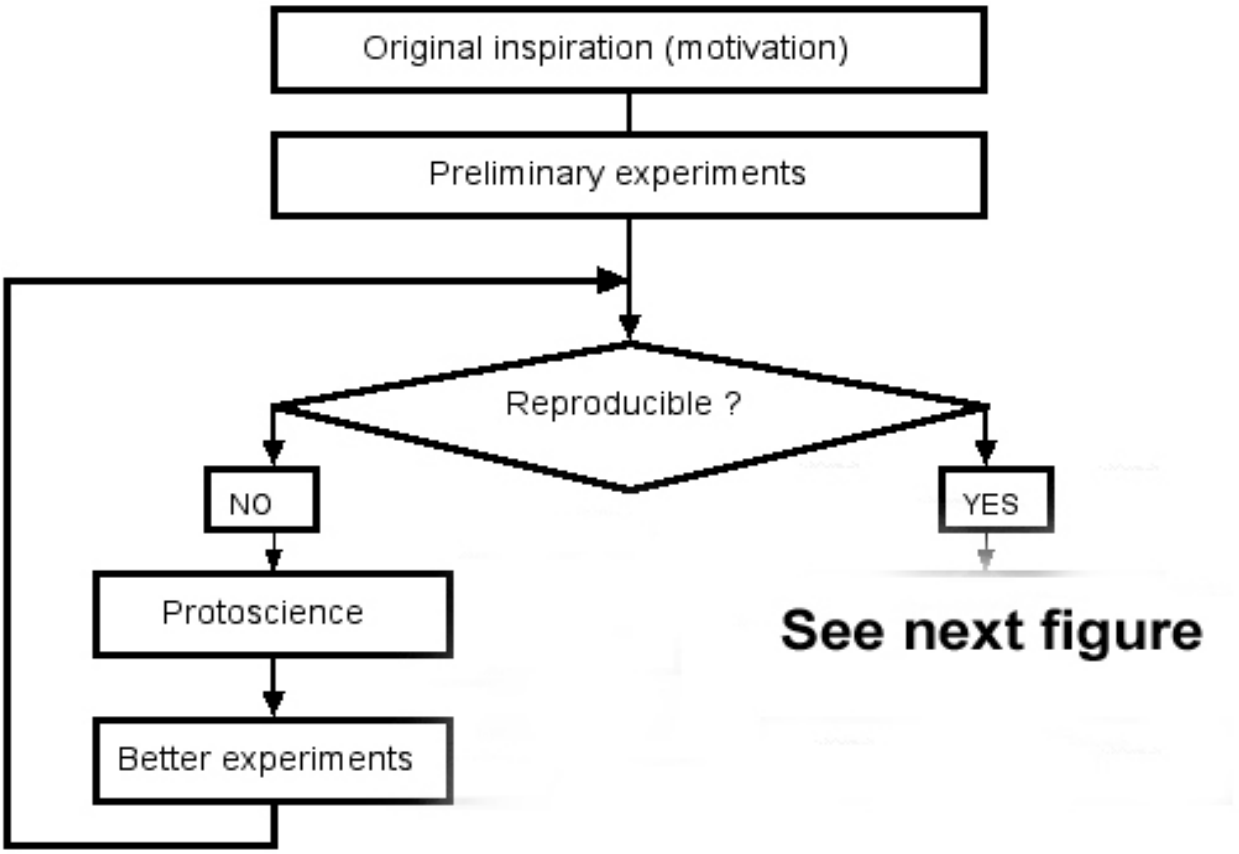
The paper (2) also refers to experimental results reported by several CMNS researchers. In an experiment conducted by Tanzella et al. (5), the CR-39 detector was surrounded by a six-micron mylar film. This was done to eliminate direct contact between the CR-39 detector and the cathode (or the electrolyte). The film

was sufficiently thin to allow alpha particles (with energies higher than 1.5 MeV) to be transmitted. No copious pits were observed in this experiment. I consider this to be a direct proof that SPAWAR-type pits are not due to alpha particles with energies higher than about 1.5 MeV.

The most interesting result of (5), however, was the presence of another kind of pits, on both sides of the CR-39 detector surrounded by the thin mylar film. These pits were not as abundant as SPAWAR-type pits (only about 100 per square centimeter versus millions per square centimeter) and they were considerably smaller than copious pits in SPAWAR-type experiments. A subsequent investigation (by Lipson et al.) showed that sizes of pits, on the mylar-protected CR-39 chip, are about the same as sizes of pits due to protons with energies between 2 MeV and 3 MeV. In my opinion, proton-like pits offer a more convincing evidence of a nuclear process due to electrolysis than much more abundant SPAWAR-type pits, near the cathode.

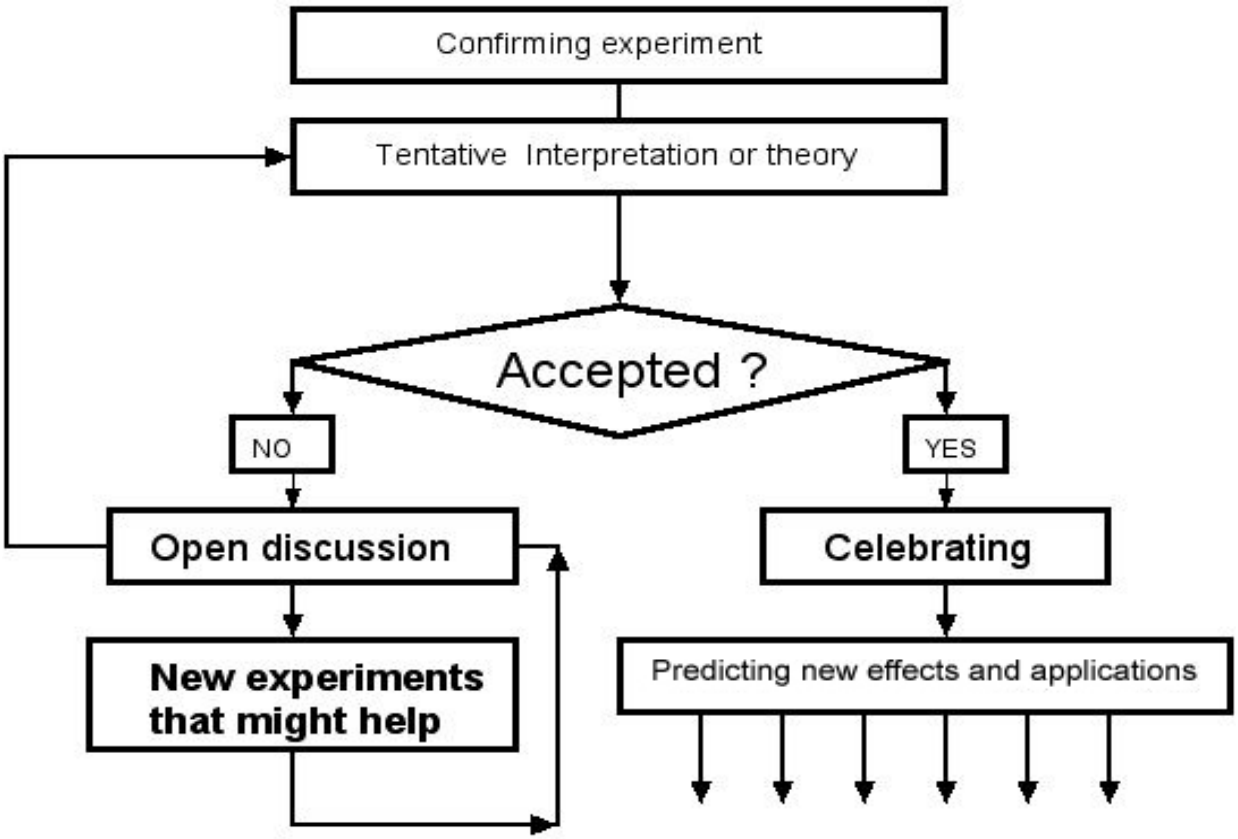
General observations and suggestions

1) Problem of reproducibility: science versus protoscience
Use of scientific methodology of validation, in a given field, is not sufficient to make a field scientific. At least some demonstrations of new phenomena must be reproducible on demand. Without this a field should be characterized as protoscientific. The expected evolution of a protoscience into science is schematically represented by the flowchart below.



2) Struggle for recognition

A field in which scientific methodology of validation is used, and in which some new phenomena are reproducible on demand, is scientific. The expected evolution of a new scientific field, toward general acceptance, is schematically represented by the flowchart below:



Evolution of protoscience into science (the first flowchart above) can be very slow because experiments are not reproducible and because researchers are not guided by a reliable theory. Theories based on ad hoc assumptions are not reliable. Success depends on sheer luck in accidentally identifying necessary preconditions for reproducibility.

But the path from reproducibility of a new effect to general acceptance (the second flowchart above) is likely to be much faster. Starting with a reproducible experiment one can quickly learn how different parameters influence the effect. Some parameters will have only negligible effect on the outcome while others will have dramatic influence. Success will no longer depend on chance; it will become a matter of skill in systematic investigations.

3) Coordinated research versus working in isolation

It is unfortunate that, except for The Galileo Project, initiated by Steve Krivit (7), CMNS researchers work in isolation from each other. This is understandable; each researcher does what matches his/her expertise and limited resources. This kind of work has been going on for 19 years. It produced remarkable protoscience, in the subfields listed below:

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1) Excess heat, sometimes orders of magnitude larger than what can be attributed to known chemical reactions.

2) Excess heat correlated with production of 4He (generated at a rate close to 23 MeV per atom of produced helium).

3) Chemically-induced changes in isotopic composition of elements. Note that the term "chemically" is very broad; it covers all atomic and molecular processes, including diffusion of gasses through solids.

4) Production of new elements, either stable or radioactive, in amounts high above what can be attributed to omnipresent impurities.

5) Chemically-induced changes in the rate of radioactive decay.

6) Production of high energy photons, for example, gamma rays, during chemical processes.

7) Emission of energetic nuclear particles

But the task of turning protoscience into accepted science is still waiting for us. How to approach this difficult task and how to proceed more effectively? In my opinion, well-focused cooperative investigations, as in The Galileo Project, are likely to be more productive, in the next two or three years, than uncoordinated efforts of many individuals. Who should select the one or two most promising effects on which to focus? Who should coordinate these investigations? And who should cover the costs (probably not more than three to five million dollars)?

I think that the task of selecting one or two effects, on which to focus, belongs to us; our CMNS discussion list, run so well by Haiko, seems to be an ideal platform to debate various proposals. A decision is likely to emerge after about three months of intensive debating. But the two other tasks, coordination and providing financial support, belong to our governments. Acting as a group, we should approach government organizations, such as NSF, National Academy of Science, or DOE, and ask for coordination and support. That is the declared mission of such organizations. National laboratories best equipped for the selected projects should be selected, to minimize additional costs. That is the only way to get a clear yes-or-no answer about the new kind of nuclear activity.

How to convince governments that a relatively modest amount of additional money is worth investing to solve the puzzle? By being organized and acting as a group rather than as independent individuals. This kind of action was already tried, about five years ago, and it produced a result; the second DOE investigation of CMNS would not have taken place without such initiative. Unfortunately, that investigation turned out to be a failure. The reason is obvious; the DOE decisions were based on voting, rather than on experimental data. Scientific decisions should be based on available data, obtained by the most qualified scientists in the most suitable laboratories. Voting is not part of scientific methodology.

4) Which experiments would I recommend?

If only one experiment were to be selected, my choice would be from the second subfield (see the above table), preferably the protocol of Mike McKubre et al. Several independent, highly qualified, investigators reported production of helium during electrolysis. Each of them reported that results were reasonably reproducible in consecutive experiments. If two experiments were to be selected then the second experiment would be from the last field, preferably the protocol described in (5). But at least one more replication of the Tanzella-type experiment would be needed; a single experiment is not enough to make the decision. Production of helium and production of protons are undeniable nuclear signatures. No matter which experiments will be selected, for coordinated investigations, the purpose should be the same, to

demonstrate a nuclear activity resulting from a chemical process.

Will we be able to act in unison and accomplish the first task quickly? Will at least one team of scientists emerge, ready to cooperate with mainstream scientists in a national lab? Will they obtain the necessary support from in at least one country? All this remains to be seen. In the final analysis, the outcome will depend on our willingness to fight for a clear yes-or-no answer.

5) **Post-confernce addendum (8/17/08)**
Contrary to my expectations, no new CR-39 results (in comparison with what was reported in Catania) were presented by SPAWAR team or by Francis Tanzella. Oriani's CR-39 paper was read by John Fisher.

References:

1) Mosier-Boss, P.A., et al. *"Use of CR-39 in Pd/D copdeposition experiments,"* Eur. Phys. J. Appl. Phys., **40**, 293 (2007)

2) Ludwik Kowalski, *"Interpreting SPAWAR-type Pits: Comments,"* accepted for publication (on July 24, 2008) in European Physical Journal Applied Physics.

3) Ludwik Kowalski et al., *"Our Galileo Project March 2007 Report,"* Winter Meeting of American Physical Society. (2007). Content of the presentation can be seen at <http://pages.csam.montclair.edu/~kowalski/cf/319galileo.html>

4) Mosier-Boss, P.A., et al. *"Production of High Energy Particles Using the Pd/D Co-deposition Process,"* Winter Meeting of American Physical Society. (2007).

5) A.G. Lipson et al., *"Analysis of the CR-39 detectors from SRI's SPAWAR/Galileo type electrolysis experiment #7 and #5. Signature of positive neutron emission."* To be published in [6]. (F. Tanzella was one of several coauthors.)

6) Proceedings of 8th International Workshop on Anomalies in Hydrogen / Deuterium Loaded Metals; 13-18 October 2007, Sheraton Catania, Sicily, Italy
Editors: Jed Rothwell and Peter Mobberley; Copyright © 2008; The International Society for Condensed Matter Nuclear Science. Printed by Instant Publisher

7) S. Krivit "2007 Galileo Project Report" at http://www.newenergytimes.com/tgp/2007TGP/2007TGP-Report.htm

353) From my ICCF14 notes

Ludwik Kowalski (8/17/08)

Montclair State University

This unit is based on what I wrote while listening to several ICCF14 presentations. Many sessions of the conference were named to match the CMNS subfields. The names were (a) Heat Results, (b) Measuring heat (calorimetry), Materials, (d) Gas and gas loading, etc.

1) What is FPE?

It is interesting that both David Nagel (conference chairman) and Dennis Lett (one of the presenters) defined the Fleischmann-Pons Effect (FPE) as "electrochemical excess heat." I like this definition because it does not refer to the origin of excess heat. Most scientists of my generation would probably say that FPE is a "nuclear reaction of some kind, triggered by electrolysis." Fleischmann and Pons speculated about nuclear origin of excess heat but they had no evidence for it. It is not hard to imagine how different history of our field would be without their premature speculations. No one would object to their isoperibolic calorimeter data and only highly qualified electrochemists, such McKubre, Szpak, Miles and Boss, would continue studying the new effect. First they would demonstrate reproducibility, then they would test interpretational predictions. Premature speculations of F&P were responsible, at least in part, for the tragic situation in which one field of research was unjustly pronounced as unscientific.

2) Superwaves

I was impressed by the report of S. Lesin (Israel). He described electrochemical experiments in which excess heat, at the power level between 0.5 and 1.0 watts, was observed (in more that 20 experiments) at levels of reproducibility between 75% and 80%. In all of these experiments the electric current was periodic (superwaves) and cathodes were bombarded with ultrasound. This should not be confused with rare experiments yielding much more spectacular results. In the best of these experiments, lasting 40 days, the excess power was said to be 32 watts. Multiplying 32 watts by the duration of the experiment one gets the total excess energy of 110 MJ. But reported value was 35 MJ. That probably means that 32 Watts was the highest power, not the mean power. The mean power was probably close to 10 W, which is spectacular.

That spectacular result was obtained by using superwaves (but without bombarding the cathode with ultrasounds). The idea of using superwaves, suggested by Dr. I. Dardik, an Americal medical researcher, has been used in Israel for many years. According to T. Zhilov, a large number of tests have been made (in the past) to show that cells powered by superwaves produce more reproducible results that identical cells powered by constant currents. According to another scientist, superwaves are also used to influence solidification of metals. Unfortunately, I am not aware of any quantitative theories explaining superwaves. Let me also mention that Preparata medal was awarded to Irving Dardik, for introducing superwaves.

3) A pectacular results to be confirmed

Another set of spectacular excess heat results were described by a Russian scientist, A. Karabut. (Unit 13, by the way, was also devoted to his results). He used an electrolytic cell powered by "periodic input" current (rather than d.c.). I suppose that periodic current consisted of nearly rectangular pulses separated by zero current, as in another paper presented by Karabut. That would be equivalent to superwaves--short pulses consist of many frequencies. The cell was made from quartz, two kinds of palladium (pretreated and not pretreated) were used as cathode and the electrolyte was made by using light water. Unfortunately, chemical composition of the electrolyte is not specified in the paper. The meaning of the "pretreated" and "not pretreated" is not at all clear. How can anyone try to replicate the experiment without such information?

But reported results are indeed spectacular. Excess heat produced with the "pretreated" palladium was produced at the rate of ~300 W for nearly 3 hours. The amount of excess heat produced was 8.1 times higher that what was used. Excess

heat produced with “not pretreated” palladium, was produced at the rate of ~100 W for 6 hours. The amount of excess heat produced was 3.1 times higher that what was used. The results were said to be reproducible on demand. Potential differences between the anode and the cathode, in these experiments, were between 800 V and 1000 V.

In both cases the electrolysis was turned off when the temperature of the electrolyte reached the level of 80 C. Does it mean that generation of excess heat, at the rate of 300 W, would continue if excess heat was efficiently removed from the cell, for example, to produce electricity? I do not know haw to answer this question. But I am surprised that no replications of Karabut’s results were made by other Russian CMNS researchers.

4) Working against all odds

During the medal-award ceremony (8/13/08) Irving Dardik referred to us as “a community of dedicated people working against all odds.” This is a profound observation. How can it be interpreted? One can say that, due to unfortunate circumstances in 1989, a situation was created in which the level of scrutiny for CMNS claims is much higher than for claims in other areas of research. This can be summarized by the well known rule formulated by Carl Sagan -- “extraordinary claims require extraordinary proofs.” How can one disagree with such rule? But the rule has often been abused. Scientific journals should not refuse sending CMNS manuscripts to referees and granting agencies should support proposals formulated by reputable scientists.

Added on 8/19/08)5

Another spectacular claim of A. Karabut

This claim has to do with generation of X rays during the glow discharge. It is not a new claim; I listened to an earlier version of it in Albuquerque (Summer 2002). But new results are much more impressive. I am not sure that the term “glow discharge” is appropriate. In a private conversation I learned that the setup operates on the right side of the Pashen curve, far away from the minimum. I would say the system operates in the controlled electric arc region. The high voltage pules are so short that the arc does not have time to develop.

On one illustration I see pulse-periodic waveforms: (3500 V and 200 mA lasting 2 ms), followed by zero volts and zero mA (lasting 8 ms). In other words, the period of repetition is 10 ms. Most laser-like beams of X-rays, presumably emitted from monocrystals embedded in cathodes, exit during the 2 ms, when the voltage is applied. But some exit when the voltage is zero. Should this be called X-ray emission after death?

In addition to laser-like X-rays (coherent and mono-directional) Karabut observed incoherent X-rays. Their intensity decreases with the distance according to the 1/r^2 law. Energies of all X-rays are between 0.5 keV and 10 keV. In other words, some photons have much more energy than one would expect at 3500 volts. The only way to understand this is to assume that multiple excitations of atoms take place at very high currents. Some of the excited levels must be long-lived, to allow for the “inverse population” of levels, a precondition for lasering.

By the way, Pamela Boss, in a presentation honoring a SPAWAR scientists, Stanislaus Szpak, reminded us that X-rays with broad energy distributions, with occasional peaks (at 20 keV and between 8 and 12 keV), were observed in electrolytic experiments as early as in 1996. The peaks were attributed to known energy levels in Pd and Ni. Presence of keV-level photons, in a low voltage cell (probably less than 10 V) could not possibly be due to chemical reactions. The same is true for production of tritium in low voltage cells, as reported by SPAWAR researchers, in 1998.

Karabut’s impressive X-rays investigations, conducted at “FSUE Luch” laboratory (near Moscow), involved a large number of cathode materials (Al, Si, Ti, Ni, Nb, Zr, Mo, Pd, Ta and W) and several gasses (H2, D2, He, Kr, and Xe). How many other Russian scientists tried to replicate his spectacular results?

6) A good overall review of CMNS

I was very impressed by a paper presented by Mike McKubre. He promised to send me excerpts from his paper. I will insert them here, as soon as they arrive.

7) Iwamura-type transmutations.

Iwamura transmutations, occurring when D2 gas permeates through a Pd multilayer complex, offer the most convincing evidence of a nuclear process due to a chemical process (in this case accumulation of deuterium ions near a CaO layer in palladium). According to the progress report, presented by Y. Iwamura, highly collimated beams of X-rays were used to study distributions of nuclear reaction products. Evidence of hot spots (places where reaction products are concentrated) was obtained. This investigation is still going on.

Two other Japanese teams of researchers presented reports on ongoing Iwamura-type projects. According to one team (T. Yamaguchi et al.), no evidence of transmutation products was found "up to now." The other team (T. Hioki et al.) did confirm appearance of molybdenum but its isotopic distribution was not the same as in Iwamura's experiments. This weakens our confidence that molybdenum was produced from strontium. These two negative reports made me think about the dilemma facing anyone whose results, or interpretations, do not agree with positive CMNS claims made by other researchers.

One has to be brave to present a negative CMNS report at a formal gathering of CMNS researchers. Most attendees want and expect positive results and convincing interpretations. Reporting something negative is likely to produce discomfort and personal friction. What fraction of CMNS researchers prefer to report nothing instead of reporting negative results? I hope that this fraction is small; reporting negative results is just as important as reporting positive results. We all know this. But how does this influence what we actually say, and not say, in public? Friendship and other considerations ("that is bad for CMNS reputation") might play a role, in practice. That, hovewer, would be bias.

8) To make future conferences more effective

ICCF14 was great; a lot of interesting papers were presented, both orally and in the form of posters. Unfortunately, most of us are not able to penetrate contents of papers heard for the first time. Our conferences (including the next one in Rome, September 2009) would probably be more productive if papers, to be presented orally, were submitted ahead of time and posted at the conference website several weeks in advance. In that case formal presentations could be shortened and more time could be devoted to open discussion, after each presentation.

Conferences would also benefit from dedicated Internet discussion lists. Suppose a list is created in connection with each conference. It would be a perfect forum for post-conference comments and debates. Many people are not able at attend faraway conferences but would like to participate. Posting papers before each conference, and offering a forum to discuss them after the conference, would be a big help for them. In fact, all would benefit from broader participation.

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354) Excess-Heat Cell of John Dash

Ludwik Kowalski; 10/5/2008

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(1) On 9/24/08, John Dash posted the following message at a private Internet discussion list for CMNS researchers:

" Scott, In Aug. 2003 my students (3 high school students and 1 graduate student) demonstrated 0.8 W excess thermal power in Peter Hagelstein's lab at MIT during ICCF-10. I believe we can do the same demo on demand, anytime, anywhere. It is now more than 2 years since you began to evaluate our experiment. When can we expect a report on your findings ? "

(2) On 9/25/08, Marissa Little, who works with her father, Scott, replied: "John, we have discussed the conclusions with you on multiple occasions. While we did see an apparent excess heat signal, we identified two mundane causes for it. Our conclusion is the same as it was at the end of testing in 2006. Thanks for reminding me that the final report was never made available. I will post a link to it on CMNS in a few days and everyone can read the details of our testing and the results.

(3) I was not aware that Scott and John cooperated to validate excess heat from a cell similar to that displayed at MIT during the first cold fusion conference I attended. Units 98 and 103 were composed after my return from the conference. Replying to the above I wrote:

"John, I was very impressed by your 2003 demo at MIT. It is worth showing in as many places as possible, and as often as possible. If I had such "demo on demand" I would aggressively invite many respected scientists to my lab. I would bet with them and used the gained money to support further research. Here is a constructive suggestion. Please describe the protocol for this demo and offer help to anyone who decides to replicate your results. The goal would be to confirm that your simple experiment can be replicated "anytime, anywhere" and by any competent person. Just imagine what would happen, after ICCF15, if several people independently reported positive results using your protocol. Describe it here (with all details) as soon as possible. I might be one of those who decides to follow your footsteps. Nothing is more urgent that a simple reproducible CMNS experiment."

(4) On 9/26/08) Mike McKubre wrote: " John has a long career in experimental science so I don't think Marissa would have communicated her conclusion of 'two mundane causes' for excess heat without significant substantiation in hand. We need to know the reasons and arguments. If there are systematic errors that can be demonstrated quantitatively to have caused (or even contributed measurably to) the excess heat effect in John's (mostly) closed cell calorimeter I (for one) would like to know what these are. If there are not, or if John does not agree, that I would also like to know. The Little calorimeter is of unusual design and may be capable of resolving errors that could cause confusion in other calorimeters. Or it may have systematic problems of it's own. It would be nice to know. Until such time as this is resolved I don't see the point of more discussion of what, where, when and who. I am much more interested in how.

(5) On 9/29/08, Pierre Carbonnelle, who is not a CMNS researcher, wrote: "Let's beware of pseudoskepticism, i.e. of 'making counter-claims based on plausibility rather than empirical evidence, of assuming that criticism requires no burden of proof, or suggesting that unconvincing evidence is grounds for dismissing it' ".

(6) On 9/30/08, Marissa Little (who works with her father Scott Little) posted the result of their investigations. She wrote: "Thanks to John's recent reminder, we've finally completed the report of our effort to verify the excess heat claims of John Dash and Wu-shou Zhang.

http://earthtech.org/experiments/DZ/report.htm

Here is the conclusion from the report:

We did see an apparent excess heat signal from the Dash-Zhang cell when operated with their protocol. It took the form of a broad pulse that started just as the cell was reaching equilibrium temperature and always died away to nothing after about 8 hours of operation. The maximum apparent excess heat power for a closed cell (DZ8) reached 0.2 W while the total input power was about 15 W. We found two separate mundane causes for this heat pulse.

1. The release of heat energy stored in the cell during the high-power period at the beginning of the run. This effect is made larger by the use of insulation around the cell such as the cardboard box recommended by Dash and Zhang.
2. The heat of wetting released when ~100 g of recombiner pellets is wetted by water vapor rising from the nearly-boiling electrolyte.

We also conducted a number of experiments using electrodes supplied by Dash and Zhang mounted in standard ET cells. None of these runs showed any signs of excess heat above MOAC's detection limit, which is approximately 30 mW.

John, if you have new cells which avoid these problems and show substantial persistent excess heat, we would be delighted to test them in our calorimeter."

(7) Several other CMNS researchers contributed to this thread. Many important details, with which I was not familiar, were emphasized. John Dash wrote that his improved protocol, and personal help in implementing it, is available to any CMNS researcher.

(8) On 10/1/08 Mike McKubre wrote: ". . . John you are using a very large amount of recombiner. Why? Would you care to comment on Marissa's observations? No matter how stated there is information and learning there and it is up to you to understand quantitatively what Marissa is reporting and get back to us with agreement, rebuttal or uncertainty. Marissa; I don't think anyone doubts your observation, just your interpretation and the forcefulness of your conclusion. I don't pretend to be an expert or superior in this matter but . . . I have not read your web-paper yet but, however rigorous your energy calculation, you apparently observed little more than a 1% peak effect. This is not (as I understand it) the Dash-FP effect."

(9) On 10/1/08 John Dash wrote: "Marissa, I should have read your report before I responded. It does have the numbers ! Sorry. You did some excellent work on our behalf. Thank you and thank Scott. What I am wondering is, did you calibrate MOAC with the calibration cell in the cardboard box ?

(10) On 10/2/08 Marissa Little wrote: "Ed and Mike, the DZ cell is quite unique – it contains 100 grams of recombiner pellets (our typical cells use 30 pellets which weighs just over ONE gram). Before each of the DZ runs, the pellets are placed in an oven overnight, so they are very dry. The DZ cell contained far more recombiners than needed, most get wet and stop working. But a few stay dry and continue to work. Here is a picture of a cell immediately after power was cut.

(11) On 10/2/08 Ludwik Kowalski posted the following message:

Dear all, Please provide data for the summary of reported results (replace question marks with wattages below).

A) John Dash ----->MIT demo 2003
Input power ~10 W, excess power 0.8 W (+/- 0.15 W)

B) Scott and Marissa Little-----> 2008
Input power ~15 W, apparent excess power 0.2 W (assuming 0.2 W was not due to two "mundane causes" suspected by the authors.)

C) Chang Chun University, China ----> 200?
Input power ???, excess power ??? (+/- ???)

D) Italy two high schools ---> 200?

(12) On 10/4/08, Scott Little sent me a private message explaining that my use of the word "suspected" (in item B above) did not properly refer to what they did. Scott wrote.

"I have a quibble with the wording of your recent statements about our efforts on the Dash-Zhang experiment. It's not worth taking up the time of everybody on the CMNS list so I'm writing privately. [quoting item B shown above]

Back in late 2006 when we were running the Dash-Zhang cell in our calorimeter, the first time we observed a temporary excess heat signal while using the cardboard box around the cell, we "suspected" that the box might be partly responsible. We then set up the differential equations for an insulated mass being heated by a declining input power and plotted the temperature of the mass vs time. As discussed in our report, this computer simulation "demonstrated" results that qualitatively matched the behavior of the DZ cell. But that's a transient effect which should have integrated to zero. In the case of the DZ cell, the integrated energy totals always came out a little positive. We then began to "suspect" the 100 grams of recombiner pellets, mainly because that was the only other unusual aspect of the DZ cell. At the time we had never heard of "heat of wetting". As discussed in our report, we set up a special experiment in our calorimeter to measure the heat of wetting of 100g of dry recombiner pellets. The result was 2760 joules, which closely matched the net energy integral we were seeing from the DZ cell.

Summarizing, we "suspected" the two mundane causes first, then we proceeded to "demonstrate" that they were both present and that they qualitatively and quantitatively accounted for the observed apparent excess heat signal.

Ludwik, we are making a concerted effort to state our conclusions properly and with appropriate reservation. I will be the first to admit that our negative results do not prove that the DZ cell does not work. But I do not think it serves us well to soften the results as you have done above. We saw an apparent excess heat signal and we demonstrated that it was due to mundane causes. That sort of reporting is exactly what the CMNS field needs if we are ever to be properly recognized by mainstream science."

(13) On 10/5/08 Ludwik Kowalski wrote: " It is great that John offered to help anyone willing to replicate his demo, and that Marissa offered access to their superb calorimeter. Will there be at least one new team of electrochemists to take advantage of the offers made by John and Marissa? This remains to be seen.

In any case, I hope that the ongoing cooperation between John and Marissa will continue till they agree on presence or absence of excess heat, in a cell constructed according to John's protocol. It would be a big waste to stop their cooperation at this time; John has great experience in building excess heat cells, Marissa and Scott have experience in measuring excess heat with their calorimeter. The instrument is not only very sensitive (able to measure small amounts of excess heat); it is also very accurate (small systematic errors--drift etc.) and very precise (small random errors--standard deviation).

I guess that more than ~95% of necessary investments (time and money) have already been made by each team. That is why I think that stopping the ongoing cooperation would be highly regrettable. Finding the truth is much more important than any other possible factor. Am I the only one to think that John and Marissa should continue to cooperate? "

(14) On 10/5/08 Ed Storms wrote: "It seems to me we have two issues. The first is whether the calorimeter and cell design John uses has demonstrated real excess energy. The heat of absorption of water on the large amount of catalyst is a worry, as shown by Marissa. The second is whether the cathodes made and used by John are nuclear-active. Since

creating nuclear-active cathodes has proven to be very difficult, any success in making active material would be a major accomplishment. Therefore, I think the emphasis should be placed on testing a Dash cathode that has made excess heat previously in his calorimeter. I would volunteer to do this using a well characterized Seebeck calorimeter. I can also examine the cathode for various characteristics before and after the run. Since my setup has in the past detected excess energy, we can be sure it can do the same in the future."

15) On 10/5/08, Marissa Little wrote: "Very good points Ed. I think that John may have moved away from the large amount of catalyst, though I could be wrong. I noticed in a poster (W.S. Zhang, Dash, Z.L. Zhang) at ICCF-14 that the catalyst was on the order of a few grams instead of ~100 grams. Here's a comparison of the "old" cell and what I think is the "new" cell (shown in an isoperibolic calorimeter) [Two photos displayed]. John, was this a unique setup or do most of your cells now use smaller amounts of catalyst?

16) On 10/5/08 Ludwik Kowalski wrote: "I agree with Ed that 'we have two issues,' excess heat and its nuclear origin. I will be happy to provide CR-39 chips and to perform the tedious task of counting tracks, if any, for anyone who observes excess heat."

17) **Final wish**
Let us hope that John and Marissa (helped by others) come to an agreement about excess heat, and publish the result in a mainstream paper. I am not going to write about details that are still being discussed. I am not an expert in electrochemistry and recombiners. But the discussion is interesting.

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355) Discussing SPAWAR neutrons, etc.

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1) Introduction
There was an exchange of interesting messages about CMNS phenomena on a private Internet discussion list for researchers. After showing four messages posted today I will show the abstract of a paper that Takahashi attached to his message.

2) Message posted by Ed Storms (from USA)
I would like to throw an idea into the discussion pot and see what kind of soup results. We all know that a few neutron have been detected from CF cells. When tritium is measured at the same time, the ratio strongly favors tritium. The challenge has been to explain this behavior.

If the neutrons result from a hot fusion reaction, such as caused by fractofusion, why is the tritium not made in the amount expected to result from hot fusion [where the number of neutrons is the same as the number of tritons] ?

Helium is produced when excess heat is detected. Suppose the helium starts with enough energy to produce neutrons from the well known alpha/n reaction. Most efforts to detect neutrons have failed because the basic helium-energy producing reaction was not initiated. Most successful studies that actually produced the helium-heat reaction did not use neutron detectors. Therefore, this emission was missed because the number of neutrons is too small to be noticed without a sensitive
neutron detector.

In other words, helium and tritium are made by the NAE [Nuclear Active Environment favoring CMNS phenomena] and neutrons are emitted in small amount because the resulting alpha can have enough energy to cause the alpha-n reaction with impurities within a F- P cell {Fleischmann-Pons cell], such as B or Li.

Takahashi is one of the few people who measured the energy of emitted neutrons under conditions that produced some heat. These neutrons had an energy that could have been produced by an alpha/n reaction. What do you think of this idea, which is counter to what we have assumed to be the case?

[Ed Storm is the author of a book about CMNS phenomena (1)] :

3) Message posted by Mahadeva Srinivasan (from India)
There seems to be strong evidence to suggest that tritium appears invariably after a neutron burst. We have gone on record to suggest that both neutron and tritium occur simultaneously. I can pull out all our results which support this contention for you.

In your alpha to neutron model where does the tritium come from? Keeping in mind the experimentally confirmed branching ratio anomaly, I would therefore propose that what ever be the origin of tritium, neutrons seem to be a small byproduct or side reaction of that process.

P.S.
* We had pointed out in our ICCF1 paper that many groups at BARC seem to have independently noted that invariably tritium is observed in the electrolyte only after a large neutron burst is recorded. See for example Figs 3 and 7 of the Overview paper by Iyengar et al at ICCF1. So I continue to support the notion that tritium and neutrons are coupled.

* Whether tritium also arises from the same NAE which generates helium is worth examining. Ed has a good point!

* The very concept of an NAE proposed by Ed suggests hot spot behaviour. We had observed hot spots through autoradiography in Ti tagets in very many TiD targets. (See our Provo meeting paper of 1990 titled "Observation of Tritium in Gas/Plasma Loaded Ti Samples". (This paper has just been uploaded by Jed in the lenr site. THANKS JED!)

* Our papers on Statistical Analysis of neutron emission published first at ICCF1 and repeated on a different Milton Roy cell in 1994 and published in ICCF5 once again pointed out that there are many instances of bunched neutron emission suggesting that there seem to be events where hundreds of neutrons are emitted.

* So what does all this mean? Neutrons are localised in time; Tritium is localised in space (hot spots & NAE); Tritium and neutrons are coupled.

* Superimpose on this the branching ratio anomaly of 10**(-8) you come to the tantalising conclusion that micronuclear explosions seem to be occurring, highly localised in space and time.

* So far I have not seen any evidence that He and Tritium are coupled, meaning that they are both occurring in the same event. But Eds hunch that there cant be two categories of NAE forming sounds quite plausible. May be they are all coupled and all emanating from micronuclear explosions!

* This is where I come back and once again emphasise the importance of statistical analysis of the neutron emission. As far as I can see neutron diagnostics is the only tool through which we can investigate the concept of cascade events.

* I have been trying to convince Pam and the SPAWAR group that they are well placed experimentally to verify this independently !

4) Message posted by John Fisher (from USA)

Polyneutron theory predicts exothermic reactions between a polyneutron An and deuterium 2H that generate reaction products ^{1}H, ^{3}H, and n. Reaction energies depend on whether the polyneutron contains an even or an odd number of neutrons. Reaction is favored by increased energy and is inhibited by a correlation barrier for An ---> AH (in which a proton is substituted for a neutron in a polyneutron).

For odd-A polyneutrons the reactions in order of their rates are

An + 2H ---> (A+1)n + ^{1}H + 8.9 MeV (favored)
An + 2H ---> (A-1)n + ^{3}H + 3.6 MeV
An + 2H ---> (A+1)H + n + 8.9 MeV (inhibited).

For even-A polyneutrons the reactions in order of their rates are

An + 2H ---> (A+1)n + ^{1}H + 0.5 MeV
An + 2H ---> (A+1)H + n + 0.5 MeV (inhibited)
An + 2H ---> (A-1)n + ^{3}H – 4.8 MeV (endothermic).

(Energies are written only to the nearest 0.1 MeV because experiment has not provided more accurate experimental evidence for polyneutron masses.) For reaction energies above 1 MeV we expect 1H and n with energies 8.9 MeV, and ^{3}H with energy 3.6 MeV; and we expect the neutron reaction to be strongly inhibited by a correlation barrier. For reaction energies less than 1 MeV we expect ^{1}H and n with energies 0.5 MeV and again we expect neutrons to be strongly inhibited. Overall we expect more tritium than neutrons. (In an earlier communication I have accounted for helium in proportion to heat without direct or indirect generation of neutrons: most heat comes from the 8.9 MeV 1H reactions and the helium comes from polyneutron decay.)

I appreciate that polyneutron theory is unpopular. By this communication I do not expect to change the situation in the short term. But I have hopes for the cumulative influence of polyneutron predictions in the long term.

[John Fisher has been promoting the idea of polyneutrons for many years. The latest version of his theory is described in the proceedings from a 2008 conference (2)]

5) Message posted by Akito Takohashi (from Japan)
Dear Ed and all; this is good idea. As you know, my past group at Osaka University made serious efforts, under the Japanese New Hydrogen Energy Project (1994-1998), to search possible correlation between excess heat, He-4, neutron, tritium and X-rays. We used most sophisticated method available at that time. I am attaching a summary paper, for people who do not have it.

We had some results to draw what was taking place, but it was not definite. We used only Pd-plates for the cathode of D_2O electrolysis (c.f. H_2O). The main difficulty was low level of excess heat, very low [rate of] nuclear emissions and [poor] reproducibility.

Now, in 2008, we have had some [progress with] reproducibility. The nano-size-Pd samples have shown encouraging results by the Energetics-SRI-ENEA method of US+SW electrolysis, The SPAWAR co-deposition electrolysis method, The Arata-type nano-particle D-gas loading method, The Celani-type nano-wire D-gas loading method, etc.

If we can combine again these newer methods and the past sophisticated diagnosis methods (see the attached paper), [then] we will have a good chance to [study] the correlation between the observed products. That would lead to understanding of the underlying mechanism. Unfortunately, funding to support such investigations is not available.

I do have some theoretical ideas but will not touch upon them today.[Akito Takahashi used to be an experimental nuclear physicists. But, after retiring, he decided to focus on theoretical considerations, as illustrated in (3)]

[Akito Takahashi used to be an experimental nuclear physicists. But, after retiring, he decided to focus on theoretical considerations, as illustrated in (3)]

6) Abstract for the 1998 paper by Takahash et al. (4)**:**
Using two electrolysis systems based on D_2O/Pd electrolysis,experimental searches were tried to find correlation between excess heat and possible nuclear products (neutrons, X rays,tritium and helium). One was the open electrolysis system, with twin cells to study correlation between excess heat, X-rays and neutrons The other was the closed electrolysis system to study correlation between D/Pd ratios, excess heat, neutrons and helium. No very clear correlation between excess heat and any nuclear products have been observed, but several marginal-level data were obtained to show helium-4 production when excess heat were observed in the closed electrolysis system. In few cases by the open electrolysis experiments, clear excess heat was observed with no visible increases of characteristic X-rays and neutrons over the background. Burst events of soft X-rays and neutrons were observed in few cases, being independent of excess heat production.

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(2) John C. Fisher “Outline Of Polyneutron Theory;” in proceedings of 8th International Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, 13-18 October, Sheraton Catania, Sicily, Italy. Edited Jed Rothwell and peter Mobberley. The international Society for Condensed Matter Nuclear Science.

(3) Akito Takahashi and Norio Yabuuchi “D-Cluster and Fusion Rate by Langevin Equation,” in proceedings of 8th International Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, 13-18 October, Sheraton Catania, Sicily, Italy. Edited Jed Rothwell and peter Mobberley. The international Society for Condensed Matter Nuclear Science.

(4) “Original Experimental Study on Correlation between Excess Heat and Nuclear Products by D_2O/Pd Electrolysis,” by Akito Takahashi, Hirotake Fukuoka, Kenichi Yasuda and Manabu Taniguchi. International Journal of of Material

Engineering for Resources, vol. 6 No.1, 1998.

APPENDED ON NOVEMBER 1, 2008

7) Horace Heffner wrote: (from USA)
Ed wrote: “To summarize, Horace, an energy difference exists between the three reaction paths that you expect to influence the reaction rates. This difference does not have an influence when the reaction is initiated at high energy. Why not?”

The energy difference does have an influence because the mass deficit from the final products limits the energy available from the reaction. If specific strong force bonds do not form then no kinetic or radiant energy results from their formation. If a strong force bond forms that must be broken in order to make some final fusion product, then the energy must be available in the excited nucleus to break that bond, i.e. to undo it, else the reaction is not feasible. Energy is key.

Further, in a vacuum initiated kinetic fusion reaction there is no electron in the excited He* intermediate product reducing the amount of available energy. This makes the reaction pathways fairly simple. In the lattice it is feasible for electron catalyzed fusion to occur. A catalytic electron has an energy related influence on the final products feasible, and this influence varies with time.

Ed also wrote “In addition, until the reaction is initiated, how do the nuclei know how much energy will be released, hence what the rate should be? “

The energy exciting the intermediate ^{4}He* comes primarily from formation of strong force bonds.

Ed also wrote “The He reaction is suppressed in a vacuum because emission of the required gamma violates several rules, hence is hard to initiate.”

Yes, there is a momentum problem and maybe a long half life for any gamma emission that allows the other branches to occur first. Again, this problem doesn't exist if there is an electron in the He*. The electron can radiate away the energy to the lattice in small increments.

Ed also wrote “In contrast, the He reaction is very favored in a solid. Obviously, the environment plays an important role.”

Yes indeed, but here it takes the Deflation Fusion model to understand this I think. The deflated state is very brief - too brief to provide electron catalysis for high energy vacuum collisions except at energies so extreme fusion is otherwise expected. Electron catalyzed fusion in the vacuum is thus just not readily observable. Though it is brief, the deflated state repeats rapidly enough (has sufficient observation probability) when in an appropriate lattice environment to make fusion observable.

Ed also wrote “Why would the environment not also affect the tritium reaction, independent of any energy considerations?”

The environment does affect the tritium reaction, the branching ratio, but just not independent of energy considerations. The key to understanding the branching ratio in detail is being able to model tunneling probabilities, lattice electron wave functions, and
catalytic electron behavior, including radiation, sufficiently to estimate the mean value m and deviation s of the catalytic electron energy deficit through time. This is undoubtedly a very large undertaking, both theoretically and experimentally. The underlying concepts are fairly simple though.

Ed also wrote “After all, the tritium is generated with only a small fraction of the total energy just like the helium.”

I don't think this is right. The mass deficit of the resulting products differs. The energy of the tritium reaction, 4.03 MeV *is* the available energy, not 23.9 MeV or some large number (ignoring any vacuum energy exchanges that might occur). It actually takes a lot *more* energy away in the form of mass (19.87 MeV) to form tritium from the He* than helium. See discussion below.

Ed also wrote "The process of distributing the energy, I expect, would be similar in the two cases. Therefore, the energy associated with the total, ideal process would not be operating on the actual reaction products. Why then should the total energy play a role?"

Because the breaking of a strong force bond, or the failure to form such, changes the final mass deficit and thus the reaction energy available. Again, the mass deficit energies are given by:

D(D,p)T 4.03 MeV
D(D,n)^{3}He 3.27 MeV
D(D,gamma) 23.9 MeV

A small waveform electron in the He* intermediate product reduces the above energy by an amount depending on the electron waveform size at the time of the final product formation.

If the electron wavelength is sufficiently small to momentarily impose a 6 MeV additional deficit, then at that time the energy to permit the D(D,p)T or D(D,n)^{3}He reactions is not available.

Another way to look at this is to assume the ^{4}He* activated nucleus is created upon fusion, having 23.9 MeV energy embodied in the thermalized motion of the constituents: two neutrons, two protons, and an electron. The highly non-linear motion of the constituents places various amounts of strain on each of the bonds until some energy is radiated away and the process continues, or one or more of the bonds is broken an reaction products result. Under this assumption the bond breaking to enable the D(D,p)T reaction requires 23.9 MeV - 4.03 MeV = 19.87 MeV. Similarly the bond breaking to enable the D(D,n)^{3}He reaction requires 23.9 MeV - 3.27 MeV = 20.63 MeV.

If in a given environment at some point, we suppose the catalytic electron creates and energy deficit -E(m,s), where m = -6 MeV, and s = 0.25 MeV, then T production would be a 4 sigma exception event, and neutron production would be a 7 sigma exception event.

It is notable that the catalytic electron wave function can be expected (at least in my framework) to expand until it can occupy a chemical energy sized orbital. It can also radiate energy in the process. The energy for the expansion and radiation is supplied by a combination of the action of the zero point field and fusion energy.

8) Peter Gluck wrote: (from Romania)
[Responding to Ed Storms] Just to mention that the optimal form of Pd samples was not tested yet. In my opinion it is metallized fibers- e.g. carbon nanotubes coated with Pd and probably other metals in layers- really monodimensional, kind of Patterson beads in one dimension. This is logical if we accept that LENR happens at the surface, in limited areas. The difficult part is the manufacture of these metallized fibers, despite a great experience existent the field. But actually it is no progress in higher heat effects obtained and reproducibility is still white (or gray) magic rather than a science. My explanation- poisoning with gaseous impurities from air that inactivate NAE is ignored. O.K- but if we do not believe in occult phenomena we have to find some explanation and prove it experimentally. Each year I am getting older and more worried for the future of the field.

[Responding to Akito Takahashi] Volume is useless, we need surface, therefore Pd coated on some nanofiber at a minimum thickness and protected with a monomolecular layer of some material is ideal. I think that all really strong heat release events as Mizuno's historical unquenchable cathode or cathode 64 of Energetics had some aleatory protective layer and avoided poisoning.

9) Akito Takahashi wrote

[Responding to Peter] I agree with your view "mono-dimensional Pd samples". Actual forms are of maximum-surface & minimum-volume as nano-wire, nano-particle or complex composite, nano-fabricated (designed) fractal samples, etc. We will try many forms of such materials. Man must get aging every day after day, but try to get something new.

10) Ed Storms (responding to John Fisher)
I agree, the polyneutron theory has some useful features and predictions. The next step is to show that polyneutrons actually exist based on some measurement outside of CF and show the conditions that cause the predicted reactions to occur. Without such supporting information, the idea has no greater benefit to the field than the other equally unsupported ideas. Can you think of a way to demonstrate the actual existence of polyneutrons? What exactly has to happen in a F-P cathode to start the sequence of events you predict?

11) Drew Meulenberg wrote: (from India)
I believe that Horace has the essentials of a correct model for LENR. See italicized comments interleaved with his story. These comments are mainly based on K. P. Sinha's and my paper at ICCF-14 (and the consequences).

> "... in a vacuum initiated kinetic fusion reaction there is
> no electron in the excited He* intermediate product reducing
> the amount of available energy. " The "starting energy of the
> He* nucleus is also higher. This means that there is more
> time for (therefore more chance of) fragmentation before the
> nuclear energy drops below the critical values. Therefore,
>2 channels dominate.

He also wrote "This makes the reaction pathways fairly simple. In the lattice it is feasible for electron catalyzed fusion to occur." KP and I feel that 2 electrons (a lochon) are required to catalyze the LENR.

> "A catalytic electron has an energy related influence on the
> final products feasible, and this influence varies with time."

The closer the deuterons approach, the higher the electron energy (1-D effect) and, therefore, the smaller its orbit and Compton wavelength.

>"...The electron can radiate away the energy to the lattice in
> small increments."

This post-fusion action of the electrons / lochon is the critical ingredient that blocks the fragmentation channels by dropping the nucleon energies below threshold. The energies drop below the D(D,n)He3 threshold (3.27 MeV) first; then the D(D,p)T threshold (4.03 MeV). If the process is too slow, fragmentation will occur.

> Though it is brief, the deflated state repeats rapidly enough
> (has sufficient observation probability) when in an appropriate
> lattice environment to make fusion observable.

I'm assuming this refers to the phonon frequencies, which may exceed 1E14/second.

> The environment does affect the tritium reaction, the branching
> ratio, but just not independent of energy considerations. The key
> to understanding the branching ratio in detail is being able to
> model tunneling probabilities, lattice electron wave functions,
> and catalytic electron behavior,

KP and I have done all this in our ICCF-14 presentation and subsequent work.

> including radiation, sufficiently to estimate the mean value m

> and deviation s of the catalytic electron energy deficit through time.

Radiation coupling of excited nucleons with the lattice (via the electrons / lochon) is not straight forward since it is an interaction of the nucleon near-field radiation with the "inside" of an electron wave function. Nevertheless, that is our goal over the next few months.

12) What started this thread?
The answer to this question came in the form of a message posted today by Akito. The attached file (quoting earlier contributions to this thread) reminded me that the discussion was triggered by a claim, made by the SPAWAR team, that their triple-prong tracks, in CR-39 detectors, are due to neutrons of several MeV. Responding to their message I wrote: "Most of [the interactions with fast neutrons would consist of] scattering (elastic and inelastic), the rest [would] consist of several competing reactions, such as ^{12}C(n,p), ^{12}C(n,d) etc. One of these reactions might be ^{12}C(n,alpha)^{8}Be, where ^{8}Be always fissions into two alpha particles, immediately. That would indeed produce three alpha particles outgoing from a single point..... Most triple-prong events would originate deep inside the CR-39, rather than at its surface. Multiple-etching technique (used by SPAWAR people and by Oriani) would either confirm or refute this expectation."

I also wrote "But how certain are SPAWAR people that neutrons with energies exceeding 6 MeV are emitted during electrolysis? Hopefully, the answer will emerge before the ICCF15." In another place I speculated about projections of the tripple-prong events, seen in CR-39. In most cases one of the projected ^{12}C(n,alpha)^{8}Be prongs would be considerably shorter than two other prongs. This observation was based on the old angular distribution data published by Takahashi.

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356) The 20th Anniversary of CMNS is approaching

Ludwik Kowalski; 12/6/2008
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Robert Park, the author of the 2000 book, "Voodoo Science: The Road from Foolishness to Fraud," has a website named What's New."

http://bobpark.physics.umd.edu/

On Friday 28, 2008, he posted a piece called "Cold Fusion: Or Is It The 'Fleischmann-Pons Effect.' " He wrote:

" It's been almost 20 years since the March 23, 1989 announcement that cold fusion had been discovered by two chemists at the University of Utah. By June, cold fusion was an object of ridicule. A small band of embattled defenders retreated to holding annual conferences of like-minded scientists to which skeptics were not welcome. The story now seems to be entering a new chapter. Believers have begun showing a willingness to confront skeptics, submitting papers to open meetings of major scientific societies. They no longer use the term 'cold fusion,' preferring the less contentious 'low-energy nuclear reactions' (LENR) to describe their field; LENR more accurately describes what, if anything, is going on. However, the use of LENR has been undone by referring to 'excess heat' as the Fleischmann-Pons effect. This only serves as a reminder of the outrageous conduct of the university administration and the incredibly sloppy research on which the claim was based.

This year, there is great excitement over the work of Yoshiaki Arata, a respected professor at Osaka University. In May Arata demonstrated the production of excess heat to an audience of 80, but there have been many such claims over the years and until it is replicated by someone outside the LENR community and a plausible explanation is advanced, it will change few minds."

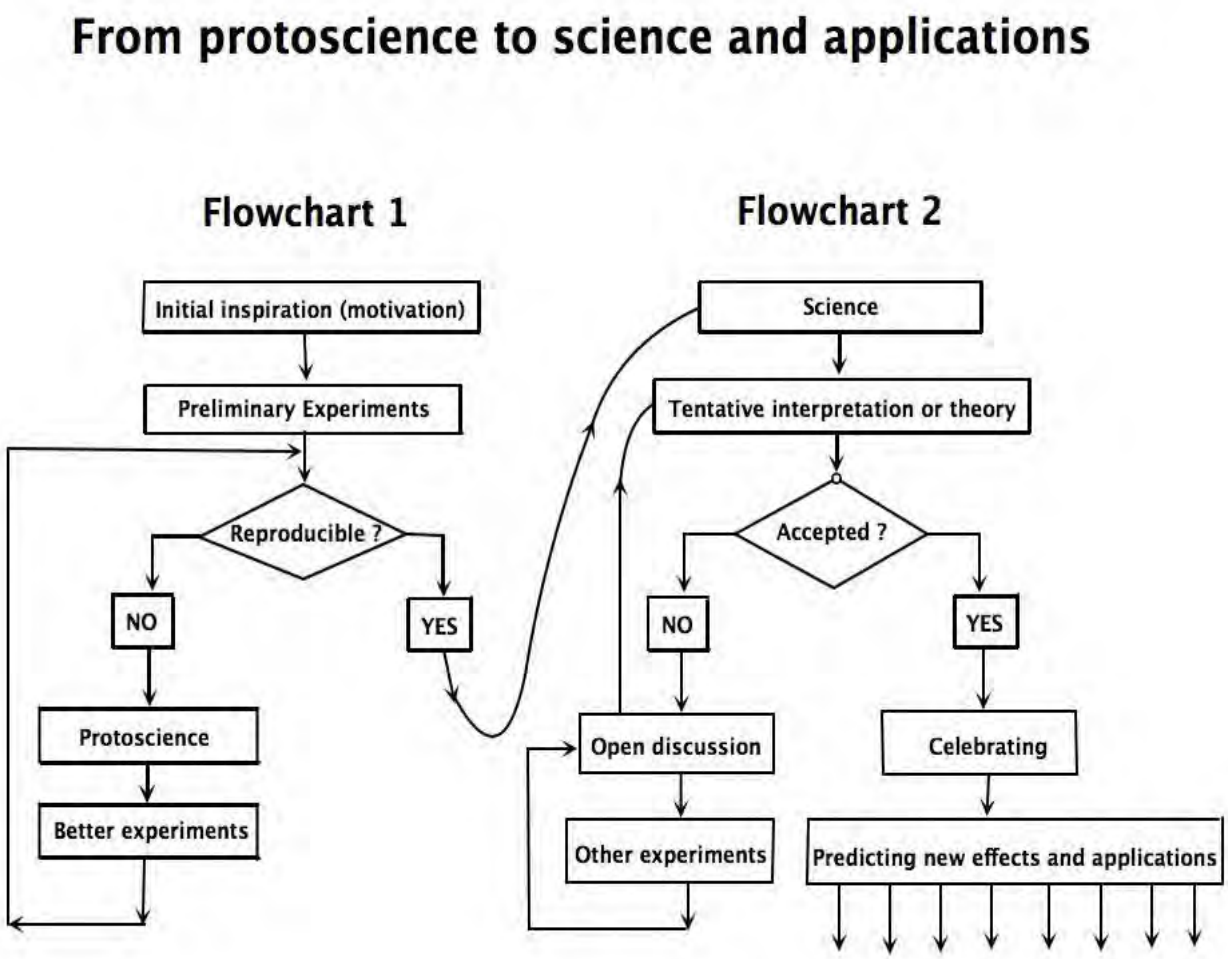
It is interesting that Robert Park, whose specialty is surface science, prefers to attack the field as a journalist rather than as a scientist. As far as I know, he was personally invited to attend at least two scientific conferences devoted to Cold Fusion, one in Boston and one in Washington D.C. Both locations are relatively close to where he lives (Maryland). Why didn't he come to at least one conference? His background seems to be perfect to discuss various topics from the scientific point of view.

Why was the original name, Cold Fusion, replaced by other names, such as CMNS or LENR? Because it became clear, during the last decade, that nuclear reactions, if any, induced by chemical processes, are very different from what is often called hot fusion (thermonuclear fusion). This was far from being obvious twenty years ago.

Am I the only one who feels that the term "small band of embattled defenders" is insulting. I have nothing but admiration for those who continue a difficult fight for a clear yes-or-no answer about reality of CMNS claims. The term "group of researchers" or "group of scientists" would be much more appropriate. Why should recognized Ph.D. scientists, from several countries, be called a band? Why shouldn't we admire those whose motivation is to contribute to the common good? Most of them work without any financial support. They do this in the climate of discrimination; their advances are usually ignored by the wider scientific community.

What follows is a slightly revised version of two flowcharts I showed at the last International Conference on Cold Fusion (ICCF14 in Washington D.C.). Using this diagram, I can say that CMNS scientists are still at the stage of

protoscience (the lower left corner loop). The field hopes to be recognized as scientific but this is not easy. Progress is slow because individual researchers, or small groups of researchers, work in isolation. In my opinion, progress would be faster if their efforts were coordinated, if funding was available, and if there were no discrimination, as written in a recent OpEd. Click below to see it.



Two flowchart in one

click to see the OpEd

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357) Interpreting SPAWAR-type pits: Comments

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Abstract:
A recent claim demonstrating a nuclear process triggered by electrolysis is challenged. An analysis, based on relative diameters, is used to demonstrate that predominant pits could not possibly be attributed to alpha particles, or to less massive nuclear projectiles. This conclusion is supported not only by positive results from a replication experiment, but also by results from the experiment on which the original claim was based. While the numerous SPAWAR-type pits seem to be highly reproducible, their interpretation is not yet clear.

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Today is January 8, 2009. I am revising this unit. Instead of showing a slightly abbreviated version on my paper I am now providing a link to the entire paper (the first link below). The second link below will download the entire rebuttal paper published by the SPAWAR team.

- -

1) To download the entire paper (pdf file) click here

- -

2) To download the rebuttal paper (pdf file), subsequently published by the SPAWAR team click here

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3) The original SPAWAR paper, published in 2007, had two parts, one printed and one online.

To download the original printed part of the article (pdf) click here

To download the original online part of the article (pdf) click here

= =

Addendum (January 11, 2009)

Let me comment on the rebuttal, and on some private remarks received by e-mail. This brief addendum is for people familiar with SPAWAR articles (links 2 and 3 above) and with my article (link 1 above).

a) Criticism of SPAWAR interpretation, presented at the 2007 ACS meeting, was based on my own experimental data. Dominant SPAWAR-type pits I saw were typically 2.5 times larger that pits due to my alpha particles of ~ 4 MeV. In other words the ratio of measured mean sizes was R1=2.5. The symbol R2 will be used below for the expected ratio of sizes.

b) Suppose the pits created during electrolysis are due to alpha particles of ~1 MeV, as claimed in a subsequently-published SPAWAR paper. In that case the ratio, according to the calibration curve used in my paper, would be R2=1.30. (The R2 would be even smaller for alpha particles of other energies.) The big difference between R1=2.5 and much smaller R2 allowed me to say that SPAWAR-type pits should not be attributed to alpha particles.

c) A subsequently-published SPAWAR paper had photographs of dominant post-electrolysis tracks, and tracks due to alpha particles of 5.5 MeV (on the same CR-39 chip and under the same magnification). Using these photos, I concluded that their R1 was close to 1.7. That is significantly smaller than my value R1=2.5. I do not know how this can be explained. (P.S. See item f below.) By the way, I was not the only one to successfully replicate the experiment. It would be desirable to compare R1 values determined by other participants of The Galileo Project.

d) Is it possible that the calibration curve, which I used to determine R2, does not apply to CR-39 chips manufactured in Japan, as suggested in the rebuttal paper? In my opinion, the shape of a calibration curve of CR-39 should not depend on where the material was manufactured. That shape reflects the energy dependence of the dE/dx (sometimes called the stopping power, or ionization density). But I know, from what has been reported by several researchers, that CR-39 chips from different manufacturers do differ in several other ways. Let me accept the fact, reported in the rebuttal paper, that the value of R2 is 1.5 for chips from Japan and 1.3 for chips from UK. The difference of 0.2, between the two values of R2, does not weaken my claim (that dominant pits cannot be attributed to alpha particles of ~1 MeV) when R1 is 2.5. But, as indicated in the rebuttal paper, the argument is weakened when R1 is much smaller, for example, 1.7 or less. Hopefully, the correct value of R1 will be determined on the basis of additional measurements. (P.S. The dE/dx, for a given material, depends on its main chemical composition; it does not depend on composition differences at the ppm level. Such differences always exist among batches from different manufacturers.)

e) I am happy to see the new sequential etching curve (Figure 2c in the SPAWAR rebuttal paper). It shows that sizes of pits due to alpha particles of 5.5 MeV grow at the same rate (versus etching times) as SPAWAR-type pits. Would the same be true for alpha particles of ~ 1MeV? This remains to be seen.

f) How should the size of a pit be defined? For a circular pit the size should be identified by the diameter; for an elliptical pit, on the other hand, the size should be identified with the minor axis. Why do I suggest this? Because I know that the major axis of an elliptical pit depends on the angle of incidence. It is interesting that the SPAWAR-type pit in the Figure 2c is nearly two times larger that the pit due to an alpha particles of 5.5 MeV. In other words, their R1=2.0. This is larger than 1.7, and closer to the value of R1=2.5 I obtained in 2007. The hypothesis of "1 MeV alpha particles" was formulated in 2008, as far as I know.

g) Note that according to SPAWAR figure 2c the value of R1 increases when etching times become shorter. That is very interresting! Extrapolating to 6 hours of etching, used in my 2007 replication experiment, the value R1 approaches 2.5. In other words, new SPAWAR data seem to be consistent with my first result. Extrapolating to 22 hours of etching, the value R1 approaches 1.3. This makes me think think that the difference between R1=2.5 and R1 "close to 1.7," might be due, at least partially, to differences between our etching times.

h) Fortunately, as described in Section 4 of the next unit, (click here) , the SPAWAR hypothesis can be tested more directly. Will experiments with alpha particles of ~1 MeV validate the SPAWAR claim? That remains to be seen.

i) The “corona discharge” argument, only mentioned in my paper (reference 8 and 9), was developed by Lipson et al. I am not familiar with details of their experiments. Because of this I am neither for nor against their position.

j) P.S. In the past I measured sizes of alpha particles under very short etching times. The tracks, already countable after about 2 hours of etching, grow when etching times become longer. Suppose each line (see Figure 2c in the SPAWAR rebuttal paper) remains straight, down to the etching time of 3 hours. That would produce R1=4.3 after 3 hours of etching and R1=3.5 after 4 hours. I suspect that something important might be learned, about dominant codeposition pits, from examining their behavior after short etching times. Dependence of R1 on the etching time, illustrated in the SPAWAR Figure 2c, is puzzling. Is it consistent with the SPAWAR hypotheisis about post-electrolysis pits? How can it be explained?

k) It is well known that prolonged etching (12 hours versus 6 hours) changes small elliptical pits, due to alpha particles, into larger circular pits. This was illustrated in Figures 4 and 5 at http://csam.montclair.edu/~kowalski/cf/336cat.html . Suppose that copious codeposition pits also change from "mostly elliptical" to "mostly circular," for example, between 3 hours of etching and 5 hours of etching. That would support the idea that tracks are due to low energy particles. Most circular SPAWAR-type pits touch each other (on my photos and on similar photos shown at the ACS meeting in Denver). Would smaller pits, seen after only 3 hours of etching, also be touching each other? Would small elliptical pits be oriented randomly or would some patterns be seen? I am thinking about clusters of two or more radially oriented tracks (see SPAWAR rebuttal paper).

l) l) Another parameter worth exploring, in searching for useful indicators, is the effect of the co-deposition time on track density. My guess is that the co-deposition time of one hour, for example, will produce 24 times less tracks than the co-deposition time of one day, under identical conditions. I know that preliminary experiments of that kind were performed by SPAWAR researchers. (Parallel cathode wires were disconnected at different times.) Was it possible to reduce the co-deposition time sufficiently to dramatically decrease track densities?

m) In the rebuttal paper, SPAWAR researches show several theoretically simulated tracks due to alpha particles of very low energy. Does their simulation code predict transitions from "mostly elliptical" to "mostly circular" pits? What etching times should be used to observe such transitions? People who developed the code probably tested its predictions by experimenting with real particles. Was the code tested with alpha particles of very low energies? If so then agreements with theoretical predictions could be used as arguments supporting the SPAWAR hypothesis.

n) P.P.S. Why are you "casing doubt?"--someone asked me recently. Because I am trying to help. It is my obligation as scientist. Casting doubt is part of being helpful. On the basis of my own experience, long before cold fusion, I can say that criticism is likely to be more helpful than compliments. Criticism often helped me to overcome personal bias. It is unfortunate that The Galileo Project, triggered by SPAWAR discovery, died so early (after Denver meeting, about two years ago). Progress would be much faster if those who confirmed initial SPAWAR results continued studying the phenomenon in unison, and argued about interpretations. That would be a natural next step for working with a protocol yielding reproducible results. Each of us already had a working experimental setup. Why did we stop working in unison so early?

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358) New phenomena or illusions?

Ludwik Kowalski (2/10/2009)

Montclair State University, New Jersey, USA

1. Introduction

How many readers remember the excitement generated by the announcement of the discovery of "cold fusion?" This event took place twenty years ago. The claim, made by M. Fleischmann and S. Ponds during a press conference in Salt Lake City (1), was that a nuclear process can be triggered by electrolysis (2). Deuterium nuclei, from heavy water in the electrolyte, were said to be so densely compressed, in the palladium cathode, that they could fuse despite strong Coulomb repulsion. The claim generated a lot of interest among scientists (3), as well as the general public (4). Laymen speculated about abundant, cheap, and pollution-free sources of energy; scientists were puzzled by the idea that a chemical process could produce a high concentration of atomic nuclei in condensed matter.

Many researchers attempted to confirm the Fleischmann and Pons results. But not all of them were successful. On that basis, and on the basis of theoretical considerations, the cold fusion claim was rejected by mainstream scientists (5). But more than one hundred researchers, worldwide, continue exploring the field. Cold fusion scientists meet regularly to discuss new experimental results and new theories (6). They no longer believe that cold fusion is similar to the thermonuclear fusion. For that reason the field is now called "Condensed Matter Nuclear Science," CMNS.

Like most nuclear physicists, I was excited by the possibility that a new kind of nuclear reaction had been discovered. And like most of them, I came to the conclusion that the original claim was totally unjustified, as claimed in (5). My professional interests were focused on nuclear reactions induced by heavy ions, and on the transmutation of highly radioactive nuclear materials. In 2002, at a scientific conference in Albuquerque (7), I heard several reports presented by CMNS researchers. Impressed by their qualifications, and by the content of reports, I decided to pay attention to their results, and to attend the upcoming CMNS conference (8). This was the beginning of my participation in CMNS activities.

The purpose of this article is to describe two kinds of experiments in which I participated; one had to do with the "excess heat," presumably due to a nuclear process, and others had to do with the chemically-induced emission of nuclear projectiles, presumably alpha particles or protons. The goal was to convince myself that at least one nuclear effect due to a chemical process was real.

2. Excess heat in high voltage electrolysis

An electrolytic cell, a device in which electric current flows through an electrolyte, converts electric energy into chemical and thermal energy. The amount of heat produced in the cell is expected to be smaller than the amount of electric energy received, unless chemical reactions are strongly exothermic. But, according to Fleischmann and Pons (2), the amount of heat produced in their cells greatly exceeded the electric energy received. The difference between the thermal energy released and the electric energy received was named excess heat (EH). The authors claimed that their excess heat could not be attributed to chemical reactions. The reality of EH, generated at the rate of about one watt, has been confirmed by numerous investigators (9). In most cases the potential difference between the electrodes was several volts while the current was a fraction of one ampere.

Excess heat was also discovered in cells operated at much higher voltages and currents (several hundred volts and more than one ampere). A schematic diagram of a cell operating under such conditions is shown in Figure 1. The cathode is a tungsten rod while the anode is a large platinum wire spiral, or a platinized niobium cylinder. The

electrolyte, in experiments in which I participated, was potassium carbonate (K_2CO_3) dissolved in distilled water. The concentration was 20 grams per liter. Decomposition of water, at high current, becomes so intense that yellow glow discharge and arcing can take place in the layer of gas-plasma surrounding the cathode.

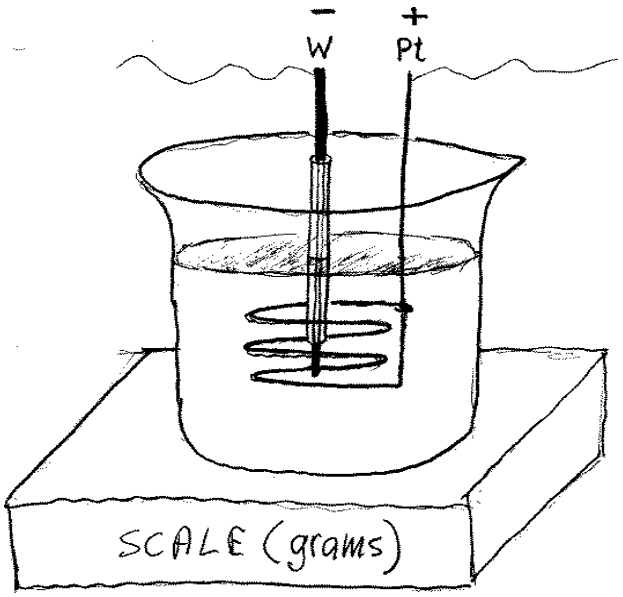

Figure 1

Generation of EH during the glow discharge plasma electrolysis was first reported by a team of Japanese scientists, T. Mizuno et al. (10). The phenomenon was then studied by other researchers, both in Japan (11, 12, 13) and in other countries (14, 15, 16). Most reports, but not all, confirmed generation of excess heat, as described by Eugene Mallowe (17). My first experiment of that kind was performed in cooperation with Scott Little. The cell was wrapped with an insulating layer of foam; the thermal energy released was calculated on the basis of the amount of evaporated water. Electrical energy received was calculated on the basis of recorded current and voltage, which fluctuated widely. Nominal potential differences were between 250 and 400 volts. No clear evidence of EH was found.

The same conclusion was reached when a similar experiment was performed in collaboration with Richard Slaughter. These findings, reported at a conference (18), were in conflict with those reported by Pierre Clauzon (19). The topic was discussed at length and Pierre decided to join us in another sequence of experiments. Working under his guidance, R. Slaughter and I were able to obtain nearly the same results as in (19), for example, about 30% more heat than electric energy, at 350 volts. This was accomplished by using Clauzon's electrical watt-meter. Two years later (Fall 2008), during another control experiment, Clauzon discovered a systematic error in measuring electric power with this watt-meter. His most recent results, obtained with a much better watt-meter, are consistent with negligible excess heat in high voltage electrolysis. Another team (16) also withdrew their excess heat claim for high voltage electrolysis. In both cases, inexpensive watt-meters were not able to deal with high frequency components of rapidly fluctuating currents.

I hope that new data, collected by Clauzon with a better watt-meter, will be formally presented at the ICCF15 next summer. Will this result in reinvestigation of earlier high voltage electrolysis experiments? This remains to be seen.

3. Emission of nuclear projectiles.

Emission of a large number of alpha particles, due to low voltage electrolysis, was reported by R. Oriani and J. Fisher (20, 21). The particles were discovered by using CR-39 track detectors. Many years ago I used track detectors (mica) to observe fission fragments. The principle of their operation is simple. Strongly ionizing particles create tracks in many solids materials; these tracks become visible under a microscope, after the material is chemically etched. According to (20), as shown in Tables 1 and 2, mean track densities in CR-39 chips inside the electrolytic cell were much higher than in control chips ourside the cell. Tracks in control chips were due to cosmic rays, radon and other possible radioactive contamination.

Impressed by the reported results, I quickly learned how to use CR-39 detectors. A year later Oriani sent me new results and additional details about the experimental method. The cathode in his small cell was a nickel foil, the anode was a platinum wire and the electrolyte was Li_2SO_4 dissolved in ordinary water. Subsequently Richard invited me to his laboratory and we worked together for one week. Two experiments were performed (22) and each produced results consistent with Oriani's claim. Unfortunately, my attempts to obtain similar results at home, using an exact replica of his cell, were not successful (23).

Several months later, again following Oriani's footprints, I started another sequence of experiments. In these experiment, described in (24), the nickel wire cathode was mechanically supported by the CR-39 chip while the platinum anode wire was above it. A thin layer of mylar (6 microns) prevented the cathode from direct contact with the chip. Oriani noticed that results become highly reproducible when the o-ring from a successful experiment is used in subsequent experiments. Richard started sending me his "seeded" o- rings and I used them in several experiments. Spectacular clusters of tracks, also reported by Oriani, were found on some of my CR-39 chips. One of the clusters is shown in Figure 2. Ten experiments were performed. Seven clusters of tracks were found in two out six electrolysis experiments; three clusters were also found in one of four experiments conducted to study emission of nuclear particles after electrolysis.

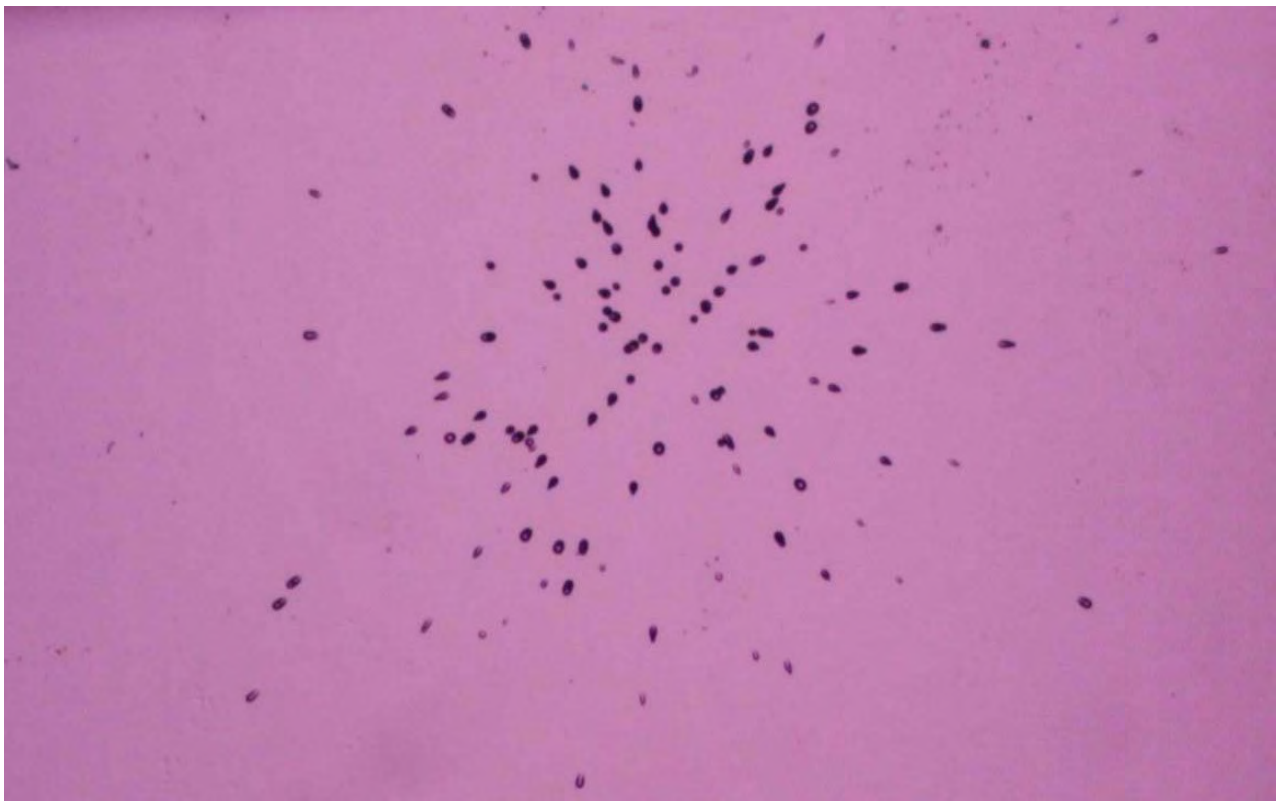

Figure 2

During the presentation (24), I said that a cluster with radially oriented tracks, as in Figure 2, could not possibly be due to contamination. But this point of view was challenged by two CMNS researchers. One of them invented the following scenario. "A gas bubble sits on top of CR-39 and a grain of alpha-radioactive material sits on top of the bubble. Ranges of alpha particles in the gas bubble are much larger than ranges in the electrolyte. That would produce a cluster whose size is much larger than ranges of alpha particles in the electrolyte, as in Figure 2. The tracks would be circular near the center and radial near peripheries, as in my Figure 2." The scenario invented by another researcher was similar; he was thinking about a small bubble trapped between the mylar film and the detector.

The issue of possible artifacts would be worth addressing if experimental results were reproducible. For the time being I am mostly troubled by the fact that four clusters were produced in Experiment I (lasting 5 days) and zero clusters were produced in Experiment II (lasting 21 days). Electrolyte, mylar film, cathode, and o-rings were unchanged

between the two experiments. Results which are not reproducible belong to proto-science, not to science, as illustrated in Figure 3. Two other CMNS researchers, John Fisher and Marissa Little, also observed clusters of tracks in CR-39 chips, using "seeded" o-rings received from Oriani. Suspecting radioactive contamination on the surface of the o-ring, Marissa exposed it to an electronic silicon detector (25). The energy spectrum of particles revealed peaks consistent with presence of Th-228, and its progeny. My attempts to discover alpha radioactivity on the o-ring surfaces, using CR-39 chips, were not successful. The origin of radioactive material on the surface of some "seeded" o-rings remains unknown. But I am troubled by high track densities on Oriani's control chips, reported in (20).

The most recent results of Richard's ongoing investigation were presented at ICCF14 (14th International Conference on Cold Fusion, Fall 2008, in Washington DC). The proceedings of that interesting conference should soon be published.

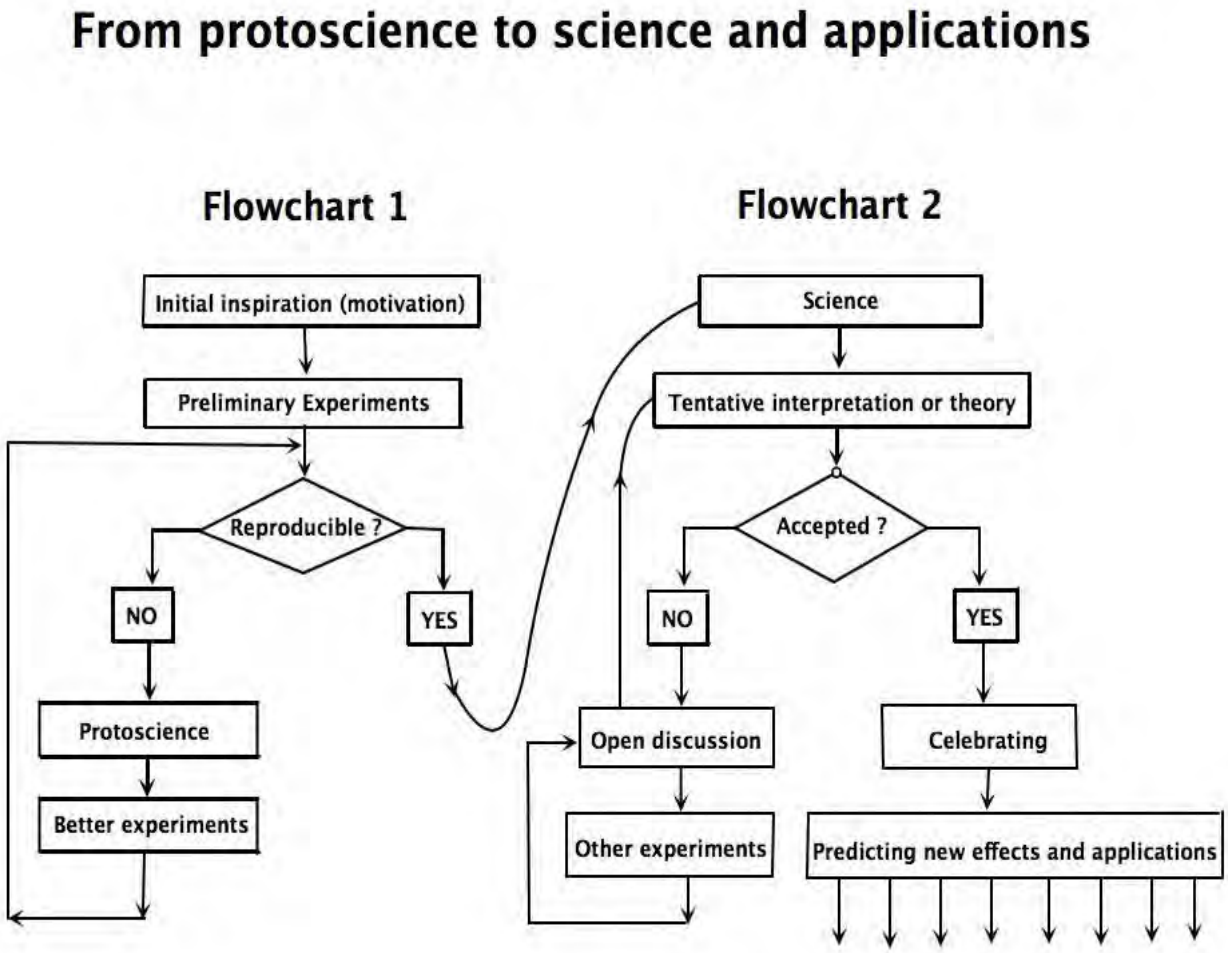

Figure 3

4. An Ongoing Controversy

At the end of 2006, researchers from a US Navy laboratory, SPAWAR, also announced detections of a large number of tracks said to be due to alpha particles emitted from the cathode of a low voltage electrolytic cell. These particles were detected in CR-39 chips that were in contact with cathodes, in the electrolyte (26). Helped by SPAWAR scientists, Steve Krivit, from the New Energy Institute, wrote the protocol for replication of one of the SPAWAR

experiments. That protocol was then distributed to those CMNS researchers who wanted to study the effect. All who were able to implement the protocol, including myself, reported seeing the SPAWAR-type tracks.

Some results were published at the ACS winter meeting in Denver, in March 2007. My slides shown at that meeting (27) were nearly identical with those shown by SPAWAR researchers. But our conclusions were very different; they speculated that dominant tracks were due to alpha particles, I speculated that our tracks could not possibly be due to alpha particles. In a subsequent internet discussion, and in a formal publication (28), they wrote that energies of alpha particles are likely to be close to 1 MeV. In my formal publication (29) I made two arguments against this tentative SPAWAR interpretation. Fortunately, the hypothesis of 1 MeV alphas can be tested by performing additional experiments. Alpha particles of approximately 1 MeV can be used and their tracks can be compared with tracks produced during electrolysis.

Suppose that the two kinds of tracks turn out to be very similar. That would reinforce the SPAWAR hypothesis and would justify more sophisticated experiments, for example, with electronic solid state detectors. SPAWAR type results are scientific because they are reproducible. The right diagram in Figure 3 shows how controversies are usually handled by scientists. Emission of nuclear particles of any kind, due to electrolysis, would be convincing evidence that a new kind of nuclear process has been discovered by CMNS scientists.

5. Final comments

The broad field of CMNS, recently reviewed by Ed Storms (9), remains controversial. But this did not stop research; many scientists continue searching for nuclear processes due to chemical reactions. Their progress would be faster if the field was not discriminated against, for example, by editors of scientific papers, and by various government organizations. In my opinion, most CMNS researchers are honest and competent and are motivated by the desire to promote science and technology. Several general observations about the field can be seen in (30). Here is one of them:

". . . What cold fusion really needs is a reliable demonstration experiment. That's what Ludwik Kowalski has been trying to find for several years now. We worked closely with him to see if Mizuno's incandescent W experiment (a la Clauzon et al) would do the trick. It didn't. That's what the Galileo Project was trying to establish. It didn't. .... or hasn't yet. That's what we are still pursuing right now in cooperation with Richard Oriani who continues to see relatively low level positive results from his PACA CR-39 experiments. That's why we continue to maintain our high performance calorimeter, MOAC, in good working condition so that cold fusion researchers can take advantage of our standing offer to test promising cold fusion cells free of charge.

Why go to all this trouble if we've never seen any real signs of cold fusion? Simple. If cold fusion is real, it will be of enormous importance to mankind. Lots of discoveries in science have come only after years of searching. Despite all our null results we still have some hope. But I also think that there is a finite chance that all of the apparently positive results that have been observed in cold fusion experiments are erroneous. i.e. the result of various artifacts. I know, it's hard to imagine how so many intelligent researchers could all be making such measurement errors. "

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359) Cold Fusion Anniversary

Ludwik Kowalski

Montclair State University, New Jersey, USA
March 23, 2009

THIS UNIT IS TOO LONG AND NOT WELL ORGANIZED. BUT SOME PEOPLE MIGHT HAVE LINKED TO THIS UNIT FOR THAT REASON I AM NOT DELEATING IT.

TWO UNITS #261 AND #262 WERE COMPSED FROM FRAGMENTS OF THIS UNIT (ON 4/25/2009). TOGETHER THEY REPLACE UNIT#359

click to see unit 361

click to see unit 362

1. Introduction
The discovery of Cold Fusion was announced exactly twenty years ago. I read several interesting messages about this, on the private website for CMNS researchers. Unfortunately, I had nothing valuable to contribute. But then a science journalist, who I do not know, sent me a URL for a file containing a recent SPAWAR paper.

http://www.newenergytimes.com/Library2/2008/2008BossTripleTracks.pdf

This paper was published in Naturwissenschaften (2009) 96:135–142. The title is "Triple tracks in CR-39 as the result of Pd–D Co-deposition: evidence of energetic neutrons. The authors are: Pamela A. Mosier-Boss, Stanislaw Szpak, Frank E. Gordon, and Lawrence P. G. Forsley. I knew about this paper but I did not have a chance to read it. The journalist, Mark Anderson working for "IEEE Spectrum," asked me to comment on that paper.

Why did he ask me? He probably knows about my successful replication of an earlier SPAWAR experiment, and about what I wrote in

http://pages.csam.montclair.edu/~kowalski/cf/357epjap.html

2. My comments
a) The methodology used is basically the same as in the experiment I performed two years ago. But a large number of control experiments make the reported results much more acceptable. Instead of focusing on dominant overlapping tracks, as in an earlier paper, the author analyze rare triple tracks, said to be due to high energy neutrons (above 9.6 MeV) emitted during electrolysis. They write that "among the solitary tracks due to individual energetic particles, triple tracks are observed. Microscopic examination of the bottom of the triple track pit shows that the three lobes of the track are splitting apart from a center point. The presence of three alpha-particle tracks outgoing from a single point is diagnostic of the 12C(n,n')3alpha carbon breakup reaction and suggests that DT reactions that produce ≥9.6 MeV neutrons are occurring inside the Pd lattice."

b) This paper confirms, once again, that codeposition experiments yield reproducible results. In 2007 I was one of several researchers who used the SPAWAR protocol and confirmed the reported results. That is why I think that codeposition experiments, developed by the SPAWAR team, belong to science, rather than to protoscience, as many other CMNS experiments. CMNS, by the way, is a new acronym for CF--what used to be called Cold Fusion is now

called Condensed Matter Nuclear Science.

c) In one set of control experiments the authors used the electrolyte made from H_2O. The number of tracks was found to be at least three orders of magnitude lower that in identical experiments in which heavy water, D_2O, was used. They wrote: "Since the natural abundance of deuterium in light water is 0.015%, it is possible that the tracks observed in the light water experiments could actually be due to Pd–D interactions. Microscopic examination of the CR-39 detectors used in Pd–D electrolysis has been done in areas where the density of tracks is less. In these areas, what appear to be triple tracks are observed interspersed among the solitary tracks. The number of these triple tracks is very low—on the order of a ten or less per detector and they are only observed in heavy water experiments. These triple tracks have been observed in every Pd–D co-deposition experiment that has been conducted using Ag, Au, or Pt cathodes in both the presence and absence of an external electric or magnetic field. When Ni screen is used as the cathode, tracks and triple tracks are only observed when an external electric or magnetic field is applied."

c) Figures 1a and 1b show pits due to alpha particles from Am-241. Clusters of triple tracks observed are due to overlapping--rare coincidences when three alpha particles hit nearly the same spot. Triple tracks created during electrolysis (in subsequent figures) are shown to be very different. That is a relevant point; triple pits produced during electrolysis do occur among single pits; showing that they are not due to overlapping is very important.

d) Control experiments described in the paper speak loudly against possible alpha-radioactive contamination.

e) A significant difference between track densities in D_2O and H_2O is also a strong argument against the chemical origin of tracks.

f) The authors conclude that "no tracks, single or triple, were obtained when $CuCl_2$ was used in place of $PdCl_2$." Yes indeed; what can be a better demonstration that Pd plays an important role in the claimed new kind of chemically-induced nuclear proces. To elaborate on this the authors write that "the deuterium must be inside a metal lattice for these reactions to occur and not simply adsorbed on the surface of the metal. This implies that the metal lattice facilitates these reactions indicating that nuclear phenomena can be influenced by the atomic and electronic environment." This is no longer an abstract speculation.

g) Presence of triple pits on both sides of the CR-39 chips is consistent with the idea that they are due to neutral projectiles of some kind (able to penetrate about 1 mm of the CR-39 material). Neutrons are the most natural candidate.

i) What is needed is an independent verification of these results. My suggestion would be to organize coordinated replications, for example, in a national laboratory. The cost of several hundred dollars per experiment is negligible in comparison with costs of routinely performed experiments in these labs. Unfortunately, I do not know how to make this happen. Confirmation of SPAWAR results (nuclear origin of tracks) would the best anniversary present to society. Will this happen during this special year?

P.S.
I was not the only one who posted a comment on SPAWAR neutrons today. One CMNS researcher directed me to:

http://www.chron.com/disp/story.mpl/headline/nation/6333164.html

where I found out about Paul Padley, a physicist at Rice University who reviewed Mosier-Boss' published work.

"Fusion could produce the effect they see, but there's no plausible explanation of how fusion could occur in these conditions," Padley said. "The whole point of fusion is, you're bringing things of like charge together. As we all know, like things repel, and you have to overcome that repulsion somehow."

The problem with Mosier-Boss' work, he said, is that it fails to provide a theoretical rationale to explain how fusion could occur at room temperatures. And in its analysis, the research paper fails to exclude other sources for the production of neutrons.

“Nobody in the physics community would believe a discovery without such a quantitative analysis,” he said.

I tend to disagree. I believe that claims based on unexplained, but reproducible-on-demand experimental data, have their own intrinsic values, especially when established ideas are challenged by new data. Emission of nuclear projectiles during electrolysis does conflict with preexisting ideas. An accepted theory of CMNS is likely to emerge very quickly after the experimental data reported by the SPAWAR team are independently confirmed, for example, by two or three teams of qualified scientists. That is why I think that organizing independent replications is more important, at this stage, than theoretical studies. What should be done to convince our government that an attempt to find a clear yes-or-no answer is worth undertaking?

Added on 3/24/2009

Here is a link to another article on the same topics:

http://www.sciencedaily.com/releases/2009/03/090323110450.htm

It informs us that many papers, on several CMNS topics, will be presented at the ongoing American Chemical Society meeting this week. Maybe this will help to generate action toward independent replications of experiments which are said to reproducible on demand.

The author writes: “. . . One of their problems involved extreme difficulty in using conventional electronic instruments to detect the small number of neutrons produced in the process, researchers say.” The “small number of neutrons produced” may or may not be correct. Yes, the SPAWAR team reported only about ten triple tracks per experiment (lasting two or three days, I suppose). But this does not include the CR-39 area in which tracks are said to be practically on top of each other. Furthermore, detection of fast neutrons via triple tracks is likely to be highly inefficient; I would not be surprised to learn that, on the average, only one triple track is produced by zilloins of neutrons. Most high energy neutrons produce recoilong protons in CR-39 material.

To see more triple tracks I would modify the SPAWAR cell geometry. Their cell, as far as I know, is a small rectangular plastic boxes filled with electrolyte. Suppose the cathode is placed in contact with one of the walls, inside the box, while a CR-39 detector is placed in air, in contact with the same plastic wall. With this arrangement, nearly 50% of neutrons produced in the cathode would be emitted toward the CR-39 detector, only 1 mm away. Low energy alpha particles would be stopped in the wall. (With the present arrangement--CR-39 inside the electrolyte--copious particles are said to interfere with detection of triple tracks.) With the modified geometry one would see many recoiling protons for each triple track due to high a energy neutron.

Note that the modified geometric arrangement would allow me to replace CR-39 by a sophisticated electronic detector of neutrons. Scientists in National Laboratories have many decades of experience with sophisticated detectors of neutrons.

Added on 3/25/2009

Several CMNS researches posted comments about SPAWAR neutrons on our private discussion list. Here is my own contribution.

1) I do not think that focusing on energies of emitted neutrons (using electronic detectors) is the most desirable next step, as far as SPAWAR experiments are concerned. But let me speculate a little about electronic detectors. At the end of unit 359 I wrote: "To see more triple tracks I would modify the SPAWAR cell geometry. Their cell, as far as I know, is a small rectangular plastic box filled with electrolyte. Suppose the cathode is placed in contact with one of the walls, inside the box, while a neutron detector is placed in air, in contact with the same plastic wall. With this arrangement, nearly 50% of neutrons produced in the cathode would be emitted toward the CR-39 detector, only 1 mm away." I was referring to the CR-39 detector, because that is what Pamela et al. use. And I still think that this is the most desirable think to do.

2) A more sophisticated (and expensive) instrument would be a telescope of two Si detectors, thin and thick. Such commercially available setups are routinely used to detect energetic charged particles, such as protons, deuterons, alphas, etc. With proper electronics (also commercially available) one can easily accomplish three things at the same time: identify particles traversing two detectors, determine their energies, and count the particles. Note that a plastic wall of a SPAWAR cell contains hydrogen atoms. Neutrons colliding with these atoms produce protons. Information about neutrons can be obtained from what one learns about recoiling protons. A plastic wall of 1 mm, however, is too thick; it would have to be replaced with a much thiner window, for example, a window in which 10 MeV protons lose no more energy that 2 MeV. The thinner the window the highest the energy resolution is. Unfortunately, desirable high resolution goes together with undesirable low counting efficiency (for example 10^-4 protons per neutron versus 10^-8 p/n). A resolution of +/- 2 MeV would probably be sufficient, at this stage.

With the threshold of 6 MeV, the electronic noise (background to be subtracted) can probably be reduced (with proper shielding of preamplifiers etc.) to less than several coincidences per day. Suppose that a pre-calibrated multichannel analyzer (0.12 MeV/channel and zero intercept) shows a spectrum of protons with a peak at channel 85. That would tell us that 0.12*85=10.2 MeV neutrons were present. Suppose that 100 of such protons are recorded in two days. Suppose that the efficiency (calculated on the basis known elastic scattering cross sections, and the number of atoms of hydrogen in the window) is 10^-6 p/n. That would mean that 100/10^-6=10^8 neutrons passed the cell's window (580 neutrons per second, on the average). I wish I knew how to estimate the efficiency for producing a triple track in CR-39 by 10 MeV neutrons. My guess is that 10^8 neutrons would produce one such track or less. Counting neutrons via recoiling protons (in thick CR-39) is most likely to be more efficient than counting them via triple tracks in CR-39 (or via recoiling protons produced in a thin window in front of a telescope of Si detectors).

3) Fortunately, CR-39 can be used to detect recoiling protons of several MeV. It would be a waste of effort to abandon CR-39 detectors at this time. Reproducible-on-demand protons of several MeV, in a large CR-39 chip, located outside the cell, would be much more numerous than identifiable triple tracks, in already performed experiments. I am thinking about hard-to-recognize triple tracks in the region where single tracks are overlapping, as stated in the SPAWAR paper. Note that no thin window would be needed; neutrons have no trouble in traversing the cell wall and producing recoiling protons in CR-39. Placing the cathode foil next to the cell wall should be the only needed cell modification. Protons recorded outside the cell would be a sufficiently convincing evidence of a nuclear process due to electrolysis. The lower energy limit of these protons, for example, 5 MeV, can easily be determined from the effect of thin foils on the number of recorded tracks.

4) Use of expensive electronic detectors (spectrometers) will be fully justified after showing that recoiling protons are indeed produced in CR-39 chips, outside of SPAWAR codeposition cells.

Added on 3/26/2009

The journalist, Mark Anderson, who sent me the SPAWAR-neutrons paper (and who subsequently interviewed me on the phone), published a short article about cold fusion in IEEE Specrum

http://spectrum.ieee.org/mar09/8407

Appended on 3/31/2009

This evening Richard Oriani posted an important message, on the Internet list for CMNS researchers. Addressing SPAWAR team he wrote: **"You have reported finding triple tracks in CR39 detector chips that had been placed within operating electrolysis cells and have pointed out that three nuclear particle tracks emanating from one point indicate the occurrence of the reaction 12C(n,n')3 alphas, suggesting the production of neutrons of energies 9.6 MeV or greater. I have also found triple tracks going out from a central point, but I have also found double, quadruple, and higher-order multiple tracks emerging from a single point. (See the attached images). You may be right in you interpretation, but in view of the variety of the numbers of tracks with a common point of emergence that can appear after electrolysis it seems rash to ascribe to one member of the family a specific nuclear mechanism that cannot apply to the other members. I suggest that other supporting**

evidence is needed before your interpretation can be accepted."

Replying to this, I wrote: **"Attached is a file showing my cluster of "multiple tracks emerging from a single point." You can also see it in Figure 7 at**

http://csam.montclair.edu/~kowalski/cf/336cat.html

The attached picture was trimmed. The larger picture (Figure 7 under the above link) shows that the cluster is not surrounded by numerous single tracks. Suppose this cluster is due to a rare reaction induced in CR-39 by fast neutrons. In that case there would be a lot of single tracks due to elastic and inelastic collisions of such neutrons with hydrogen nuclei. Absence of tracks due recoiling protons is a valuable argument against the idea of high energy neutrons. Note that I was using Oriani's protocol, not the SPAWAR protocol. A clear yes-or-no answer, about the hypothesis of high energy neutrons can be obtained in the modified version of SPAWAR experiment, [as described earlier in this unit]. Does this suggestion make any sense? Is it worth implementing? "

I have observed recoiling protons in CR-39 chips exposed to a Pu-Be source of neutrons, many times. I would not fail to recognize them. These tracks are similar to those due to alpha particles from Am-241, but most of them are about one half the size of alpha tracks.

The lead article in the Science and Technology section of The Economist (March 28 2009) also has an article on SPAWAR neutrons:

http://www.economist.com/science/displaystory.cfm?story_id=13361472&CFID=49711510&CFTOKEN=42825308

The author, who was apparently present at the Salt Lake City conference last week, writes that **". . . most researchers in the field, though, do not accept that heat is sufficient evidence of fusion (if only because it was the basis of the Pons/Fleischmann claim). So to strengthen her case, Dr Boss placed a special plastic called CR-39 next to the hot electrode. If fusion was taking place, then neutrons flying through the plastic would cause protons within the material to recoil, leaving telltale tracks. Studying CR-39 under a microscope and counting the number of tracks is a standard way to assess how many neutrons bowled past."**

In reading the article, I realized that SPAWAR own experimental data can be used against the hypothesis that fast neutrons are responsible for triple tracks. Here is my tentative argument. Suppose the hypothesis is correct. In that case triple tracks would be surrounded by an overwhelming number of tracks due to recoiling protons. But, according to the SPAWAR published article, and according to their 2007 presentation in Catania, triple tacks were surrounded by single tracks due to alpha particles, not recoiling protons. Surrounding tracks were definitely too large to be attributable to protons. That confirms what Richard Oriani wrote, multiple tracks emerging from a single point are not due to neutrons.

Fortunately, SPAWAR experiments are said to be reproducible. Therefore a clear answer, about generation of high energy neutrons in co-deposition experiments, can be obtain by performing the experiment I suggested. What can be more scientific than this? Will such experiment be performed? This remains to be seen.

Appended on 4/3/2009

The current discussion of neutrons emitted during SPAWAR experiment reminded me of a 2007 report of Larry Forsley (at "8th International Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals," in Catania, Italy). Larry is a member of SPAWAR team. I do not know why his report is not printed in the Workshop Proceedings. Perhaps this would conflict with plans to publish the paper in a journal).

But another paper (page 182 in the Proceedings), is related to neutrons emitted in SPAWAR-type experiments. The authors of the report are Lipson et al (Russia), and Tanzella et al (USA). Experiments were performed in SRI (Stanford Research Institute) but CR-39 were sent to be analyzed in Moscow.

The abstract states that it is a **"preliminary evidence for the fast neutron emission. The energy is estimated to be in the range of ~2.2 - 2.5 MeV with a rate of 1 - 3 n/s."** That evidence was based on recoiling protons, from scattering of neutrons. Protons produced during electrolysis were compared with protons resulting with collisions with neutrons from a Cf-252 source. No subsequent report was presented at ICCF14, last fall. It probably means that nothing more was done along this line of research. If neutrons of ~2.4 MeV are really emitted, at the rate of one per second, during SPAWAR type electrolysis, then triple tracks would be surrounded by tracks from single protons--probably millions of them per triple track. That is a puzzle.

The same report sheds light on another puzzling aspect of SPAWAR results. The first 2007 idea was that tracks (whose presence I was able to independently confirm) were due to alpha particles of several MeV. My analysis conflicted with this interpretation; tracks were too large for this. The Catania report of Lipson et al. informs us that one of the two CR-39 chips, sent to Moscow, was covered by a mylar film during electrolysis. Alpha particles of several MeV (but not 1 MeV) would travers the film and would produce tracks in CR-39. Some nuclear tracks (densities exceeding the background) were observed, but the number of tracks was orders of magnitude less than what is observed when a CR-39 is in direct contact with the cathode, during electrolysis. In other words, at least 99% of SPAWAR trcks, seen in our replication experiments, disappear when CR-39 is covered with mylar.

These results are in conflict with the idea that SPAWAR "copious tracks" are due to alpha particles of several MeV (or to other charged particles able to traverse the mylar film). That is how, I suppose, the interpretation shifted from alphas of several MeV to alpha particles of about 1 MeV. But that is my guess; SPAWAR papers do not mention the results reported by Lipson et al, and Tanzella et al. Why is it so?

Appended on 4/4/2009

I am surprized that no one on the CNMS list answered my question, even those who were involved (Tazella, Lipson, Boss, etc.) Why do they prefer to remain silent? Perhaps they do not read my messages. But I received a private message from Richard Oriani, who does cover CR-39 chip with mylar. He suggested that I try to replicate his recent PACA results, as described in unit 333 at my website (see the link below). This prompted me to post the following message on the Internet list for CMNS researchers:

Two CR-39 protocols have been used to discover, and to confirm, reality of nuclear-like tracks produced during electrolysis: Oriani's ordinary-water protocol and SPAWAR heavy-water protocol. I was lucky to familiarize myself with these protocols (helped by Richard and by Pam) and to observe tracks with my own eyes. Actually, there are two Oriani protocols; let me call them A and B. Protocol A, described at

http://pages.csam.montclair.edu/~kowalski/cf/333physrevc.html

can be used in a totally independent experiment." Results reported in Oriani's rejected manuscript (see the URL above) are more recent and they are said to be 100% reproducible. I know that many people on this list are able to independently replicate Oriani's protocol A. With this in mind, I would like to suggest a cooperation, to be called "Curie Project." The purpose of The Galileo Project, organized by Steve Krivit, was to independently confirm SPAWAR results; the purpose of Curie Project would be to do the same for recent Oriani's results (see the URL above). I am certain that Richard will be happy to assist those who need technical help. I have enough CR-39 for at least ten experiments. I can also perform microscopic examination of already-etched CR-39 chips, for someone who has no access to a microscope. The entire experiment, including etching, must be performed in different labs (a possible alpha-radioactive contamination is no longer a problem after etching).

We would give ourselves a deadline, for example, to finish experiments before the end of May or June. Then one of us would draft a cooperative paper to be submitted to a journal. All results, both positive and negative would be reported. Later we would discuss which paper to choose. More detailed reports, focusing on individual results, are likely to be suitable for ICCF15. Please reply by sharing what you think about this idea, even if you are not interest in participation in the Curie Project.

I think our experiments should be as identical as possible. Let us use a constant-voltage source of 12 volts (a car battery or an equivalent power supply). We all should be using Ni foils as cathodes (Pd is more expensive) and light distilled water (heavy water is much more expensive). Richard, confirm that these are good suggestions. If not then suggest something else.

2) Do you know people who are not on the list but able to participate in the Curie Project? If so then please share this message with them. In my opinion, any serious researcher, willing to conduct a CMNS experiment, should be recommended to Haiko.

Appended on 4/5/2009
No one (except Oriani, in a private message) replied to what was posted yesterday. This prompted me to post another message:

"1) In a private message, Oriani wrote (I have permission to quote): 'I applaud your effort to stimulate the replication of my results that showed reproducibility. I shall be glad to provide advice whenever needed to whomever is interested in carrying out the project. I emphasize that the demonstration of reproducibility of development of nuclear tracks in CR39 detectors placed in electrolysis cells is the desired goal. Therefore at least ten separate experiments with appropriate controls will be required from an experimenter.'

2) A list of what is needed (including prices), to replicate Oriani's protocol, can be seen at:

http://pages.csam.montclair.edu/~kowalski/cf/188oriani.html

Figure 1, next to the list, shows the cell, as we used it before Richard's PACA protocol was introduced. PACA stands for 'Protected Against Chemical Attack.' He introduced this protocol after SPAWAR CR-39 results became known in 2007. The setup is the same except for the following:

a) The CR-39 detector is no longer where it was (inside of the lower tube).

b) What is labeled "nickel" should be a CR-39 chip, separated from the electrolyte by a thin film (6 microns or so) of mylar.

c) A nickel cathode wire (with a foot resting on the mylar) is introduced into the cell from above, more or less like the anode.

Please correct me if I am wrong, Richard. Also provide additional details, if needed.

3) The photo of my cell, and additional details can be seen at:

http://pages.csam.montclair.edu/~kowalski/cf/192logbook.html

4) Another picture of my cell (for the experiment Richard wants us to replicate) can be seen in Figure 3, at:

http://pages.csam.montclair.edu/~kowalski/cf/336cat.html

Figure 2, by the way, shows a spectacular Richard's cluster; Figure 4 is a similar cluster from my replication experiment.

5) Do you see the question in Figure 1? The purpose of Curie Project would be to answer it. Is it true that nuclear-like tracks, not attributable to background, are reproducible on demand, when PACA protocol is used? The Galileo Project did answer this question positively for the SPAWAR protocol. Will the same be true for the PACA protocol? This is possible, provided at least three more sets of independent experiments are performed.

6) Please share what you think about the idea of performing replication experiments at the same time.

7) Yesterday I wrote: 'Do you know people who are not on the list but able to participate in the Curie Project? If so then please share this message with them.' Was it a good idea?

8) With some hesitation, I suggest we use nickel from the same batch, and from the same supplier. Perhaps Richard will be willing to order a box of foils and send one foil to each of us. This would lower the cost; suppliers do not sell single foils. Other expenses could be lowered in the same way, provided participants pay for what is sent to them, including postage.

9) Not everyone is likely to have time for ten replication experiments, suggested by Richard. What about only four, providing at least four people participate in the Curie Project? A person (or a team) deciding to participate should make a commitment within a week or two."

Appended on 4/7/2009

One person wrote that the suggested deadline in not realistic. Responding to this, I wrote: **"Let me push the suggested deadline (for making Curie-Project commitments) to the end of May. This would be four months before the ICCF15. We would report results, or preliminary results, at that conference, if we are ready. Otherwise we would continue working till a clear yes-or-no answer is found. Additional comments and suggestions would be appreciated. Is the Curie Project worth organizing? Is the task well defined?**

P.S. My last message referred to Ni foils. That was a mistake. I forgot that the cathode, in Richard's PACA experiments, is no longer a foil, it is a wire. Should we use wires from the same spool or is it better if each of us uses a nickel wires from a different manufacturer? Even very tentative commitment would be appreciated on this list. This could encourage others. Thanks in advance.

John Fisher, who worked with Oriani, and reported the results at ICCF10, posted the following comment: **"Although I think we could learn a lot from the Curie Project I doubt that it would convince a hard-nosed skeptic.**

*****>Even if all of the participants were to report positive results, the skeptic would note that they had selected themselves. The request for volunteers was submitted to a large number of individuals. Before committing to the project, many might have attempted to achieve a positive result. Those that failed may have declined to participate. In light of this potential bias the skeptic would reject a claim of reproducibility.**

*****>If some participants report positive results and some report negative results, the absence of reproducibility would be demonstrated. The skeptic would likely believe that unidentified and uncontrolled factors were responsible for the positive results.**

*****> The protocol does not include studies of radon contamination, of other radioactive contamination, or cosmic rays, to eliminate them as causes of the etch pits.**

*****>It has not been shown that in all cases the etch pits are actually associated with particle tracks, and the identities of the presumed particles making such tracks have not been determined.**

In light of such considerations a skeptic would probably conclude that although something odd my have been observed in some electrolysis experiments, there is no reason to believe that these observations are evidence of nuclear reactions.

If, however, we view the project as a means of educating ourselves, of discovering factors that disfavor or favor reaction and removing or enhancing them as appropriate, we then may be able to achieve a reliably reproducible protocol and a much stronger project.

This is an interesting observation. It seems to apply to all CMNS projects, not only to CR-39 work. I was not thinking

about the "hard-nose skeptics;" I was thinking about majority of honest scientists who would start performing reproducible-on-demand experiments, and start testing their theories against undeniable results.

Responding to John Fisher, I wrote **"I think that we should not worry about "hard-nosed skeptics." We should try to convince common scientists, most of them are honest and objective. Suppose four teams participate in the Curie Project, performing 16 experiments. Suppose that 15 of these experiments show nuclear-like tracks, at densities at least five times higher than the background. Suppose that this is published, for example in EPJAP or in Phys. Rev. Suppose that journalists write about our results in newspapers. I am certain that this would contribute significantly to the ending of discrimination against our field."**

And what if results are inconclusive, or negative? Negative results would convince us that Richard's tracks were not due to a nuclear process resulting from electrolysis. We would be disappointed but that would not prevent us from obeying the rules of the game. Would we still publish the results or would we hesitate, thinking that "this might hurt the field"? I would vote for publishing negative, or uncertain results. Uncertain results would prolong the agony; they would again indicate that something is going on but we do not know how to control it. A poisoning effect of some kind (as postulated by Peter Gluck) or an elusive NAE (Nuclear Active Environemtas postulated by Ed Storms)? Uncertain results would force us to continue, obeying scientic-method. Natural human emotions, such as tendency to deemphasize (or hide) negative (presumably explainable) results should be resisted at all costs. Yes, it is difficult to be objective. Based on my experience so far, I am afraid that some experiments will produce tracks while others will show nothing above the fluctuating background.

Appended on 4/10/2009

The 20th anniversary of the Salt Lake press conference prompted me to think again about the strategy needed to convince mainstream scientists that our claims are valid. I think that the issue is worth discussing. My advice would be not to inject theoretical interpretations until facts are recognized as real. Remember what happened in 1989. Instead of focusing on real experimental facts (generation of excess heat) discussion quickly shifted to theoretical considerations, such as coulomb barrier, expectations based on wrong models, etc. It would be much better if the new phenomenon were called UEH (unexplained excess heat) rather than CF (cold fusion), until the reality of UEH were recognized by most scientists.

Explaining facts in terms of unexplained ideas seems to be counterproductive, at this stage. But this is not something unheard of. I am thinking about the famous paradox of missing energy in beta decay. Calorimetric measurements of the mean energy per beta particle, conducted in 1930s, were not consistent with the law of conservation of energy. To explain these experimental results, Pauli invented neutrino, a particle of negligible mass that carries the missing energy. I suppose that many people had reservations about this, just like many of us resist premature explanations based on polyneutrons, erzions and magnetic monopoles. But Pauli’s ad hoc hypothesis was eventually shown to be correct by Cowan and Reins (1950’s).

In our situation, however, mixing experimental facts with theories might backfire again. What we are facing is more complicated than a conflict between valid nuclear theories and new experimental data. We are also facing a political conflict between two groups of scientists. We want people to look at our experimental data; we want them to perform experiments and to focus on critical analysis of results. Yes, pure empiricism is not science. Yes, theoretical debates are essential. Knowing what happens is not the same thing as knowing why it happens. Progress is faster when theory and experiment go hand in hand. But, like other powerful tools, theories can have both positive and negative effects. I am afraid that premature theoretical considerations can produce more harm than good at this delicate stage. Experimental data are easier to defend than ad hoc theories. Let us fight in terains with which we are familiar; let us avoid terains where we are weak. Theoretical people will probably do the same and the two tracks will coexist without interacting formally with each other, for the time being.

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370) What is going on?

Ludwik Kowalski

Montclair State University, New Jersey, USA
June 11, 2009

1) What follows is a message I posted on the private list for CMNS researchers. It a reply to a comment on my ongoing attempt to verify Oriani's claim that pits he observes are indeed reproducible on demand. Three other researches are going to make the same attempt this year. I will certainly have a unit devoterd to this cooperative project. I am showing this message because it leads nicely to updating of the situation with SPAWAR-type experiments. I wrote:

"Yes, the first question is hard to answer because we do not know how particles are emitted. I suppose they are emitted continuously during electrolysis. But that might not be so. One thing, however, is certain--one long experiment will not produce less tracks than an experiment lasting only three days. Your comment about not counting tracks outside the o-ring was a useful reminder. Your ICCF14 paper reported tracks on the "other side" of CR-39. The side facing air, rather than mylar and the electrolyte, might contain tracks due to radon in air.

There are so many things to study, once results become reproducible on demand. Another parameter to investigate would be the effect of magnetic field, as in SPAWAR experiments. Showing that presence of magnetic field changes the density of excessive tracks significantly would be a very convincing argument against many artifacts. In the last paper SPAWAR people show how magnets are used to keep hold pieces together. I would very much like to know if their earlier observations (strong effect of magnetic and electric fields of production of tracks) were confirmed by more recent experiments. Perhaps Pamela will tell us something about this (I have strong neodymium magnets, ready to be tried)."

2) Two days passed and no one responded. This prompted me to post another message. I wrote: "Dear all, the most recent SPAWAR publication, at:

http://lenr-canr.org/acrobat/MosierBosscharacteri.pdf

is worth reading and discussing on this list. About 90% of previously-observed pits disappeared when a thin mylar film was inserted between the cathode and the CR-39 detector. That simplifies the analysis of the remaining pits; they do not overlap as much as before. The mylar film absorbs alpha particles with energies up to 1.45 MeV. The previous hypothesis, that most pits are due to alpha particles of about 1 MeV, is thus rejected. The current thinking is that ""the majority of the particles formed as a result of Pd/D codeposition are 0.45-–0.97 MeV protons, 0.55-–1.25 MeV tritons, 1.40-–3.15 MeV 3He and/or 1.45-3.30– MeV alphas.""

This conflicts with results from another codeposition experiment, described in Section 5 of:

http://lenr-canr.org/acrobat/KowalskiLcommentson.pdf

A mylar film, of equal thickness was used in that earlier experiment. But results were dramatically different. First, the number of pits was orders of magnitude smaller. Second, the pits were identified as protons, suspected to be produced by 2.5 MeV neutrons. Is this a real discrepancy or are the different results (from two presumably identical codeposition experiments) due to something different in how the experiments were performed? Knowing what made

the difference seems to be very important. Were magnets used in both experiments? “

3) After posting the above I started to read the now-published proceedings from Catania conference. Then I posted this message:

“This question is answered on page 182 of Catania proceedings. No external electromagnetic field was used in SPAWAR-type experiment performed at SRI. The last paper of Pamela et al., on the other hand, shows that two magnets were used in their experiment (see the first link above). It seems that the difference (eliminating only about 90% of pits versus eliminating at least 99.9% of pits, by mylar) is due to the magnet. This is the only logical conclusion, provided experimental results are indeed reproducible on demand. What else can be responsible for the dramatic difference? I tentatively translation "200 tracks in 15 days of electrolysis" into "at least 99.9% pits were eliminated by mylar." Is this acceptable?

P.S.
(a) Actually, the magnetic field is not the only parameter that has a dramatic effect on the number of tracks created in SPAWAR-type experiments. Nearly all tracks disappear when D2O, used to make the electrolyte, is replaced by H2O, and when PdCl2, also used to make the electrolyte, is replaced by CuCl2. These are extremely powerful arguments against many suspected artifacts. Details are in Section 1 of

http://lenr-canr.org/acrobat/MosierBosscharacteri.pdf

(b) Why is the report by Lipson, Tanzella et al. not even mentioned by Pamela et al. in their paper

http://lenr-canr.org/acrobat/MosierBosscharacteri.pdf

published two years later? Is there a good reason for ignoring what was independently reported in Catania? If so then please let us know.

(c) Who else, besides the SRI team, continued replicating SPAWAR-type experiments, after phase one of The Galileo Project? We have an ideal situation, at least three parameters influencing the unexplained nuclear activity, have been identified. Perhaps it is time for initiating phase two of that project.”

4) I am still hoping that Pamela, or someone else from SPAWAR team will comment on what I wrote. Meanwhile, oneCMNS researcher (Ed Storms) posted the following observation: “

“You should consider that the radiation produced by SPAWAR conditions is not produced by the same conditions that produce heat and helium. In other words, the conditions determine the nuclear product. Consequently, this experiment says nothing about the other aspects of CF.

When neutrons are detected, they are always produced at a very low rate compared to tritium and even lower compared to helium. In fact, these three products appear to have no relationship to each other. Helium seldom occurs with tritium or neutrons. Tritium and neutron can occur at the same time, but with a t/n ratio that is highly variable with an average near 10^6. In other words, the conditions giving the different nuclear products should not be used in theories to explain all nuclear products. The SPAWAR conditions are producing the least active of the possible nuclear reactions.”

5) Responding to the above, I wrote: “This might be true. But ‘the least active’ does not bother me. We are not yet at the stage at which scientific understanding can lead to useful devices. Reproducibility on demand, confirmed in phase one of The Galileo Project, seems to be more important, at this time. Information from experiments that are not reproducible is not very reliable. What other experiments, demonstrating presence of unexplained nuclear particles, reached the same status as SPAWAR-type experiments, as far as reproducibility on demand is concerned?”

Responding to Ed Storms, a theoretically-inclined researcher (Andrew Meulenberg) posted several messages about significance of “the least active.” I am not going comment on their ongoing debate. But here is how Ed responded to

my message above: “My point Ludwik, is that an effect, although reproducible, must be applied properly to understand CF. I suggest this result may be caused by a mechanism that has no relationship to the main CF effect. While it is unique and gets people's attention, it may not give any insight into the major reaction, which is helium-heat production. In other words, people may be chasing a distraction even though this distraction is interesting from a scientific point of view.”

Here is my reply: “My recollection is that excess heat was measured in earlier SPAWAR-type experiments. Is this correct? In my opinion, any electrolysis, based on D2O, is a potential candidate for understanding helium-heat production. Other CMNS reactions, described in your book, are also worth studying”

”Ed responded “Although heat was claimed, my efforts to replicate this claim have revealed many problems with such a measurement. I don't think this claim can be accepted until it is replicated at higher levels when these problems are eliminated.”

I wrote: “Scott Little made an offer to measure excess heat with his sensitive instrument for other researchers. If I were Pamela, I would bring the cell to Texas and would perform at least two experiments inside Scott's calorimeter. The instrument is probably available on short notice. It has more than enough space for several SPAWAR cells, used at the same time. They can be connected in series.”

In another message I wrote: “What would I do if I were a powerful NSF director? I would release the following solicitation for proposals: "It has been reported that SPAWAR-type CMNS experiment, demonstrating a nuclear process triggered by electrolysis, reached the level of reproducibility on demand. To verify this claim, and to promote further research, NSF is now willing to support further investigations. Financial support for five grants, $50,000 each, is available to qualified scientists. Please submit your proposals as soon as possible; the deadline is December 30, 2009."

6) Several hours later I asked another question: “Another question to authors of paper

http://lenr-canr.org/acrobat/MosierBosscharacteri.pdf

I have no doubt that pits shown in Figure 6 resemble tracks of nuclear particles. Looking more carefully, I see that sizes of dark pits in Figure 6, are much larger than sizes of pits in Figure 7.

All pits in Figure 7 are due to alpha particles (of several energies). The photo in Figure 6a and all photos in Figure 7, cover an area whose height is 100 microns. Unfortunately, the magnification 500 is specified for the Figure 6a only. Should I assume that pictures in Figure 7 were taken under the same magnification as the picture in Figure 6a? If not then what was the magnification for pictures in Figure 7?

7) We were informed that magnification was 500, as I assumed.

8) My next post: “Please tell me what is wrong with the following argument.

a) Both Figure 6a and Figure 7d are self-calibrated (by 100 micron arrows along borders). Therefore diameters of pits, shown on these figures, can be determined independently of each other, and independently of magnifications under which the pictures were taken.
b) I zoomed on a dark pit in Figure 6a (near the upper right corner); that pit is due to a post-electrolysis particle. Then I determined its size on my screen (in millimeters). After that, under the same zoom, I determined the size of the largest pit in Figure 7d (also in millimeters on my screen). Pits in Figure 7d are due to alpha particle of very low energy; they were nearly stopped by a mylar filter of 24 microns.
c) I divided the first size by the second size and the ratio happened to be 2.0. I concluded that post-electrolysis pits are typically twice as large as pits due to alpha particles of very low energy.

The sizes could have been expressed in microns (by measuring the length of the 100 micron arrow on my screen). But I do not think that this is necessary; I am interested in ratios of sizes not in individual sizes expressed in microns.

Where am I wrong?

P.S.

d) Who else can confirm that sizes of pits due to alpha particle, recorded two different batches of Fukuvi CR-39 material, can occasionally differ by as much as a factor of two, under the same etching conditions? I would certainly noticed this, using Fukuvi chips from different batches for several years. My chips are also irradiated with alpha particles, at a little corner spot. The first thing I always do is to look at that spot, to verify that etching was successful.

e) Unless convinced by missing information, I will continue claiming that dark pits in Figure 6a are about two times larger than pits on Figure 7d.

9) Let me review the evolving interpretations, based on reproducible on demand SPAWAR data.

a) The first hypothesis was that "copious pits" are due to alpha particles of several MeV (which was not consistent with my own successful replication results, under The Galileo Project. The pits at the little corner spot were 1.7 times smaller than typical post electrolysis pits.

b) The second interpretation, emerged after our Catania conference, nearly two years ago, was that pits are due to alpha particles of about 1 MeV. Please confirm or deny that this resulted from information provided by F. Tanzella et al. As far as I know, they were the first to use a thin mylar filter in a co-deposition experiment.

c) The interpretation presented in the most recent SPAWAR paper (April 2009), based on new information, is different. Some tracks are now attributed to protons and tritons while others are attributed to 3He and alpha particles with energies between 1.5 and 3.5 MeV.

d) Changing interpretations, after learning more from new experiments, is a sign of health, especially in our situations. Taking (c) for granted, one is led to believe that large pits in Figure 6a are due to alpha particles. That is why I expected sizes of pits in Figure 6a be the same as sizes of pits in Figures 7e and 7d.

e) But dark pits in Figure 6a seem to be twice as large as pits due to alpha particles of 1.5 to 3.5 MeV. How can this be interpreted? The only thing I know is that pits due to heavier ions, such as fission fragments, are about two times larger than pits due to alpha particles. Yes, it is crazy to think about fission. But this is not less crazy than thinking about fusion. Reproducible acts, should always be respected. Correct interpretation will emerge, sooner or later. I think that we are making progress.

f) The new design of SPAWAR electrolytic cell, shown in Figure 1 of

http://lenr-canr.org/acrobat/MosierBosscharacteri.pdf

seems to be perfect for replacing the CR-39 chip with a commercially-available silicon detector. That is what I would recommend to do next. ”

Let me summarize the most recent SPAWAR paper, available at

http://lenr-canr.org/acrobat/MosierBosscharacteri.pdf

That paper, submitted on January 15 , was published in April 17, 2009. Suppose my note is submitted tomorrow (June 12); would it be published in the middle of September? That would three weeks before our next conference (ICCF15).

a) The authors remind us that CR-39 detectors are widely used in hot fusion research. Their reference 13 points to a recent review article. After describing previous uses of CR-39 in CMNS research, they describe their own codeposition method. Here is a quote: “In these experiments, a non-hydriding cathode substrate, such as Au, Pt, or Ag wire, is placed in direct contact with a CR-39 detector. The cathode is then placed in a solution of PdCl2 and LiCl in

D2O. When cathodically polarized, Pd metal plates out on the cathode in the presence of deuterium gas. At the end of these experiments, tracks were observed along the entire length of the cathode. The density of tracks was greater than that obtained for the Pd wire experiments indicating that the nanostructures created as the result of Pd/D co-deposition exhibit greater activity than bulk Pd."

b) What follows is a description of control experiments. First they verified that cell components are not contaminated with an alpha-radioactive substance. Their electrolyte consists of PdCl2 and LiCl. They showed that PdCl2 is essential; an experiment conducted without that substance did not produce tracks on the CR-39 detector. That experiment convinced them that tracks are not due to D+ ions reacting with CR-39 material. In some experiments PdCl2 was replaced by CuCl2. No tracks were produced. Track formation rate was reduced by four orders of magnitude when heavy water is replaced with light water. These important observations are then used to argue agains suspected artifacts.

c) I would like to know what the track density was when electrolyte was made from light water. Is it comparable with what was observed by Oriani? In an experiment lasting three days he often observes at least hundred tracks per square centimeter. How as the "four orders of magnitude" estimated from experiments in which counting of tracks was practically impossible, due to overlapping? The same question can be asked the effect of th e mylar filter on the number of recorded tracks. They say that ~90% of particles were stopped in mylar. How was this estomated? I suspect it is just a reasonable guess. It may be 90% and may be 99%. Both numbers show that most particles were stopped.

d) Their electrolytic cell, made from butyrate plastic, has a hole, civered with a thin mylar window. "A silicone-based cenent was used to epoxy that film to the flat cell surface. (I did not know that the word epoxy can be used as a verb. In my limited vocabulary, epoxy is the name of a very strong glue.) Two strong neodymium magnets are used to keep the CR-39 detector very close to the mylar film. A rubber band would probably be eqaually effective. Based on what SPAWAR people published earlier, I will assume that the main purpose of using magnets was to create a magnetic film inside the cell. But that aspect is not emphasized in this paper. In an earlier paper they reported that, with a nickel cathode, tracks were produced only when magnetic field was appled. This is an extremly important discovery--no tracks are formed unless PdCl2, D2O, and magnetic field are present.

e) Etching of chips after electrolysis, and microscopic examinations of pits are described in Section 2.4. In addition to human examination of pits they used an automated scanning track analysis system. In this case decisions about what to count and not to count is made by sophisticated software. A scanning electron microscope was used to observe the surface of gold cathode after electrolysis.

f) Here the description of the electrolysis protocol: "Typically 20–25 mL solution of 0.03 M palladium chloride and 0.3 M lithium chloride in deuterated water is added to the cell. Palladium is then plated out onto the cathode substrate using a charging profile of 100 microA for 24 h, followed by 200 microA for 48 h followed by 500 microA until the palladium has been plated out. This charging profile assures good adherence of the palladium on the electrode substrate. After the palladium has been electrochemically
plated out, the cathodic current is increased to 1 mA for 2 h, 2 mA for 6 h, 5 mA for 24 h, 10 mA for 24 h, 25 mA for 24 h, 50 mA for 24 h, 75 mA for of 0.4 cm2. Given the amount of plating solution placed in the cell, the density of Pd is estimated to be $9*10^{20}$ to $1.1*10^{21}$>/sup> Pd atoms/cm^2.

g) Next section, named "Results and discussion," shows photographs of what was actually observed. Figure 4.1, for example, shows that pits near the cathode no longer overlap as strongly as in the case of experiments conducted without mylar. I have no doubt that pits shown if Figure 6 resemble tracks of nuclear particles. But what kind of particles are they? Looking more carefully, I see that sizes of dark pits in Figure 6, are much larger than sizes of pits in Figure 7. All pits in Figure 7 are due to alpha particles (of several energies). The photo in Figure 6a and all photos in Figure 7, cover an area whose height is 100 microns. Unfortunately, the magnification 500 is specified for the Figure 6a only. Should I assume that pictures in Figure 7 were taken under the same magnification as the picture in Figure 6a? If not then what was the magnification for pictures in Figure 7?

11) Another appeal for a clarification (appended on 6/23/2009)
On June 19, 2009, I posted this message on the Internet list for CMNS researchers

Waiting for clarifications, I would like to reconstruct recent history of experiments said to demonstrate a nuclear effect due to electrolysis. I will begin The Galileo Project (ignoring very important earlier publications). Please correct me if something is wrong.

a) SPAWAR team started using CR-39 detectors in codeposition experiments. The discovered a large number of pits said to be due to nuclear projectiles.

b) The Galileo Project, to confirm presence of reported SPAWAR-type pits, was organized by Steve Krivit.

c) Most participants, including Richard Oriani and myself, independently confirmed presence of such pits.

d) Detailed investigations convinced Oriani, that dominant SPAWAR-type pits are due chemical corrosion, as he reported on this list.

e) Instead of using the codeposition method, invented by SPAWAR team, Richard returned to his old method (using Li_2SO_4 electrolyte based on light water). But he borrowed one element of SPAWAR design; his cathode was put next to CR-39. To minimize a chance of corrosion, Richard protected the CR-39 by a layer of Mylar. The layer's thickness was six microns; this corresponds to the range of alpha particles of about 1.5 MeV.

f) Meanwhile SPAWAR team continued studying pits resembling tracks of nuclear projectiles, and publishing results. Their report was presented at the ACS meeting in 2007. It was followed by a publication in a refereed journal (The European Physics Journal; Applied Physics). That paper can be downloaded from:

http://lenr-canr.org/acrobat/MosierBossuseofcrinp.pdf

g) Ludwik Kowalski, one of several participants in The Galileo Project, also observed SPAWAR type pits. His first report was also published at the ACS 2007 meeting. But he presented evidence that dominant pits could not be attributed to alpha particles, or to less massive projectiles. That interpretation was also published in The European Physics Journal; Applied Physics. The paper can be downloaded from:

http://lenr-canr.org/acrobat/KowalskiLcommentson.pdf

h) Arguments against that interpretation were developed in another paper, published by SPAWAR team:

http://lenr-canr.org/acrobat/MosierBossreplytocom.pdf

i) Another team of CMNS researchers, Francis Tanzella et al., also conducted a SPAWAR type experiment. But, like Oriani, they used Mylar to protect the CR-39 chip, as reported at out Catania Workshop (October 2007). Their protected CR-39 chip was exposed to the cathode for 15 days. Only about 2000 pits were recorded. In other words, at least 99.9% of pits seen without a Mylar filter were eliminated. What remained, however, was much higher than on the control chip. The 2000 pits were identified as protons, with energies close to 2 MeV. That number of pits seems to be consistent with numbers of pits observed by Oriani.

j) SPAWAR team also started protecting their CR-39 detectors with Mylar films of six microns. This was reported in their 2009 paper, downloadable from:

http://lenr-canr.org/acrobat/MosierBosscharacteri.pdf

That report states that a Mylar filter eliminated only about 90% the previously observed pits. Their analysis of pit sizes resulted in the following conclusion: "the majority of the particles formed as a result of Pd/D codeposition are 0.45–0.97 MeV protons, 0.55–1.25 MeV tritons, 1.40–3.15 MeV ^{3}He and/or 1.45–3.30 MeV alphas" No reference to Tanzella's experiment was made in the last publication.

k) Richard Oriani was not successful in trying to publish results of his experiments in a refereed journal. But his earlier results were presented at our Catania workshop (October 2009).

http://pages.csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

l) Ludwik Kowalski was able to confirm reality of clusters of tracks, reported by Oriani. But results were not reproducible on demand. That was also reported at our Catania workshop (October 2009).

m) Motivated by The Galileo Project, Ludwik Kowalski tried to find CMNS researches willing to replicate Oriani-type experiments. This initiative was named The Curie Project. Unable to succeed, he appealed to teachers (on the Phys-L list for physics teachers) and found three participants. Results from the four ongoing studies, including his own, will probably be known before ICCF15.

But the list remained silent, conrary to my expectation. On June 22 I made another appeal, to break the silence. Quoting the above I added:

n) I forgot to include another very important piece of data. Looking at the first two lines of Oriani's first table in:

http://csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

I see that he also performed two SPAWAR-type experiment in which CR-39 were protected from the cathode. The first experiment lasted 168 hours (7 days) and produced 284 tracks/cm^2 on the side facing the cathode and 150 tr/cm^2 on the other side. The second experiment lasted 172 hours (7.2 days) and produced 156 tracks/cm^2 on the side facing the cathode and 160 tr/cm^2 on the other side. As far as I know, Richard was using the same CR-39 from Fukuvi that I am using (background ~13 tr/cm^2).

The total number of tracks he observed was probably close to 750*4=3000, where 4 cm^2 is the total area over which tracks were observed. Like in Tanzella's case, the total number of observer tracks was much higher than the background. And Richard's 3000 number is surprisingly close to 2000 tracks found on Tanzella's Chip #7. Each of these two numbers is probably orders of magnitude smaller than the 10% of the remaining tracks, observed on SPAWAR chips, as described in the most recent SPAWAR paper.

What else should be mentioned in my summary? Is the discrepancy between what was seen on Tanzella's Chip #7, and what was published recently by SPAWAR, real or is it apparent? Please share with us what you know, and what you think.

Appended on 6/25/2009

Why is no one responding to my questions about the apparent discrepancy between the new SPAWAR results (CR-39 was protected with mylar) and results from a similar SRI experiment (F. Tanzella et al) and from two similar experiments of R. Oriani? All participants, including Russian scientists, are on this list. Do authors and coauthors of reports know something that I do not know?

This sudden silence reminds me of another episode of silence. This was in 1940, when scientists decided not to publish results connected with nuclear fission. They realized that a chain reaction could possibly be used to produce a new weapon. They did not want Germans to benefit from research conducted in other countries. I do not think that SPAWAR-type experiments are already yielding practically useful (patentable) results. But who knows; they are in a miltary lab. Perhaps they received an order to stop sharing. But who knows; perhaps this can explain silence.

P.S. How many people on our list know that conspicuous silence about nuclear fission research (in 1940) was noticed by a Russian scientist G. Flyorov? Guessing the reason, he wrote a letter to Stalin and provided a possible explanation. This was in April of 1942, at the early stages of Manhattan project. Soviet governmental preoccupation with nuclear fission was probably triggered by this letter. K. Pietrzak and G. Flyorov, by the way, discovered spontaneous fission of

uranium; this was in 1940. After the war, Flyorov became an academician, and the director of the heavy ion acceleration laboratory in Dubna.

Appended 12/5/2009
1) My article, replying to the reply from SPAWAR, submitted to EPJAP, was rejected during the ICCF15. On October 12 I submitted it to JCMNS (Journal of Condensed Matter Nuclear Science). It is our own peer-reviewd journal. Two days ago I received comments from the referees. Let me show them here, to illustrate that CMNS researchers do not belong to a mutual admiration society.

Referee #1
In reviewing this paper for publication I do not concern myself with the question of the reality of the production of nuclear particles by electrolysis. My sole concern is to judge how well the author has succeeded in invalidating the assertion by the authors of refs. 1,2 and 4 that the etch pits that they observe were caused by alpha particles.

This paper is almost impossible to understand due to the author's manner of writing. The author refers to the measurements by the Spawar group of the etch pits produced by Am-241 alpha particles and those produced by the alleged nuclear reaction during electrolysis. The latter are one-half as large as those produced by 2.2 MeV alphas from Am, and therefore the electrolysis produced pits can not have been caused by 2.5 MeV alphas. In this the author is correct, but those results do not preclude alphas of energies larger than 2.5 MeV, and this is what the Spawar group claims in ref.1.

The author attempts to contradict the Spawar group assertion that the calibration curves of Brun et al. (ref.7) do not apply to Fukuvi CR39 plastic because the curves were established with a different detector plastic. The author does this by pointing to the similarity of the ratios of pits sizes for alphas of two different energies for two kinds of CR39 plastic. However, this is a weak argument because size ratios for alphas of two energies are functions of etching time and conditions.

I conclude that the author has not succeeded in invalidating the Spawar group's contention that alphas of 1.45 - 3.30 MeV are involved, although the Spawar group's assignment may in fact be incorrect. Because the attempted invalidation is the purpose of the paper, this paper should be rejected.

Referee #2
This paper gives unprejudiced critical view on SPAWAR results and it will be very desirable to publish it in JCMNS.

Referee #3
I find Kowalski's paper very difficult to follow. The logic is poor and it is all expressed in a very unscientific way. I do not know what the Spawar tracks are due to. pretty much agree with Kowalski that they most likely are not due to alpha's - but he does not make the case very well - and I don't know who claims that they are. A strong statement such as "cannot possibly be" needs much stronger support. The title MUST be changed. It is not clever - it is insulting and unscientific. All in all I guess the paper does not advance any case and I would be just as happy to not see it in JSCMNS.

Referee #4
I think that some version of the manuscript by Kowalski might be publishable, but that it should not be published in its present form. The SPAWAR group has attributed some of their tracks to low energy alpha particles, based on their interpretation of track diameters in experiments which they have published in several papers. We know from the experiments of Lipson and Roussetski that the primary signals that they see included the dd fusion products, as well as energetic alphas above 10 MeV. If the SPAWAR interpretation is correct, then the energetic particles that are seen are different from those reported by Lipson and Roussetski.

Hence, the interpretation of the tracks attributed to the low energy alpha particles are of interest, and worthy of discussion. In essence, we would like to be sure as to what they are. Kowalski has published on this previously, challenging the interpretation of the tracks as due to alphas, and the SPAWAR group has responded. In the new manuscript of Kowalski, comes back to similar arguments. One thing that is new is the speculation that the tracks are

due to more massive particles than alphas.

I would recommend for publication if some significant changes are made in the manuscript.

[***] The title is inflammatory, and is not supported by the text. It needs to be changed.

[***] The conclusion section is only weakly connected to the paper. Some of the nontechnical discussion should probably be eliminated; or if kept, it should probably be moved to an introductory section.

[***] It would be helpful to change the Conclusion section to a Summary and Conclusion section. In this section, a short summary of the key arguments should be given. This should be followed by the comments regarding the use of a silicon SSB detector. Note that Kowalski's suggestions along these lines are understood by everyone involved, and he is contributing essentially nothing to the discussion. He has apparently not worked with such detectors previously himself, or else he would be less enthusiastic about the noise issues that accompany the use of such detectors.

[***] Kowalski appears to be mistaken that the SPAWAR group has retracted results. As this is inflammatory as written, it should probably be removed. If Kowalski has a point to be made in this regard, it needs to be made in such a way as to not be inflammatory, and stated in a way in which there is general agreement. If these changes are made, then I would support the publication of the paper.

2) I modified the article and sent it to the editor last night, with the following note: “I agree with Referees 1 and 3 that my manuscript was difficult to follow. I rewrote it completely, de-emphasizing the old topic (comparisons of diameters) and emphasizing the new one (results reported in Catania, my reference 6). I also agree that the term ‘cannot possibly be due to ...’ was not appropriate. I am not using it anymore.”

This website contains other cold fusion items.
Click to see the list of links

Physics Teachers Discuss the Energy Problems (April 6 and 7, 2009)

Ludwik Kowalski, Montclair State University, Montclair, NJ

It would be interesting to read these comments, posted on Phys-L (an Internet list), in ten or twenty years. Unfortunately, I will not be around. Like other items on this website, this one is for future generations. Discovery of cold fusion was announced twenty years ago; will cold fusion become a promising option in twenty years? This remains to be seen.

Contribution 1
You wrote: "In 50 years we will probably have used up most of the oil, but then, if nothing else is available, we will switch to coal, of which there is enough to last a few hundred years." Are you sure of that? On a worldwide basis?

When I do the numbers, it looks like if present trends continue within a few decades we will have used up all (not just most) of the world's oil, all of the world's coal, all of the world's natural gas, and all of the world's 235U.

The numbers might /appear/ different if you include only the current US trends, because the US has a disproportionately large share of the world's coal yet is burning it at a disproportionately low rate, preferring (for the moment) to burn imported oil.

To repeat: On a worldwide basis, including coal does not solve the problem, and even including 235U does not solve the problem.

There are only two ways out: renewable energy and/or breeder reactors.

Breeder reactors can get energy from 238U, which is 200 times more abundant than 235U. Also thorium. This could provide energy for a few centuries (not just a few decades). However, almost* nobody wants to propose this as a solution, because it makes the weapons proliferation problem so much worse. Also the track record for breeder reactor safety is not good.

(*) There are folks in India who want to build a thorium-based breeder. Apparently India has indigenous supplies of thorium but not much uranium. The safety and proliferation issues remain.

Contribution 2
This brings up some excellent points. Personally, I see nuclear power as an intermediate step. Not because of technology but because of humans. To convert to a system that is dependent on renewables will require changes in our behaviors and in our ability to accept responsibility for our actions and impacts on the planet -- including population control, the 200 kg gorilla in the room. I don't see that as happening quickly unless there is a crisis. It would also take tremendous political will to move the country in that direction quickly (10 to 20 years). One or two politicians with vision and knowledge and will is not enough. Look what happened to Gore for example.

As for nuclear power, we are in some serious trouble. The power plants that were built in the 60's and 70's were made to last for 30 years. Well, that "warranty" has expired. The metals are getting brittle, the containment buildings are getting weaker, and on and on. Those are also old designs with fewer safety features. Yet, we cannot afford to turn them off as Hugh has pointed out.

I visited the Idaho National Laboratory a few years ago and was quite impressed with the technology of newer designs for nuclear power plants. (The name of this lab is a curious example of public pressure and fear in its own right. The name has changed several times to make it sound more cuddly. Swapping the word "nuclear" for "environmental" and

then dropping that all together.) https://inlportal.inl.gov/portal/server.pt?open=512&objID=255&mode=2

They have developed a system that results in glassified nuclear waste that they claim is very hard to reprocess into weapons material or to leak into the ground water. That said, the technology is new and experience with it is limited.

I'd love to see the shift straight to renewables, but I just don't see that happening. I agree with the closing quote that Hugh uses from Chip Giller. But I just don't see the USA heading in that direction because it requires personal change and thinking long term. (I just saw the movie **"Idiocracy" which is crude and** vulgar in many ways but is disappointingly accurate in even more ways. Maybe that is coloring my thinking right now...)

In the meantime, I'd like to learn more about what INL has developed and see if we have some new options for nuclear power.

Even so, we would still be left with issues like uranium mine tailings. I come from a state (Utah) that has one of the worst mine tailings problem on the planet with the old Atlas mine tailings next to the Colorado River near Moab, Utah. A legacy of our 1950's rush to all things nuclear. So even new technology nuclear plants is at best a stop gap measure. But, as Hugh points out, there are loads of problems with coal too.

Short of a national, in your face, crisis that directly impacts the "average Joe" in obvious and direct ways, I just don't see a direct conversion to renewables.

Contribution 3
One of the biggest asset of nuclear power during the transition period from fossil to renewable is HIGH DENSITY. The current energy infrastructure relies on siting power plants in high population/land use areas. Coal/gas/nuclear power plants can be sited on a couple square miles of land and produce 2000 MW of power. It would take 4000 1.5MW wind generators to AVERAGE that much (with 33% operational efficiencies--with are typical) and you'd still have periods of no wind in the region. It will take decades to build up an infrastructure where HUGE wind/solar farms can produce, STORE, and TRANSPORT our total energy needs. The North-East (Boston to DC) is always going to be a problem area.

One should be a bit skeptical (another thread) of Fusion power at this point since 50+ years of research has yet to yield anything remotely practical. The researchers are always saying "10 more years", but that has been the mantra for the past 30 or so. When my class does their energy project (supplying U.S. energy 100 years from now without the use of oil or gas) we don't allow any 'deus ex machina' solutions. We work with proven technology. It can be done, for 10s of trillions of dollars, and several hundred thousand square miles of land. But nuclear, 'clean coal', and as much hydro as possible are part of the program along with hydrogen as a storage medium and portable fuel. What can be done in the U.S. can be more difficult elsewhere. Japan for example has land problems. They really need some high density energy sources.

Contribution 4
President Obama's Secretary of the Interior announced today that the use of wind energy on the east coast could replace all the coal powered electrical power plants in the US. When asked how many windmills would be required he said "I haven't done the calculation yet". Good grief!

We can't get the Cape Wind project going here in New England because of people like Ted Kennedy opposed to seeing windmills in the distance of the coast of Cape Cod. The promoter of the Cape Wind project estimates that it will take several hundred thousand windmills to match the output of coal fired plants, but that they will accomplish that only 1/3 of the time because of the periods when the wind is not strong enough. The coal plants will still have to be kept on line as a backup. I hope Mr. Obama's economic advisors are more competent than his energy advisors.

Contribution 5
You wrote: “Personally, I see nuclear power as an intermediate step. Not because of technology but because of humans. To convert to a system that is dependent on renewables will require changes in our behaviors and in our ability to accept responsibility for our actions and impacts on the planet -- including population control, the 200 kg

gorilla in the room."

First, nuclear power just isn't in the cards as an "intermediate" step. It takes about 10 years or more to go through the planning, designing, licensing, building and certification process before a nuclear plant can come on line, and we need change *now,* not 10 or 12 years from now (there are plants in other parts of the world that have been 20 years and more in the building and still aren't completed--we don't have that kind of time to fool around with a problematic technology, that may be abandoned before it even can have an effect). Second, I don't see how renewables will require much in behavior change. The goal is primarily to keep the electric power (and other needed energy sources) coming when and where they are needed. I agree that if we try to create a system that requires massive lifestyle changes, the problem becomes dramatically more complicated. The object is to integrate renewables and efficiency into the fabric without making a big deal about it, so that the main thing people will notice it their utilities bills decrease (overall--there may be increases in one sector or another, but the total utilities costs should come down), and the air getting cleaner. If we electrify the railroads and make it economically feasible to shift most long-haul shipping away from trucks and onto the railroads, the costs of maintaining our highway system should decrease by up to 40%, and the roads will become less congested. The net effect should be to make life better and at lease a bit less expensive. This shouldn't require a major change in our behavior, although the increase in freedom these changes bring might encourage us to do even better.

I agree that the population problem is with us still, and will be for at least the next couple of centuries, which makes it all the more important that we move as quickly as possible on energy efficiency, so that all the gains made in how we produce our energy won't be erased by uncontrollable population growth. But the solution to that problem has less to do with how we fix the climate change problem than with the education, freedom and empowerment of women world-wide. It is well established that in those regions where women have control of their own reproductive systems, and have access to family planning information, birth rates decline. I see the largest problem in the population arena is in figuring out how to successfully manage a world whose population is stable. For too long we have based our economic system on an ever-increasing population, to the point where governments faced with a stable or even declining population don't have a clue about how to deal with it, and often panic--either importing labor or enacting policies to encourage increased birth rates.

You also wrote "I don't see that as happening quickly unless there is a crisis. It would also take tremendous political will to move the country in that direction quickly (10 to 20 years). One or two politicians with vision and knowledge and will is not enough. Look what happened to Gore for example."

Well, there is a crisis, and we need to start moving the country in that direction *now,* not in 10 to 20 years. We need to start a massive PR campaign in support of renewable energy--to convince people that wind turbines are beautiful (which I, personally, think they are) and that, combined solar energy, both PV and concentrated thermal can meet our energy needs if we do it intelligently (with the prospect of geothermal and wave energy in the not-too-distant future). That, of course, starts with a smart national grid, which will enable us to efficiently move electrical energy over large distances (perhaps superconducting?) and to use the grid to control how we use our electricity and to enable feed-in from individual sources (rooftop PV, etc.), as well as to eventually enable plug-in electric and hybrid vehicles to serve as our energy storage system to meet high demand situations, when the penetration of such vehicles into the market is enough to support that.

You also wrote "As for nuclear power, we are in some serious trouble. The power plants that were built in the 60's and 70's were made to last for 30 years. Well, that "warranty" has expired. The metals are getting brittle, the containment buildings are getting weaker, and on and on. Those are also old designs with fewer safety features. Yet, we cannot afford to turn them off as Hugh has pointed out."

There is considerable pressure now to re-license many of those aging plants, which are facing deterioration problem that you mention, not to mention the aging of the nuclear engineering and plant operator populations, most of whom will be retired within the next decade or two. And the designs for the new generation of reactors is still very fluid. None has yet been finally approved by the NRC (the Westinghouse AP-1000 has just been sent back to the drawing board for rework, putting the plans for most of the 35 new reactors planned for this country on hold). We cannot afford to turn them off all at once, but relicensing should be done very carefully, and it is likely that an aggressive efficiency

program, including building retrofits as well as new construction, and encouraging the use of solar thermal and solar PV units on homes as well as solar PV on commercial rooftops and parking lots can obviate the need for any new plants, so that the conversion to renewables can be phased with the retirement of exiting coal and nuclear plants as they come to the end of their design lifetimes.

You also wrote “I visited the Idaho National Laboratory a few years ago and was quite impressed with the technology of newer designs for nuclear power plants. (The name of this lab is a curious example of public pressure and fear in its own right. The name has changed several times to make it sound more cuddly. Swapping the word "nuclear" for "environmental" and then dropping that all together.) https://inlportal.inl.gov/portal/server.pt?open=512&objID=255&mode=2

They have developed a system that results in glassified nuclear waste that they claim is very hard to reprocess into weapons material or to leak into the ground water. That said, the technology is new and experience with it is limited.”

And of course that is a problem in itself. We need a storage technique that will be reliable for millennia, and we're talking about techniques with which we have "limited experience." It's clear that, even if by some political accident Yucca Mountain comes back into the picture, we have a long way to go in figuring out how to handle the nuclear waste. Another good reason for not creating any more of it than we are now doing.

You also wrote “ I'd love to see the shift straight to renewables, but I just don't see that happening. I agree with the closing quote that Hugh uses from Chip Giller. But I just don't see the USA heading in that direction because it requires personal change and thinking long term. (I just saw the movie "Idiocracy" which is crude and vulgar in many ways but is disappointingly accurate in even more ways. Maybe that is coloring my thinking right now...)”

There is a lot of inertia in the system. Power companies don't like to get away from the old technologies that they are familiar with, but this is an emergency, and I think that if the government takes the lead in emphasizing the critical nature of the problem that we can start moving in the right direction, but it has to be well planned. We can't build a bunch of wind parks or solar farms without having a robust means of getting that energy to market, and we can't afford to turn the problem over to people who don't have sufficient appreciation of the situation. We need to make sure that we have the capability to provide the base power needs until we can get the renewables well-enough designed and distributed to that they can pick up at least part of the base load requirements as well phase out the coal and nuclear plants and phase in the smart grid technologies that can provide the base load capabilities that will be needed for the long term.

You also wrote “ Even so, we would still be left with issues like uranium mine tailings. I come from a state (Utah) that has one of the worst mine tailings problem on the planet with the old Atlas mine tailings next to the Colorado River near Moab, Utah. A legacy of our 1950's rush to all things nuclear. So even new technology nuclear plants is at best a stop gap measure. But, as Hugh points out, there are loads of problems with coal too.”

Cleaning up the uranium tailings mess is easily comparable but not as dramatic as cleaning up the mess left over from building weapons. And the cost in lives to the miners, mostly Native Americans, on whose lands these predations were committed, is still going on. Cancer death rates among miners and other living near the mines (including children who played in the tailings piles without realizing the danger they placed themselves in) remain elevated and the life expectancy among those people has been measurably reduced by their exposure to the uranium ore that had lain safely underground for eons.

You also wrote “ Short of a national, in your face, crisis that directly impacts the ‘average Joe’ in obvious and direct ways, I just don't see a direct conversion to renewables.”

What the "average Joe" is concerned about, and rightly so, is his or her job and this or her family's future. By any ethical principle, we need to make sure that our efforts in environmental cleanup and GHG reduction end up being an opportunity for those people to better their own and their families' lots. Proceeds from such things as carbon cap and trade sales or carbon taxes or any other forms of revenue raising that goes on in the name of converting our energy sources to renewables, must be used to provide the opportunities to those whose present jobs will be impacted by such

things as closing coal or uranium mines, or operating power plants, or other jobs that have kept them at least existing for decades if not generations. If they can see a future for themselves in what we are trying to do, they will not oppose the efforts and can be enlisted to help in them. A 21st Century "WPA," especially in the current economic downturn, will provide those who are facing a bleak future as our energy economy shifts from coal, uranium and petroleum to cleaner renewable sources with some hope that they will be a part of the new prosperity as it develops.

There are a dozen or more books on the market now that provide prescriptions for achieving a transition to renewable energy within the next 40 years, but my favorite is Arjun Makhijani's "Carbon-Free and Nuclear-Free: A Roadmap for U. S. Energy Policy," published by RDR books. It is available at Amazon, or can be downloaded for free from <http://www.ieer.org/carbonfree/CarbonFreeNuclearFree.pdf>

Much of what I have said in my two posts on this thread is lifted from the pages of that book.

Contribution 6
It's amusing that the "renewable" people are just as "all or nothing" as the "fossil" people. That's why NOTHING is done.

Contribution 7
You wrote “One of the biggest asset of nuclear power during the transition period from fossil to renewable is HIGH DENSITY. The current energy infrastructure relies on siting power plants in high population/land use areas. Coal/gas/nuclear power plants can be sited on a couple square miles of land and produce 2000 MW of power.”

And that has meant that we have not built the smart power grid that we need for the 21st century, whether we do any conversion to renewables or not, and have subjected untold numbers of people to the radiation and pollution inherent in coal and nuclear plants, to say nothing of the dangers in storing nuclear waste on plant sites and coal ash in lakes like the one in TN that collapsed recently.

You also wrote “ It would take 4000 1.5MW wind generators to AVERAGE that much (with 33% operational efficiencies--with are typical) and you'd still have periods of no wind in the region. It will take decades to build up an infrastructure where HUGE wind/solar farms can produce, STORE, and TRANSPORT our total energy needs. The North-East (Boston to DC) is always going to be a problem area.”

I could point out that we once had hundreds of thousands of square miles under cultivation to produce hay for the horses that were our primary means of transportation, and 19th century environmentalists were afraid that cities like New York would be buried under and avalanche of horse manure.

Yes, wind parks and solar farms occupy large chunks of land, but they don't have to be dedicated to that use. Farming and cattle grazing can take place on the same land (under wind turbines at least--among solar panels is a bit more problematic). The wind farms east of San Francisco mix power generation and cattle grazing quite nicely, and have for nearly 40 years. And there is enough open parking area in this country to provide a good chunk of our electricity needs by covering them with solar panels (the parking lot surrounding the Pentagon in Washington, could by conservative estimate produce about 3300 MWh/month on a year-round average). In this country, we have between 10 and 13 parking spaces per car. It's time we used them for something more that leaf catchers during the fall. Offshore wind farms are better looking than offshore oil rigs, and can rely on much more predictable and constant wind sources. And while it is true that the wind doesn't blow all the time, so a windmill can typically produce only about 30% of its rated power on average (I note that both coal plants and nuclear plants operate at only about 30% thermal efficiency, and provide between 60% and 90% of rated power, on average), The wind is blowing *somewhere* all the time, and widely separated wind parks can, when interconnected, produce a combined output that is never below about 25% of rated power. (see: Louis Bergeron and Stephanie Kenitzer, "Study finds that Linked Wind Farms can Result in Reliable Power" <http://news-service.stanford.edu/news/2007/december5/windfarm-120507.html>; also Cristina L. Archer and Mark Z. Jacobson, "Supplying Base Load Power and Reducing Transmission Requirements by Interconnecting Wind Farms," Journal of Applied Meteorology and Climatology, Vol 46, November, 2007, pp. 1701-1717) There is enough potential wind energy off our coasts and in the Great Lakes to power the nation at present rates, and the central plains contain at least that much again.

Meanwhile, both solar PV and Concentrated Solar Thermal power farms are sprouting all over the Southwest, which contains as much potential solar energy as all the wind sources together. In other words, there is more than enough wind and solar energy to meet our needs, and it will occupy only a rather small fraction of our land area, and need not impact on agricultural land. The northeast (Boston to DC) can be powered by offshore wind as soon as we can convince Ted Kennedy to sell his yacht :-).

You also wrote " When my class does their energy project (supplying U.S. energy 100 years from now without the use of oil or gas) we don't allow any 'deus ex machina' solutions. We work with proven technology. It can be done, for 10s of trillions of dollars, and several hundred thousand square miles of land. But nuclear, 'clean coal', and as much hydro as possible are part of the program along with hydrogen as a storage medium and portable fuel. What can be done in the U.S. can be more difficult elsewhere. Japan for example has land problems. They really need some high density energy sources."

"Clean Coal"??? And you don't use "deus ex machina" solutions? If there was ever an oxymoron, "clean coal" is it. We have no idea if we can even capture a significant fraction of the CO2 emitted by a coal plant, or what the cost would be, and no one has a clue if any practical form of storage is or ever will be available for the CO2. It will need to be at least as long-term secure as nuclear waste, and there will be a lot more of it. Such a process is at least 20 years away from any possible practicality, and doesn't begin to deal with the severe environmental problems with every form of coal mining, especially the "mountaintopping" variety that is turning West Virginia into an extension of the great plains, without the grass.

If we can learn how to ship electric power halfway and more across our country, the rest of the world can learn how to ship power internationally (we can too--sending power to Canada and purchasing power from Mexico). North Africa can provide all of Europe's needed electricity, and China could supply Japan. I have seen a computer simulation developed by a team of German engineers that shows how Japan could power itself with renewables successfully. The simulation takes into account time of day outputs and local weather conditions. They show that Japan has the potential to produce at least enough to power its current needs, without taking into account any possible efficiency improvements.

The point here, of course, is that if we keep thinking up reasons why something won't work, we will never solve the climate problem. Creative, but level-headed thinking and planning is needed, here and abroad. My sense right now is that much of the rest of the world is well ahead of us in both.

Contribution 8
You wrote: " President Obama's Secretary of the Interior announced today that the use of wind energy on the east coast could replace all the coal powered electrical power plants in the US. When asked how many windmills would be required he said "I haven't done the calculation yet". Good grief!

Obviously that answer depends on what size wind turbines will be used, how much space is available and how close to shore they can be placed. His statement obviously spoke to the wind potential in that area, and it is certainly true that there is more than enough wind power potential to provide the electrical needs of New England, but how it is to be done is a complex issue with scientific, engineering, aesthetic and political ramifications, as you pointed out, although I think Ted Kennedy can be brought around, and I thought I'd heard that a judge had already overruled Ted's objections and the plans for the Cape Cod wind farm were going forward.

As with everywhere, there are potential problems with offshore wind farms in that area, primarily because of the weather. I don't have a good feel for how large wind turbines (5 MW and up) handle gale force winds or higher, but if they are going to have to be replaced too often due to wind damage, that makes New England offshore electricity less attractive. The same problem exists on the mid-Atlantic coasts due to hurricanes. I doubt anyone has done the calculations yet.

Contribution 9
You wrote: "It's amusing that the "renewable" people are just as "all or nothing" as the "fossil" people. That's why

NOTHING is done."

If my only choice was coal or nuclear, I'd take nuclear. Coal is worse, except for the proliferation problem and the fact that nuclear waste lasts so bloody long and is so dangerous. But the damage coal is doing right now is definitely worse and in the immediate future, nuclear could be tolerated if it was the only choice.

Fortunately, we don't have to make that choice. The renewables are ready now, can be done incrementally so the effects can be seen almost immediately. I don't need all 1000 MW of my wind park installed before I can get the first watt out of it. Same with Solar PV and even some types of CST. Nuclear plants are going to take upwards of 10 years to get anything, and that is just too late. But the renewables do need to be accelerated even more. The sooner the better. If GM wants something to do between trips to Washington to beg for more handouts they could convert some of their idle auto plants to building wind turbines (the engineering is similar--a wind turbine is just a big transmission). Or maybe the folks at Chrysler could get a handout or two if they agreed that that would be a better investment than more cars that won't sell. Also wind turbines don't require specialized training to meet the special safety requirements that nuclear plants do, so the labor force is there essentially now.

Also fortunately, the debate over nuclear is mainly ideological. There will be few if any new nuclear plants built in this country--they're just too expensive. $10-$12B per reactor are the numbers that we are talking about here in the Southeast, where compliant legislatures and utilities commissions are falling all over themselves to approve whatever the local power companies want. But once the bills start coming in, I rather think that most of them will be cancelled, leaving the rate payers holding the bag, while the power company execs take the money and run.

But NOTHING is not what is being done. Wind and solar are building apace, and coal plants are being cancelled all over the place. As soon as the power company folks realize that nuclear isn't sexy any more, they'll quit driving for them, too.

I certainly agree that there is a group of anti-nuclear types who are ready to go back to ox-driven grist mills to avoid the possibility of a nuclear plant. But they are a small, if noisy, minority. I'm sure that there will be a few new nuclear plants started. Whether any of them is ever finished remains to be seen.

Contribution 10
Calculating the "wind resistance" of current ones, I think is an easy engineering exercise. The blades are made to be completely feathered already?

Contribution 11
You also wrote " The point here, of course, is that if we keep thinking up reasons why something won't work, we will never solve the climate problem. Creative, but level-headed thinking and planning is needed, here and abroad. My sense right now is that much of the rest of the world is well ahead of us in both."

It is not a case of thinking up reasons 'why it won't work', but rather trying to be realistic about what it is going to take TO make it work.

OK--'cleaner' coal, if you wish, but with one of the world's largest supplies, coal (as clean as we can make it) is very likely to be part of our near future (next century) energy supplies. With the inevitable phasing out of oil and natural gas, the net environmental problems of coal can be minimized. Together with some nuclear, coal can provide the high-density power source that will be needed in _some_ locations. It too can be phased out, but to do so that big IF about shipping power across country (say North Dakota to New York) has to be solved, a national (international if you wish) grid has to be designed and implemented--(that cost is almost never included in 'renewables' estimates)--or we really would need to develop something like Fusion power.

There are several things to include in discussing using wind and solar for the major fraction of energy needs:

1) As you phase out fossil fuels, you then move many energy demands from 'chemical' to electrical. Heating and transportation energy must now come from the renewables. Biomass could only handle a fraction of the transportation

fuel needs--we just calculated (for our project) that 3% of the energy needs in 2100 in the form of biodiessel (from soybeans) and ethanol (from switchgrass) would require 150,000 square miles of land use. If we can back off of oil soon enough, then perhaps some specialized needs--big rig trucks and aviation might still draw from remaining petroleum reserves. The big point here is that the electrical demand could triple without oil and natural gas, and then if you want to eliminate coal, the numbers become daunting.

2) The population (U.S.) is going to continue to increase. Can we hold the population down to say 450 million by 2100? That will take some aggressive work in immigration policy. If the country keeps adding a million or more immigrants a year, immigrants with traditionally higher fertility rates than the 'native' population, and generally more religious attitudes against birth control, 450 million would be a very low estimate. So lets assume at least a 50% increase in population.

3) Efficiency and conservation can certainly lower energy needs. 25% proves difficult (for my classes) to fully quantify, but that should be possible and maybe a bit better. However, with the population growth it means the overall energy demand will increase and with the reduction or elimination of most 'chemical' energy sources, the demand for electricity rises sharply.

4) So what's a 'reasonable' estimate of yearly energy demand in 2100? We work with 20,000 TWh or a power demand of 2.3 TW. What do such huge numbers really mean? IF--you wanted to run the country on wind (assuming you had a grid and had generators spread so that you could guarantee the 25% availability at all times (according the Hugh), then using 1.5MW generators (pretty much the standard although bigger ones are available) you need over 6 MILLION wind generators. In a more realistic system, without any fossil fuel, you might split up the demand, but you can't get much more than 3% from Hydro (and the environmentalists want to dynamite all the dams anyway), maybe 3% from geothermal, might push 10-12% from biomass, but then you have to split the rest--over 80% between wind and solar. Without coal and nuclear--this is huge.

5) Effective use of solar (and to a lesser extent wind) will most likely require storage techniques--maybe electrolysis to hydrogen--to assure an 'energy on demand' network. Whatever the storage and distribution system, it will cost some big bucks. We've estimated about $5 trillion for a hydrogen capable pipeline distribution system.

6) Cost estimates have to account for the likely shorter lifetimes of wind generators and some forms of solar (PV panels) compared to fossil/nuclear plants (about 75 years) and dams (100 years or more). We don't have enough experience to know the replacement rate for large wind generators, but it is likely to be at least twice that of current plants--this increases the cost above those normally estimated for the switch to renewables. Again, such estimates are just to be somewhat realistic about the task--not to dismiss it.

****

To repeat what we do in my energy class--a MODEL of energy distribution for 100 years in the future (one that still uses coal and nuclear but is heavy on wind and solar) costs out at $30-50 trillion--capital costs-- and uses about half a million square miles (much for biomass) of land. What these numbers say, to me, is that this is not a 10 year, 20 year, or barely a 50 year project. To be affordable, the transition must be stretched over a century time frame. However, clearly we need to start now (we are starting now) and will have to ramp up the transitions, but thinking we can eliminate the fossil fuels in a couple decades is, IMO, delusional.

Contribution 12

You wrote “I disagree with your analysis. By not charging the rate-payers for the external costs, you reduce the incentive to conserve. Thereby driving the cost up for everyone. Here in southern California we have tiered rates for residential electric consumption to encourage conservation.”

US energy policies have led to profligate consumption of energy. We have about 5% of the world's population, yet we consume 25% of the world's energy resources.

I get really tired of this one as well--(and the percentage is falling quickly as China rapidly moving to become king of the energy use hill). Our energy use per person is about the same as Canada and Australia. Not too far above

Germany. Big, developed nations, use energy. Its why they are developed. The larger the physical size of the country, the more energy necessary to move people and goods. The more industry that resides within the country, and the more that raw materials are mined, processed, and used within a country, the higher the energy use. Switzerland seems to use no energy--but it lives off the energy burned in France, Germany and Italy. You also have population density and (more to your point) life-style differences. If all Americans would put their family of four into a 1000 square foot apartment, with that apartment stacked on top of and surrounded on all sides by other such apartments--sure the energy usage would drop. Yes, we can use less energy--but I repeat that reducing by more than 25% is tough. As a country of 300+ million people, stretching 3000 miles coast to coast and 1000 miles border to border, with (still) the biggest economy in the world and a 21st century technology, the U.S. does not do all that bad. The problem is really not that the U.S. uses too much energy (OK, a little too much), it is that the rest of the world uses too little to provide a 21st century living standard to their people. The comparison to be made is between the developed world and the 'third world'--average out the variations for population, area, population density, percentage of 'home grown' economy versus imported resources and then do your comparisons. Spare us, please, from the 5%--25% mantra. It really is not useful.

Contribution 13
Here in the U.S. we use 67% more energy per person than the average of the top 18 developed countries.

Canada comes close, but most developed countries use much less energy per person than we do.

http://www.nationmaster.com/graph/ene_usa_per_per-energy-usage-per-person

China and India are using more energy as they develop, but don't come close to any of the developed countries in energy use per person.

Contribution 14
You wrote: “Calculating the "wind resistance" of current ones, I think is an easy engineering exercise. The blades are made to be completely feathered already?”

It isn't the steady winds that I worry about. A feathered blade can handle a substantial wind speed. I've flown around in airplanes with a propeller or two feathered at speeds up to 250 knots with not problems. What I don't know is how well a turbine blade that may be as much as 30 meters long would handle gust loading that could easily be non-constant over that dimension, if the hub structure could be damaged by violent slewing as the wind gusts changed direction quickly.

I also worry about how well the foundation structure would withstand the storm surge. I heard of massive oil rigs being destroyed during some of the really bad storms in the North Sea, and I don't think a hurricane would be any easier on a wind turbine tower.

I'm sure these can be handled by competent engineers, but at some point the additional cost of making them strong enough to withstand a Category 4 or 5 hurricane may run the cost up to the "not worth the effort" level.

Offshore wind turbines are already significantly more expensive than on-shore ones, and having to replace them before their design lifetime due to wind damage doesn't make them any easier a sell.

Contribution 15
It's nice to see a reasonable approach championed. Alternate energy sources are "alternate" because they are not quite ready for prime time. We have an absurdly large supply of ridiculously cheap coal (sorry - I'm not swayed by arguments of "true" cost). It is national economic suicide not to use this supply. Clean it up - do whatever will lessen its environmental impact - but use it. Many "alternate" sources can be refined to the point where they will eventually make economic sense. Wind power NEVER will. No one wants wind farms near them. They may look wonderful to an environmental activist, but to most sane people they are just plain ugly. When even the East coast liberals won't live next to them you know you have a problem. When I fly to California every couple of years and look at the windmills outside of Vacaville blighting those beautiful California hills I get angry enough to almost start shaking. What idiots

would do that to such magnificent countryside?

Centralized power from nuclear and coal feeding in to grids leaves the rest of the land available for our use and enjoyment. Solar and wind sources in diffuse arrays that are everywhere in sight are too big a price to pay for a supposed couple of degree warming from models that can't even hindcast.

Contribution 16
I haven't read every post on this thread, but there are two potential energy sources that seem to always be overlooked. One is OTEC (Ocean Thermal Energy Conversion); energy produced by using the temperature differential in the deep oceans, and the other is tidal (think underwater "windmills"). One of the biggest problems with energy generation/usage is transmission and storage, and these impact greatly on OTEC.

Contribution 17
You wrote “The problem is really not that the U.S. uses too much energy (OK, a little too much), it is that the rest of the world uses too little to provide a 21st century living standard to their people.“ That, of course, begs the question "What is the right amount of energy use?" and the related question "Where will we get that energy?"

I would propose that the answer to the first question is "As much as we can sustainably (100+ yr time-scales) produce." The goal SHOULD be to raise up the per capita use for those who do not enjoy the benefits of energy use, not to cut back those who benefit from the advantages of energy use.

Of course, "sustainable" must also include the ecological effects - pollution, global warming, destruction of habitat, etc. It could even include aesthetic factors (like power lines or pristine wilderness or skylines) or political factors (like handling of nuclear waste or concerns over weapon-grade nuclear fuels).

Unfortunately, I don't see any real answers that don't involve some rather drastic choices - most of which come down to population! From what I can figure out at current rates:

* We do not have 100+ yr reserves of fossil fuels (and there are global warming concerns) * We do not have 100+ yr reserves of uranium (and there are disposal concerns) * We do not have 100+ yr reserves of copper (to expand distribution and to build several 100,000 generators for wind turbines) * We do not have enough land to grow food AND biofuels to replace petroleum. (And even current agricultural practices are not sustainable in many areas).

Solar holds promise it seems (it requires vast land, but the sun should keep shining for well beyond 100 years!), as do geothermal and energy from the oceans.

It looks like we need to (or will be forced to). * cut back on energy use * expand the few truly sustainable sources that can deliver huge quantities of energy * find completely new sources (fusion???) * decrease the world population. Am I missing something?

Contribution 18
You asked “Am I missing something?” Yes, choice (e) all of the above!

Contribution 19
You wrote “ Wind power NEVER will. No one wants wind farms near them. They may look wonderful to an environmental activist, but to most sane people they are just plain ugly.”

For what it's worth, I actually think that windmills look incredibly cool. That said, it will take several states worth of land to get enough solar and wind energy to start replacing many coal, natural gas, and nuclear power plants.

However, we already blot the views of several states with farms. The Central Valley of California should be a lovely desert but it is filled with ugly canals and hideous fields of food, looking totally unnatural. Don't think I'm crazy that

most farms look terrible. Most do not look like calendar pictures or gardens, especially after harvest. They smell bad too, not even counting the livestock.

Why do people put up with it? Well, we like to eat what we like to eat.

And I love my computer and the electricity that runs it.

Contribution 20
Wassa matta? You think the California hill sides are somehow better or more deserving than the W Virginia mountains? Or... maybe you just think the California *people* are better or more deserving.

Quite aside from environmental concerns or what is done to "magnificent countryside," I seriously doubt, for example, just for starters, that any California family's cemetery has been dug up, moved, and then lost, by a stupid windmill company.

Don't get me wrong, I'm all for using the coal as well, but... gimme a break. At least find a different point to argue.

Contribution 21
You wrote “What we can do is find ways for people around the world to generate enough energy to improve their standard of living ...” Can we indeed do this??? I certainly hope so, but it will take some MAJOR changes.

Planetary-scale energy needs will only be met by "harvesting" energy
from planetary-scale sources:
* energy from the earth (geothermal)
* energy from the sun (including wind and biofuels)
* energy from the moon (tides).
* energy from atoms (fusion - not fission because we don't seem to have
enough U)

Until we start thinking on these scales, we are thinking too small! Furthermore, these are mostly diffuse sources, so storage & transportation on a global scale become an issue too. If we do not figure out how to (or develop the will to) harvest these sources on these scales, civilization will not continue as it is.

Contribution 22
Just because people have had to suffer because of property rights abuses in one region is no reason to continue the practice and spread the abuse to other areas. Whenever people justify stomping on individual rights because of the "common good" (read mineral rights) there will be wholesale abuse.

I don't want to look at windmills while enjoying the beauty of Martha's Vineyard, and I doubt if West Virginians enjoy seeing the tops of their mountains sliced off to expose coal. I totally agree with your point that coal production has produced much greater ugliness than windmills have so far. We have the technology to stop these desecrations. Oil companies have figured out ways to let one platform gather oil from many sources miles away.

The remedy to the coal abuses is to have West Virginians and the state of West Virginia sue those companies that lower people's quality of life - and to keep suing until until it becomes more profitable for coal companies to take more responsible approaches. Courts need to review the whole concept of mineral rights in a country that is no longer predominantly wilderness. Northeastern states have taken such an approach with Midwestern power plants that spewed pollutants that were carried to them by the wind.

Environmentally friendly coal production will cost more - but we can't turn our back on such an abundant source of energy. Neither can we just ignore the abundant oil off our coasts. Coal and oil make more sense than seeing windmills everywhere one looks.

Contribution 23

Question 1 -- what percentage of energy supply is domestic?

Question 2 -- what percentage of our GDP is production and what percentage is consumption?

Contribution 24
For answers and more data, please see my PowerPoint I use in my energy course...
http://www.phy.ilstu.edu/~marx/phy207/High_Energy_Society.ppt

Contribution 25
Here's a sad but interesting story. A few years ago we came pretty close to getting a 3MW Vestas wind turbine for Bluffton University. I was one of the persons pushing for this, and I was appointed to the planning committee. We did a preliminary study and found we had a good site with reasonable wind. We found a group of investors willing to loan the whole amount as a capital loan at a reasonable interest rate.

Our regular source of electricity is American Electric Power (AEP), which in our area is 90% coal and 10% nuclear, and our rate is about $0.07/kWhr. We had two options for using the electricity from the turbine...

(Option 1) We could sell it to AEP for their generation cost of about ($0.02/kWhr) and then buy it back at $0.07/kWhr, thus saving $0.02/kWhr. We estimated a 10-year payback period doing it this way. This would be easiest to implement because we wouldn't need a substation/switch to use AEP as campus backup when the turbine isn't enough.

(Option 2) Install a substation/switch that would allow us to use the electricity directly (saving $0.07/kWhr on what we use, and selling any excess to AEP at $0.02/kWhr), and also be able to supplement the turbine from AEP (at $0.07/kWhr) when the wind was too low or the turbine was down. This was more difficult for determining the payback period because it's difficult to determine how often we would have electricity to sell to AEP as opposed to needing to supplement the turbine by purchasing electricity from AEP. Our estimate for this was about a 3 to 6-year payback.

Either option seemed like a no-brainer, we had a lot of community support, and I thought we were going to do it. But the best site on campus was west-southwest of the intercollegiate soccer field and baseball field. The athletic department reasoned that late afternoon sun coming through the turbine blades would cast flickering shadows on the playing fields during games. That killed the program. The fear that an athletic event would occasionally get flicking shadows during a game killed what would have been an excellent investment in alternative electric energy for the university.

Since that time we have had a change in the administration, and I am trying to rekindle interest in a turbine, but movement is slow.

This is just one example that shows how difficult it can be to use "alternative energy" even in situations for which the economics looks quite favorable.

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372) Useful references for users of CR-39 chips

Ludwik Kowalski
Montclair State University, Montclair, NJ, 07055
April 13, 2009

1) g) Energy distributions of alpha particles from Am-241, after they traverse mylar filters of several thicknesses, are reported (by Jan Jakubek) in

http://aladdin.utef.cvut.cz/ofat/Methods/HeavyParticles/index.htm

Figure 1, extracted from this reference, shows, for example, that after traversing 24 microns of mylar the mean energy is still 2.2 MeV. The distribution of energy is nearly gaussian. Jakubek's Figure 2 should also be very useful.

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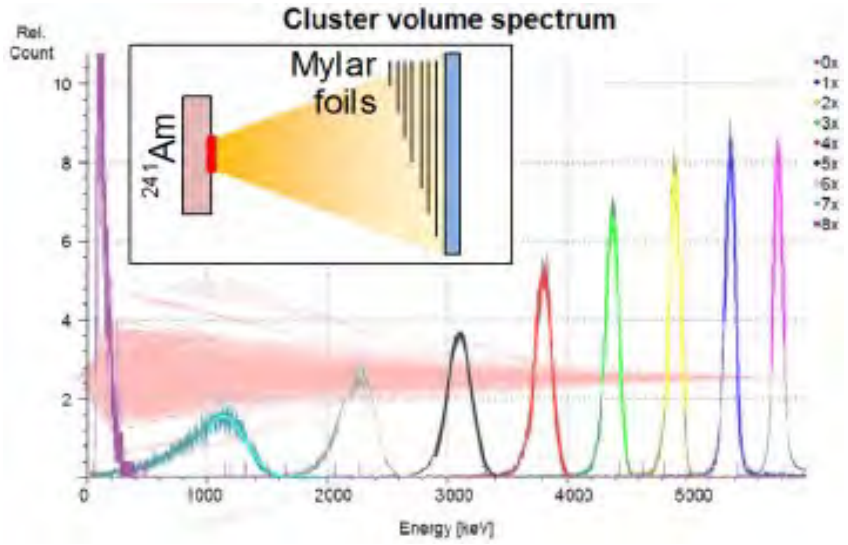

Figure 1

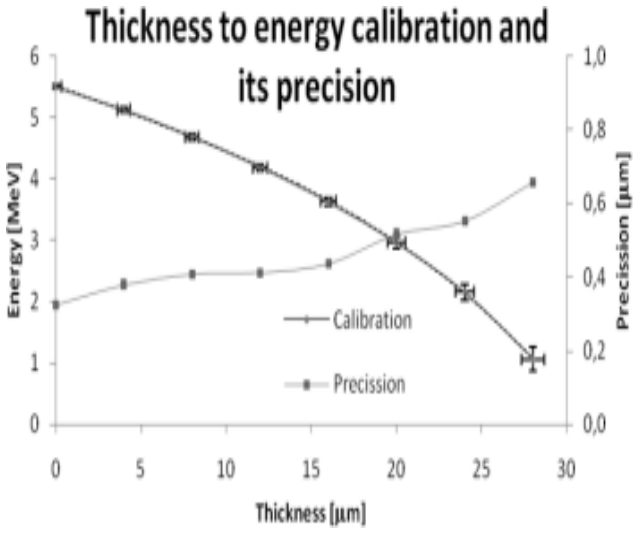

Figure 2

******* Mylar films can be purchased at a company called "spi supplies" *******

P.O. Box 656
West Chester, PA 19381-0656
USA

tel: (800)-2424-SPI or (610)-436-5400
email: spi3spi@2spi.com

It is called XFR Thin Mylar Support Films (for X-ray spectroscopy)

A spool of 6 microns (3 “ wide and 92 meters long) costs $73

********** They also have 3.6 and 2.5 micron mylar **********

2) Those of us who work with CR-39 detectors should not forget about a powerful particle identification toll developed by Russian researchers. SPAWAR team is using this tool by observing how diameters of pits grow when the etching time is increased. A detail description is in two papers that A. Lipson et al. published, in the proceedings of our Catania conference (pages 163 and 182). I used their data to construct turves in Figure 3. Hopefully, this will be useful to some researchers.

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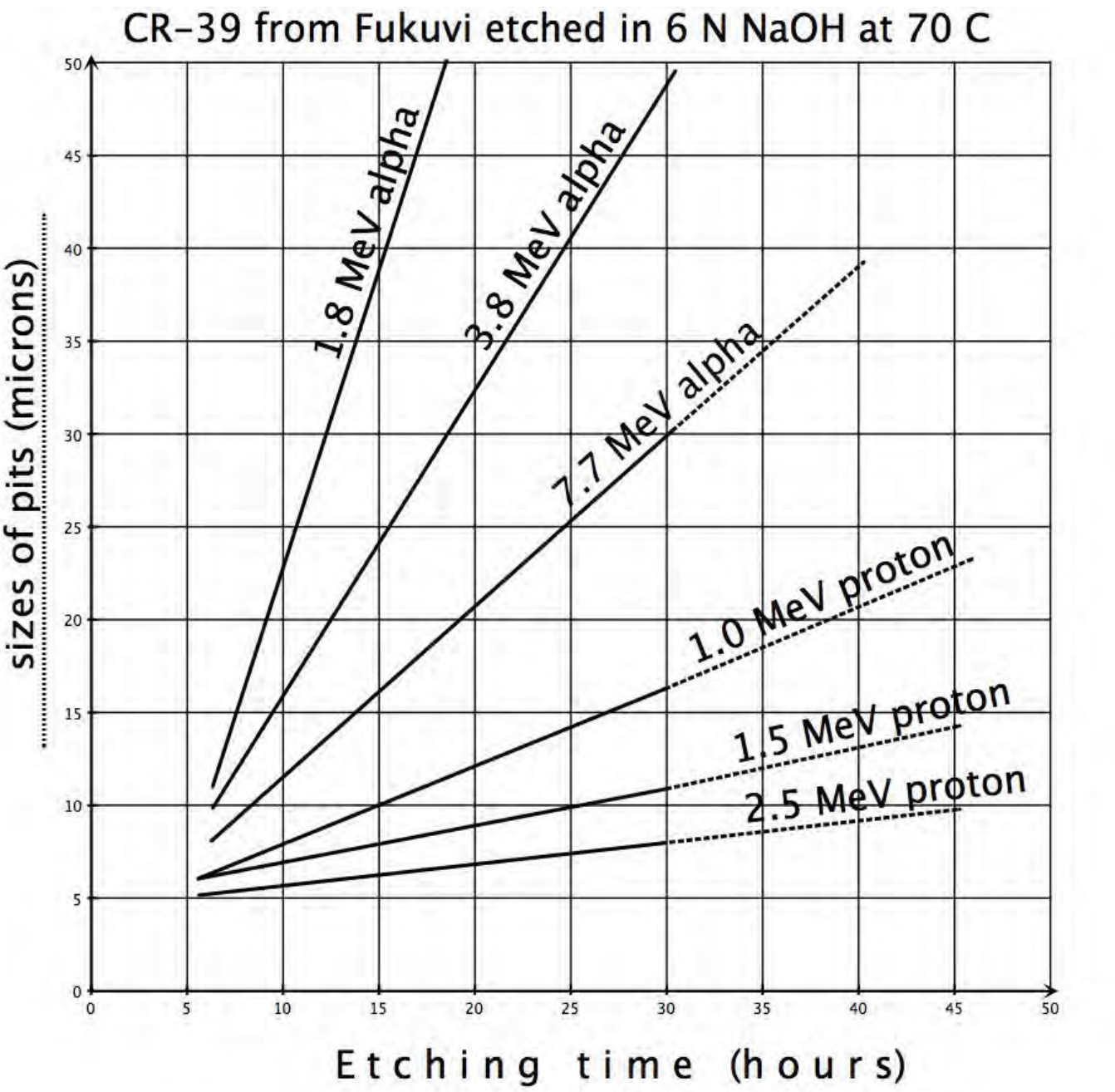

Figure 3

3) Someone wrote that alpha particle pits might have different sizes on CR-39 chips from different manufactirers. I have no experience with chips from manufactures other than Fukuvi. A little corner of each of my chip is always irradiated with alpha particles. And I always look at this corner, to make sure etching was succesful. A big change in size, for example by a factor of 1.5 would probably be noticed. I do not recall any big changes in sizes. Replying to the posted message I wrote:

“One thing that might produce Am-241 pits of different sizes on CR-39 from the same manufacturer (and from the same batch) are different average etching temperatures. I also suspect that an etching solution looses "potency" in time. My etching solutions are always stirred, to make sure the temperature is uniformly distributed within the beaker on the hot plate. I am sure you do the same , or something equivalent. ..... By the way, I suspect that an etching solution looses "potency" in time. My etching solutions are always stirred, to make sure the temperature is uniformly distributed within the beaker on the hot plate.”

4) Another CMNS researcher responded: “ It is correct that an etching solution can lose potency over time [especially if kept in an open beaker] many days. When NaOH etchant is exposed to air, carbon dioxide from the air reacts with NaOH to make sodium bicarbonate NaHCO3. As this reaction proceeds the concentration of NaOH falls and the etchant becomes weaker. Holding other parameters such as time, temperature, and stirring constant the pit size that is reached in an experimental measurement becomes smaller. If one wishes to establish a reproducible correlation between particle energy and pit size, full strength etchant must be used for each measurement.”

5) Range energy curves,for example for alpha particles, seem to be very different for different materials, such as air, polyethylene, aluminum, or iron. But this is true only when ranges are expressed in units of distance. Express ranges in terms of mg/cm^2 and curves become very similar, especially when atomic numbers (Z) are not very different. This is illustrated below.

```
10 MeV alphas in gold: R=45.5 mg/cm^2
6 MeV alphas in gold: R=22.8
5 MeV alphas in gold: R=18.0
4 MeV alphas in gold: R=13.6
3 MeV alphas in gold: R=9.6

10 MeV alpha in nickel: R=22.6 mg/cm^2
6 MeV alphas in nickel: R=10.9
5 MeV alphas in nickel: R=8.5
4 MeV alphas in nickel: R=6.4
3 MeV alphas in nickel: R=4.5

10 MeV alphas in aluminum: R=16.5 mg/cm^2
6 MeV alphas in aluminum: R=7.5
5 MeV alphas in aluminum: R=5.8
4 MeV alphas in aluminum: R=4.2
3 MeV alphas in aluminum: R=2.9

10 MeV alphas in carbon: R=12.4 mg/cm^2
6 MeV alphas in carbon: R=5.4
5 MeV alphas in carbon: R=4.1
4 MeV alphas in carbon: R=2.9
3 MeV alphas in carbon: R=1.9

10 MeV alphas in hydrogen: R=4.19 mg/cm^2
6 MeV alphas in hydrogen: R=1.70
5 MeV alphas in hydrogen: R=1.24
4 MeV alphas in hydrogen: R=0.85
3 MeV alphas in hydrogen: R=0.53
```

A range-energy distance for aluminum, for example, can be used to predict how the energy changes as a function distance in mylar or iron, provided high accuracy is not needed.

To covert cm into mg/cm^2 one must know the density of the material. The density of aluminum is 2.7 g/cm^3. That means that 1 cm of aluminum translates into one gram per square centimeter, or 1000 mg/cm^2. The density of mylar is 1.39 g/cm^3. That means that 6 microns of mylar translates into 0.83 mg/cm^2. A thickness of a thin foil (actually it is its surface density) can be calculated my dividing its measured mass by its measured area. This is often more

accurate than using a mechanical micrometer.

6) How does the size of a pit, due to an alpha particle, depends on the etching temperature of the 6.5 N NaOH electrolyte? My alpha particles were from an Am-241 source removed from a smoke detector. I do not know what the energy was, after particles traversed a layer of something that prevents contamination. It is probably 3 MeV or 4 MeV. To make my data useful at any energy (between ~3 MeV and ~ 8 MeV), diameters of pits are expressed in arbitrary units. The diameter at 60 C is 1 unit, be definition, after 12 hours of etching.

temp=60 C ---> diameter=1.0 units after 12 hours
temp=65 C ---> diameter=1.8 units after 12 hours
temp=70 C ---> diameter=3.1 units after 12 hours
temp=75 C ---> diameter=4.3 units after 12 hours

7) It would be interesting to know (in light of what was stated in point 4 above) what the effect of potency (concentration) of the etching solution is. Perhaps I will investigate this, if necessary. But here is a bit of useful information. One of the researchers who replicated a SPAWAR-type co-deposition experiment, under The Galileo Project, was Winthrop Williams. His CR-39 calibration chip, exposed to alpha particles of 5.45 MeV, was examined by Russian Scientists: A. Lipson, A. Roussetski and E. Saunin. I am reading their 2007 report, in Proceedings of our Catania workshop (page 163).

Williams' chip was etched in NaOH solution of 6.5 N (for 3 hours at 68 C). Russian scientists also irradiated their CR-39 chip with alpha particles of 5.45 MeV. But their etching conditions were different (6 N NaOH, for 7 hours, at 70 C). In both cases CR-39 was from Fukuvi. The pits on the Russian chip were found to be larger than pits on the American chip. That is not surprizing, considering a large etching times difference. What is surprizing is that less than 2 hours was needed to compensate the etching times difference of 4 hours. By the way, Russian scientists did not study the effect of concentration of the NaOH sulution; additional etching for 1.75 hours was necessary to start sequential etching when mean sizes of pits, on chips to be compared, were identical.

To study the effect of the NaOH concentration, one can perform the following experiment. Two Fukuvi chips are exposed to alpha particles of the same energy. Then they are etched in the same oven, for example at 70C, for exactly the same time, in two freshly-made etching solutions, 6.0 N and 6.5 N. I have no doubt that pits etched in the more concentrated solution will be larger. But I have no idea by how much.

8) Referring to 4, one CMNS researcher wrote: "You should be using fresh NaOH solution for each etch. We found close-fitting test tubes that enabled us to use only a few mL of fresh etchant for each CR-39 chip." They probably have an oven with a thermostat, or perhaps tubes containing the etching solution are immersed into a beaker with water, which is standing on a hot plate.

9) I wrote: "Another possibility is to accumulate several CR-39 chips (keeping them in salty water, rather than in air) between the end of electrolysis and etching. That what I am going to do this summer. Following Oriani's protocol, I always drill a little hole in each chip and suspend chips in the stirred etching solution. That would really be essential if the goal were to compare sizes of pits on different chips.

In order to compare experimental results from different labs (I am still hoping for phase 2 of The Galileo Project), the etching temperatures must be specified to within 1 degree or so. The hot plate I am using does not allow me to do this. How precise are typical ovens in chemistry labs? Etching for 12 hours at 70 C would produce much larger alpha pits than etching at 65 C (see unit 6 above).

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373) The Curie Project

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June 22 , 2009

1) Introduction

History of my CR-39 cooperation with Richard Oriani is summarized in unit 369. The beginning of The Curie Project is described in unit 368. At the end of this unit I plan to show results from that cooperative project; I hope they will be available in September, before ICCF15 (15th International Conference on Cold Fusion in Rome, Italy).

INSERTED ON JULY 17, 2009

As indicated in the title, this unit was created for reporting anticipated experimental results. But I changed my mind; results will be shown in a later unit. What follows are selected messages posted on the private Internet list for CMNS researchers.

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2) Another appeal to CMNS researchers

Here is a message I posted (6/24/2009):

‘1) As I wrote earlier, The Curie Project is in progress. But it has only three participants (from the Phys-L list for physics teachers), plus myself. We are using Oriani's protocol; it is described in his ICCF14 contribution.

http://pages.csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

The only two departures from the protocol are:

a) Keeping chips in salty water, when they are waiting for electrolysis and when they are waiting to be etched. Richard wrapped them in Al and kept them is air.

b) The rear side of a chip, during electrolysis, is protected from being in contact with air (by a ~0.5 mm layer of polyethylene). Richard did not protect the rear surface from air. The project desperately needs more participants; it is not too late to join us.

2) Here is a summary of Oriani's results; this does not include nine recent experiments. These experiments will be counted as his contribution to The Curie Project. Richard is using the same CR-39 I am using (we bought one Fukuvi sheet together, two years ago). Not surprisingly, his mean background, measured 13 times, is nearly the same as my background, measured 6 times (so far). His mean background was 13.6 tr/cm^2; his standard deviation was 6.8 tr/cm^2 (as shown in Table I). My mean background was 13 and my standard deviation was 5.

a) Richard’s post-electrolysis chips had tracks on both surfaces. His Table II shows results from 22 experiments (#4, #5, #6, ...#25). As you can see, results differ very significantly from one experiment to another. But 40 out of 44 surfaces had track densities higher than 16 tr/cm^2. In other words the number of post-electrolysis tracks exceeded the

number of background tracks in 90% of cases. That is remarkable.

b) Let me summarize Oriani's results in terms of mean values and standard deviations.

Front surface (facing the electrolyte) mean=142; st. dev.=146
Rear surface (facing away from the electrolyte) mean=132; st.dev.=148

The lowest density was 9 and the highest density was 498 tr/cm^2. In one case the number of tracks was too high to count; I tentatively turned this into 498. The two distributions are very wide. But the mean track density on each side of the post-electrolysis chip, is about ten times higher than the mean density on control chips. This includes the four cases in which the densities were only 9, 9, 11 and 16. The distribution of track densities, on 44 surfaces of post-electrolysis CR-39 chips, is shown in Figure 1 below.

Figure 1 (373histogr.png)

Why is the histogram so strongly skewed? I am not at all comfortable with the fact that the overall mean, 147, and the same as the standard deviation. Yes, 147 exceeds the background by one order of magnitude. But the level of confidence that this would be confirmed in another set of 22 experiments is very low. I was trained to believe that, in order to be taken seriously, a difference between a signal and a background should be at least as large as the sum of standard deviations (of the signal and the noise), preferably two or three times larger. On the other hand the above curve shows that densities exceeding the background are not at all rare. Is there a better way to evaluate a level of confidence than standard deviations? '

3) Categorical data

Suppose Richard's data are used to answer simple "yes" or "no" questions. The questions would be:

Is it true that track densities are higher than mean background?
Is it true that track densities are higher than 69 tracks/cm^2?
Is it true that track densities are higher than 140 tracks/cm^2?

Richard asked the first question 44 times and the answer was "yes, it is true" was obtained 40 times. On that basis one would be able to say that f=40/44=0.91 represents the expected probability that the statement is valid. In other words, 91% of CR-39 surfaces, in another sequence of identical 22 experiments, are expected to validate the statement (that the signal is higher than the background).

This kind of predictions is common. Here is a typical illustration. Suppose a random sample of n=10 people was selected in a town. Six of them speak Spanish. In this case the answer is either "yes" or "no." A fraction f=6/10=0.6 is calculated on the basis of this small sample. How reliable is the statement that "about 60% of people, in this town, speak Spanish? " The answer depends on the value of f, and on the size of the sample. The distribution of f, from many samples of size n, is expected to be nearly Gaussian, when n>20 (actually, a it is already nearly Gaussian at n=10). The width of the distribution of fractions (actually the standard deviation of f) is expected to be equal to sqrt [f*(1-f) / n].

The uncertainty about the expected 91% reproducibility, in subsequent sets of similar 22 experiments, the standard deviation s=sqrt [f*(1-f) / n] = sqrt [0.91*0.09/44]=0.043. Subsequent sets of experiments are expected to produce values of f that are between 0.91-0.043=0.877 and 0.91+0.043=0.953 (two standard deviations). The theoretical reproducibility of nearly 91%, based on real experimental results from one set of 22 experiments, is a rather convincing argument for the claim. That "more than background" claim, however, is not very strong.

So let us make a stronger claim, for example "more than 69 tr/cm^2, as above." Richard reported 25 such outcomes out of 44 measurements (f=25/44=0.57). Thus only 57% of sets of replications are expected to support this new claim. The predicted reproducibility for evan stronger claim, "more than 140 tr/cm^2" is considerably lower than 57%. All this is intuitively obvious; stronger statements, based on the original set, are less likely to be supported (by results from

subsequent sets of 44 measurements) than weaker statements. But thinking in terms of standard deviations of fractions might be helpful. I am learning this from an elementary statistics textbook. According to the "central limit theorem," the distribution of mean values, from several sets of experiments, is Gaussian, even when distributions from individual sets are not bell-shaped.'

4) Ludwik's message posted on CMNS list (6/28/2009)

"The attached histogram [see Figure 1 above] is based on Richard's Table II in

http://pages.csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

It has 11 bins and bins are populated with the following numbers of observations:

14, 10,, 6, 3, 3, 1, 1, 1, 1, 1, 1

The purpose of The Curie Project is to verify Richard's results. He examined 42 surfaces and measured track densities on them. Suppose several people also measure track densities on 42 surfaces, from their own sets of experiments. The histograms would be quite different. This is intuitively obvious, considering small numbers of observations (see above). I verified this by simulating replications with the Monte Carlo method. To accomplish this I simply assumed that the true distribution (one that would be obtained if thousands of surfaces were examined) is the same as Richard's distribution. Below are results from my first five simulations. Each line is a set of numbers to plot one histogram (and to calculate its mean value). The last numbers, after the asterisk, are mean track densities for the five histograms.

19, 7, 8, 8, 0, 0, 0, 0, 0, 0, 0 * 106
14, 16, 2, 3, 4, 3, 0, 0, 0, 0, 0 * 121
9, 11, 7, 7, 4, 0, 0, 0, 3, 0, 1 * 164
21, 7, 0, 8, 3, 0, 0, 3, 0, 0, 0 * 126
11, 8, 6, 0, 2, 0, 4, 0, 0, 1, 3 * 187

The distribution of mean density, from 1000 simulated histograms turned out to be bell-shaped; the mean of means was 154.5 tr/cm^2; the standard deviation was 18 tr/cm^2. Individual histograms are strongly skewed but, according to the Central Limit Theorem, the distribution of the mean of means should be Gaussian. My Monte Carlo simulations confirmed this. And they provided a qualitative definition of "success," for The Curie Project.

The purpose of that project is NOT to replicate the shape of Richard's histogram; it IS to show that the mean track density, on post- electrolysis chips, is not very different from his value of ~140 tr/ cm^2. What does it mean "not very different" ? The difference should not be larger than two standard deviations (2*18=36). In other words, any mean density, between 140-36=104 tr/cm^2 and 140+36=176 tr/cm^2, would be consistent with Richard's result, provided tracks on many surfaces are counted in The Curie Project. That is why additional participants are needed. It is not too late to become a participant. Comments and suggestions will be appreciated."

5) How to mount electrodes

The attached figure shows (very schematically) how to make a cell without a titanium rod in a guiding glass tube (see Oriani's illustration). My cell is open and the rigid column slides into it.

Figure 2 373column.png

6) My CR-39 Chips for measuring background

Each of my post-electrolysis chips has a matching control chip--it is kept in the unused electrolyte during electrolysis. In additions to this, one control chip will be mounted inside the electrolytic cell in exactly the same way as it is during electrolysis, including the position of electrodes. But electrodes will not be connected to the power supply, as Richard did. This test will be much longer than three days; it will show me what is the total contribution of radioactivity, if any,

from the polyethylene, Mylar, o-rings, the electrolyte and the cathode.

7) Another message posted on CMNS list (7/2/2009)

"As I specified before, there are only two departures from Richard's protocol,

http://csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

in the ongoing project. They are:

(a) Keeping chips in distilled salty water, when they are waiting for electrolysis and when they are waiting to be etched. Richard wrapped them in aluminum and kept them in air. During each electrolysis his control chip is kept in the unused electrolyte. We am doing the same thing; in the past (see my Catania report) I kept control chips in air.

(b) The rear side of a chip, during electrolysis, is protected from air (by a ~0.5 mm layer of polyethylene).

Why am I trying to minimize exposure to air? Because I suspect that electrostatic charges somehow appear on CR-39 surfaces kept in air. Some areas become positive and others become negative. The reason for suspecting this is based on the fact that distributions of tracks on control chips, left in air for long time, are often very nonuniform. That could be explained by assuming that some areas attract positive radon ions from air while other areas repel them. Keeping chips in distilled salty water, and using a polyethylene protection, reduces exposure of CR-39 to air (from many days to no more than about 20 minutes). How can this hurt? Following Richard protocol, our control CR-39 chips are kept in a bottle with unused electrolyte, during electrolysis. This also protects chips from air.

P.S.
Only three people joined me in The Curie Project (all three are from Phys-L, an Internet list for physics teachers). Do you agree that the result would be treated more seriously if there were at least one or two electrochemists, with impressive credentials, among us?

The goal of the project is modest; it is to verify Oriani's results--nothing more at this stage. Suppose we confirm that excessive tracks are reproducible on demand. Then we might have a good chance of obtaining financial support, from NSF or DOE, for more sophisticated investigations, for example, with electronic detectors. The goal would be to identify particles responsible for observed tracks, to find parameters influencing the rate of emission, and to test specific predictions based on theoretical models.

IT IS NOT TOO LATE TO BECOME A PARTICIPANT IN THE CURIE PROJECT. "

8) CMNS researchers did not reply to my appeals.

Why not a single CMNS researcher joined me in The Curie Project? Most of them probably agree that participation of Ph.D. electrochemists would be very helpful, as far are credibility of results is concerned. But they prefer to work individually, on their own projects, rather than to work together to verify already-published results. That is why progress is so slow in this area.

9) A message sent to three cooperating teachers

Practical details are usually not described in published papers. But details might help three teachers who will perform experiments in The Curie Project. I assume they read Oriani's paper. Here is what I wrote: "I just started Experiment #7. All goes smoothly. Here is a description of details. Perhaps this will be useful.

1) Performing experiments at home is convenient. This allows me to record the current avery several hours, without any effort. Each experiment now takes three days. But etching will be done at school; in the chemistry lab, under the hood. This is because etching solution, if spilled, is much more dangerous than the electrolyte.

2) I have several well labeled containers. They are:

a) Unused electrolyte (a plastic bottle)
b) Used electrolyte (a plastic bottle)
c) Unused electrolyte for control chips (a plastic bottle with a wide opening)
d) Salty water for unused chips (a beaker)
e) Salty water for used chips (another beaker)
f) Distilled water (also a beaker)

2) Initially all CR-39 chips are in (d). To perform an experiment I remove two chips from (d), rinse them in (f) and set up an experiment.

3) Here is a very important detail. I make a pencil contour of each chip. Control chip is at once placed into (c); the chip is totally immersed.
Each experimental chip has a label, scratched on its surface. My convention is to position the chip in such a way that the scratched label is facing down (away from the electrolyte). Using Richard's terminology, I can say that my front surfaces are unlabeled while my rear surfaces are labeled. By careful; you must know which side is facing what, during the electrolysis. It is easy to make a mistake. I double-check the chip orientation after each experiment. Recognizing the side where the label is might be difficult. But it becomes easy after a right kind of illumination, and the angle of observation, are found. In case of doubt; one can always do this under the microscope.

4) My cell is not as tall as Richard's. But I have enough of electrolyte above the anode to last three days. At the end of day 3 the level of electrolyte is about 8 mm above the anode. I would certainly add a little of water if I noticed a possibility that the level might become much lower during the night, or during a prolonged absence. During the electrolysis, the current increases slowly from the initial value of 65 mA to about 90 mA. Following Richard's advice, I am not trying to keep the current constant (which can be done by changing the voltage or by adding distilled water to the cell).

5) I am using very light cables between the power supply and electrodes. Each cable is scotch-taped to a supporting stand, near the cell. In that way the weight of electric cables does not contribute much to the weight of the column inserted into the cell. I practiced this with during the first experiment (when a layer of polyethylene was used instead of CR-39 and when distilled water was used instead of the electrolyte).

6) At the end of the third day, I reduce the voltage to zero and turn the power supply off. The cable clips are disconnected from the Pt and Ni wires and electrodes are slowly removed from the cell. Then the used electrolyte is placed into (b) and the cell is carefully dismounted. I examine the Mylar and write down what I see (one time I had a hole in Mylar). I also examine the CR-39 surface and write down what I see. (Is it dry or is it wet? It is always dry, but this should be checked.) During that examination I always see where the 0-ring was, because it makes a visible imprint on the chip surface. This allows me to draw a circle on the contour I made in the logbook (see item 3 above). This circle defines the area in which tracks will be counted, after etching. It is important to be organized, and to have a logbook. After drawing the circle, I rinse the post-electrolysis chip in (f) and place it into (e). The control chip is then removed from (c), rinsed in (f), and also placed into (e). Contours of chips, and scratched labels, will allow me to identify chips later.

7) Then I proceed toward the next experiment, etc., etc. Etching will be performed after all experiments are performed. Meanwhile all used chips are in salty water. I add some water to this beaker occasionally, to compensate for evaporation.

8) Let me know when you are ready (after observing tracks on the little chip that was irradiated with alpha particles, and after building a cell). I will send you enough chips for three experiments (two chips per experiment). I suggest we share the results on the same day; probably before the end of September. In this way we will not influence each other."

10) Another message posted on CMNS list (7/4/2009)

I do not know why no one replies to messages I keep posting about The Curie Project. Perhaps no one reads them;

perhaps CMNS researchers think that the topic is not worth discussing. Below is another message; will it generate some interest? This remains to be seen.

'What does the adjective "reproducible-on-demand" mean? Strictly speaking, reproducibility is not possible because every measurement is associated with a random error. The same can be asked about the "highly reproducible," about "reproducible," and about "more or less reproducible." This question, particularly important in our field, is prompted by Richard's ICCF14 report

http://csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

The abstract of that paper contains the following: "However, not every electrolysis experiment produced nuclear particles so that total reproducibility was not achieved. Therefore another experimental technique has been developed which has shown the generation of nuclear particles in each of twenty five consecutive electrolysis experiments using heavy or light water solutions of lithium salts." Does this statement mean that Oriani-type experiments are "reproducible on demand" ?

The purpose of The Curie Project is to verify that consecutive experiments demonstrate emission of nuclear particles due to electrolysis. Note that results from the first eleven experiments, in Richard's Table II, were performed with the older CR-39 in which the mean density, on 16 control chips, was 26.4 tr/cm^2 (st.dev. 12.1). The remaining 14 experiments were performed with the newer CR-39 in which the mean density, on 13 control chips, was 13.6 tr/cm^2 (st.dev.=6.8).

Please help me to decide what kind of minimum results would be needed to conclude that The Curie Project validated reproducible generation of nuclear particles due to electrolysis. I will probably perform at least ten experiments. Three other participants will probably perform at least three experiments each. Richard's contribution will be in the form of results from experiments performed after ICCF14. Do you agree that a definition of "success" should be made before the results are known?

P.S.

IT IS NOT TOO LATE TO BECOME A PARTICIPANT IN THE CURIE PROJECT. '

11) Comments etc.

a) Ed Storms, the author of a recently publish CMNS book

http://www.lenr-canr.org/Introduction.html#StormsBook

posted this message on the CMNS list: "I would like to extend the discussion that Ludwik has started. Two different types of experiments are possible. The first tries to demonstrate that a phenomenon is real. Reproducibility is important when the reality of a phenomenon is in question. This the issue Ludwik has raised with respect to the Curie project, i.e. how big must the result be before it can be considered real? The other kind of experiment accepts the reality of the effect, no matter how difficult it is to reproduce or how big it might be, and tries to understand how and why the phenomenon occurs. I think this is the kind of experiment we now need because the phenomenon has already been shown to be real by a huge data set.

We now need to understand the variables and conditions that affect the phenomenon. This means that people need to explore different conditions, not simply duplicate conditions that have been successful in the past. A very large parameter space awaits exploration, which requires trying many conditions, most of which will give nothing. However, the conditions that give a positive result become important because they provide a boundary to the active parameter space. Experiments that attempt to exactly duplicate previously successful conditions provide very little new information other than to show that the collection of conditions created at the time, most of which are unknown and unmeasured, happen to combine to give a positive result. The only way to determine which of these conditions is

important is to change them and see what happens. For example, suppose the radiation is very sensitive to the concentration of $PdCl_2$ and the chosen value happens to be on the edge of the critical value. One person happens to be on the high side of the designated value and gets a positive result and another person happens to be on the low side and gets no radiation. Without knowing the true effect of the $PdCl_2$ concentration, the result will provide no useful information other than to give one more positive result to show skeptics. This is only one example of hundreds of possible variables that might have an effect. We need to start the long and boring exploration of these possibilities rather than trying to simply reproduce the effect. Without a useful theory (perhaps too many theories) to guide this search, the work needs to be done mainly by trial and error methods until some of the important variables have been identified.

On the other hand, I agree that discovery of a reproducible method would make such an exploration of variables easier. For example, it is easier to find first base if the location of the ball field is known in advance. However, we don't even know the city in which the ball field is located. We need to send out explorers to every city we can think of to locate the ball field before we start asking the location of first base. We have shown by chance that the ball field actually exists, but the map was never properly drawn by the successful explorers. We don't need to once again prove the ball field exists. We only need to locate it in a way that results in a useful map.

This means the variables we have available in the experiments need to be changed by amounts that are easy to measure in order to be sure that the variables are not affecting the result by small chance variations. This takes time so, I suggest, the sooner we get started the better."

Hmm, "people need to explore different conditions, not simply duplicate conditions that have been successful in the past." That might be a good explanation CMNS researchers do not want to participate in The Curie Project. But shortly then I received a message from a after young researcher wrote does want to collaborate. Announcing that event, I wrote:

"1) I am happy to report that one researcher from our list is also going to participate in The Curie Project. That is very welcome; the more the better.

2) That person wrote: "My goal of working on the Curie Project is so that I can give simple reproducible experiment to scientists who have venture capitalist connections and let them run it and get them excited about it. The key is that I need something relatively simple and highly reproducible.

I have some CR-39 and I'm gathering the equipment over the next few weeks. Do you have any useful components that I can buy from you? In particular, where to buy the Mylar?"

3) We have practically no 6-micron Mylar left. Can someone send me some, to be sent to project participants? Each Oriani-type experiment uses about one square inch of Mylar. Five participants, performing 5 experiments each, would use about 30 square inches. I would be happy to pay for about 300 square inches, in anticipation of future experiments.

4) Will the "reproducibility on demand" be confirmed in The Curie Project? We will probably have a clear answer in September.

5) Please help me to decide what kind of minimum results would be needed to conclude that generation of nuclear particles due to electrolysis is "reproducible on demand." I agree with Ed that a lot of investigations will be needed, after reproducibility on demand is accepted. Each sequence of several experiments would begin with an experiment that always produces excessive tracks. Other experiments in that series will be conducted to answer a simple question--what effect a change in one parameter has on the mean track density? A change in one parameter could be:

a) Replacing Ni cathode by a Pd or Pt cathode.
b) Placing the cell into a magnetic field.
c) Replacing one kind of electrolyte by another.

6) I also agree with Ed that this kind of investigations would benefit enormously from specific theoretical (computational) predictions. Something like this:

a) My theory predicts that replacing Ni by Pd would increase the mean track density by at least one order of magnitude.
b) My theory predicts that magnetic field will reduce the mean track density by a factor of about two or three
c) My theory is that using $PdCL_2$, instead of $Li2SO_4$ will increase the mean track density by two orders of magnitude.

Theoretical predictions should be specific; the more specific they are the more useful they are likely to be."

In a private message one CMNS researcher wrote:

" 1) In my opinion, in addition to reproducibility one might test for additivity (number of tracks of particles due to electrolysis should be proportional to duration of electrolysis). And it should be proportional to excess heat. Naturally, this should be true when other conditions are strictly identical. My prediction is that there will be no proportionality; there will be some kind of saturation.

2) In applying magnetic field, one must not forget about the terrestrial field. Do not be disturbed by its small size. In the final analysis, we do not know what is the nature of terrestrial magnetic field, no matter what the experts say. By the way, terrestrial field has two components. That is why it would be useful to apply magnetic field in both directions. My prediction is that the form of tracks might change radically when the vertical field (exceeding 20 Gs) is applied.

3) S-based electrolytes might produce stronger effects than Cl-based electrolytes.

Ludwik, I am aware that these comments are useless. There is no need to answer, or to pay attention to them. "

This is not what a useful theoretical prediction; the author is aware of this. Why is S more desirable than Cl? Why should vertical magnetic field produce the described effect? What is the cause of the described saturation? Predictions without justifications are not likely to be productive. Cooperation between theoretical and experimental scientists is necessary. Theoretically inclined people should be informed about essential experimental details; experimentalists should be informed about assumptions on which theoretical calculations are based, and, if possible, with mathematical equations as well.

12) More comments

In a message posted on CMNS list, another scientist wrote that 100% reproducibility is not a necessary requirement for the existence of a phenomenon. Some CMNS effects are not reproducible on demand because we do not know how to control them.
Responding to this I wrote: "That is true. But, according to Table II in

http://csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

each of the 25 consecutive experiments "has shown the generation of nuclear
particles." That is why the initial goal of The Curie Project is to verify the high level of reproducibility. Suppose we confirm that Richard's protocol yields highly reproducible evidence of chemically-induced emission of nuclear projectiles. Suppose that a paper, coauthored by all project participants, is submitted to Physical Review, or to another journal that rejected Richard's original manuscript. Suppose that this fact is widely publicized (with help of honest TV journalists, etc.). I suspect that this might produce at least one desirable result--publication of our paper. That would be an important step toward the end of unfair discrimination.

At the other extreme we might find the opposite situation--no one but Richard observed a clear signature of a nuclear process due to electrolysis. That would be the end of the story, as far as high level of reproducibility is concerned, at least for me. And what if some of us, excluding Richard, observed the clear signature while others fail to observe it?

That would be a situation described by Brian. We would say that Oriani effect is not very different from many other CMNS effects--it seems to be real but not yet reproducible on demand. Of course, we hope that the exceptionally high level of reproducibility will be confirmed. That would be our "success." But we should be prepared for different outcomes--a "failure" or a "partial success."

Another message posted by Ed Storms
Responding to an interesting observation made by another CMNS researcher (also about importance of reproducibility) Storms wrote: "I agree with what you say and with the logic behind Ludwik's approach. However, I do not agree that a reproducible way to make pits in CR-39 will convince skeptics that LENR is real. First of all, the measured flux is very small and is not clearly related to the other kinds of evidence for a LENR effect. Second, the method is not well understood by the scientific establishment, which would be necessary for the effect to be considered proof. The neutrons claimed by Pam et al. get deserved attention for reasons that Ludwik and other successful studies cannot duplicate. The SPAWAR results were examined at a respected laboratory using high level professional equipment. Even if a procedure is found allowing anyone to make the required pits, the results will not prove that important heat can result from such a reaction. Such a procedure will show only that low level radiation can be produced, which I agree is important but not important enough to interest serious founders, especially ones who want to make energy. Cold fusion is important because it might make useful energy and it implies a nuclear reaction rate far in excess of the SPAWAR results, which is in excess of what can be explained by any theory.

Instead of trying to simply reproduce one small part of the effect this way, I suggest the best approach is to focus on trying to understand the effect, either what is required to make it happen at a higher rate or the nature of the radiation. Granted, getting the effect to work is essential before it can be explored. Nevertheless, I'm suggesting the emphases be placed not on reproduction but on understanding, with reproduction being only the first and not even the most important step. The stated goal would not be to provide a method people could use in their own lab to prove to themselves that the effect is real. Instead, the goal would be to use the reproducible procedure to discover something important about the effect. While this would happen automatically when a reproducible procedure is found, stating it up front with emphasis gives the effort more importance than simply trying to achieve reproducibility."

And here is the message posted by me: "Brian, Ed and Tom clearly identified two distinct aspects of our dilemma: we need to understand what is going on, and we need protocols yielding reproducible CMNS signatures.

Why do I think that focusing on reproducibility is now more important than focusing on understanding? Because, as stated above, we do not have an "accepted baseline case from which controlled explorations can be made." Suppose I want to learn about the effect of X on excess heat, or on the rate of a transmutation. I perform ten experiments and results scatter from nothing to a lot. What only thing I learn is that experiments do not yield information about the effect of X on the selected effect. This can be contrasted with a situation in which results from consecutive experiments are more or less similar. I obtain the mean value of the result and start another sequence, for example, after increasing or decreasing X. This would tell me something definitive about the effect of parameter X. Focusing on understanding is not possible without a reproducible baseline."

On July 7, 2009 I wrote:
How can one objectively distinguish real excess tracks from apparent excess tracks? I had a plan for this. Unfortunately, the number of participants in The Curie Project is too small to implement it. (Not counting Richard, whose results we will be verifying, there are five of us: two high school teachers, two engineers--who are on this CMNS list--and myself.) Suppose the number of participants is 25, instead of 5. Suppose that each of us performs 6 experiments. This would produce 25*6=150 measurements of track densities. Let me ignore two trivial outcomes-- (1) every measurement is positive (confirming that track densities due to electrolysis are significantly higher than on control chips), or (2) every measurement is negative.

The third possible outcome is that some measurements yield positive results while others yield negative results. I have a plan for this difficult scenario (see below). Do I explain it clearly? Would it be acceptable to most scientists? What is wrong in this plan? What is a better alternative?

a) Each participant has a series of six results. This gives us 25 mean track densities, one from each participant.

b) A histogram of mean values is constructed. Suppose the first bin is for means between 0 and 30, the next is for means between 30 and 60, etc.

c) Yes, 25 is not a large number. But it might be sufficient to answer a simple question--is the distribution consistent with the bell- shaped curve?

d) Reasonable consistency would then be interpreted as an indication that excess tracks are real (the mean of means becomes our estimate of the "true" excess track density). Clear inconsistency, on the other hand, would prevent us from making this assertion. Yes, I would also prefer 250 participants, instead of 25. But this is totally unrealistic, in our situation. Suppose 12 means are in the first bin, 6 means are in the second, and the remaining means are nearly randomly distributed (a tail) among bin 3, bin 4, bin 5, etc. That would certainly not be consistent with the expected bell-shaped distribution.

The rational for depending on the histogram of mean values is based on the so-called "central limit theorem" of statistics. If occasionally- observed excess tracks are real then the distribution of mean values must be Gaussian, provided the number of mean values is very large. Existence of a "true value" is equivalent to the existence of a large unchangeable population from which individual samples (yielding mean values) are randomly selected."

Ed Storms responded:
"Ludwik, I think you are making this problem too difficult. If you do not see significantly more tracks on the CR-39 that is near the cathode compared to locations more distant, then you have failed to demonstrate reproducibility. The exact number of pits does not matter. Your object is to demonstrate the reality of the effect, if the reality cannot be recognized by eye without complicated mathematical analysis, it has failed. The Spawar results were easy to recognize as localized radiation simply by visual inspection of the CR-39. You need a similar result if you intend to claim to reproduce this result. Of course, complicated analysis is required to identify the nature of the radiation. However, this is a secondary result that does not appear to be part of your procedure. On the other hand, if your object is to study the effect and show which variables are important, as I suggested, then you need to make qualitative measurements including when pit density is low, because you need to relate pit density to the different conditions being explored. However, this does not appear to be part of your procedure either."

My reply:
"1) Yes, situations in which statistical analysis is not necessary are preferable.
2) Particles producing "tracks due to electrolysis" might be produced in the electrolyte, as reported at ICCF10. The first thing I will do, if reality of excess tracks is confirmed, will be to change the distance between the cathode and the chip from nearly nothing to ~2 mm (which is more than sufficient to stop alpha particles emitted from the cathode). The purpose of The Curie Project is nothing more than to get a clear yes-or-no answer about reality of excess tracks via Oriani's protocol. Investigations of parameters is tempting; but we should not be distracted. This would be the next step."

On July 8 I posted this message:
" 1) I hope some of those who are not able to perform The Curie Project experiments will be able to participate in the analysis of results. Our data will probably be available in September. But this should not prevent us from discussing strategies. Please save this message; it might be useful later.

2) As you know, row data are summarized in Table II of Richard's ICCF14 paper

http://csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

In table I of this paper he tells us that the first 11 experiments were performed using CR-39 where mean background was 26.4 tr/cm^2 (stdev=12.1 tr/cm^2). Better CR-39 was used in the remaining experiments (mean=13.6, stdev=6.8)

2) What do we want to accomplish? We want to answer two questions: (a) are our results statistically consistent with Richard's results? (b) Do our results (and Richard results, summarized below) support the claim that his protocol yields

a highly reproducible signature of a nuclear effect due to electrolysis?

3) The most interesting Richar'd result comes from his experiment 9 (too many tracks to measure). But I am excluding it; I am going to use only what was actually measured. The first two experiments (SPAWAR-type electrolysis) are also excluded. In other words, we have 21 experiments in which track densities were measured. Each experiment provided two numbers (after first etching)--track density on the front surface and track density on the rear surface. For the time being I am place all measured densities into one set of 21*2=42 results. What follows are measured track densities, listed in ascending order; ignore the number in parentheses; it identifies the experiment, not the number of tracks per square centimeter (like the first number in each line).

9(16)
9(17)
11(14)
16(4)
26(25)
28(18)
35(19)
36(16)
38(12)
38(24)
40(10)
41(20)
47(19)
48(11)
49(15)
51(23)
60(21)
62(23)
70(8)
71(7)
72(20)
74(6)
76(6)
80(8)
81(14)
96(7)
98(10)
102(18)
102(24)
127(17)
132(21)
167(12)
193(13)
195(15)
207(22)
229(11)
298(13)
344(25)
352(4)
393(5)
426(22)
498(5)

4) The mean value is 122 tr/cm^2; the standard deviation is 124 tr/cm^2, and the median is 73 tr/cm^2 (in other words, one

half of the results produced 73 tr/cm^2 or more). A large difference between the mean and the median indicates that the distribution is strongly skewed. This is confirmed by following histogram:

bin 1 (0-30 tr/cm^2) < ---------- 6 results
bin 2 (30-60 tr/cm^2) < ---------- 10 results
bin 3 (60-90 tr/cm^2) < ---------- 9 results
bin 4 (90-120 tr/cm^2) <-------- 4 results
bin 5 (120-150 tr/cm^2) <--- 2 results
bin 6 (150-180 tr/cm^2) <--1 results
bin 7 (180-210 tr/cm^2) <------3 results
bin 8 (210-240 tr/cm^2) <--1 results
bin 9 (240-270 tr/cm^2) <0 results
bin 10 (270-300 tr/cm^2) <--1 results
bin 11 (300-330 tr/cm^2) <0 results
bin 12 (330-360 tr/cm^2) < ---2 results
bin 13 (360-390 tr/cm^2) <0 results
bin 14 (390-420 tr/cm^2) <--1 results
bin 15 (420-450 tr/cm^2) <--1 results
bin 16 (450-480 tr/cm^2) <0 results
bin 17 (480-510 tr/cm^2) <--1 results

5) How to deal with a situation in which the distribution is not Gaussian, and when the standard deviation is about the same as the mean value? These are real experimental data. Knowing that the standard deviation is 124, what is the level of confidence in the following statement "The mean=122 tr/cm^2 is significantly larger than the background." The level of confidence would higher than 90% if the distribution was roughly bell-shaped and if the standard deviation were something like 40 tr/cm^2, or less. ”

Ed Storms responded:
‘If the standard deviation is as large as the value, my suggestion is to ignore the results and work to improve the signal rather than applying math. No one will believe the results no matter how you "skew" the data.’

My reply July 9, 2009
“I have no doubt, at the intuitive level, that Oriani's results provide a very strong evidence that a difference between the mean track density--on his 42 post-electrolysis CR-39 surfaces (122) and the mean track density on his control chips (26)--is highly significant. But I do not know how to calculate the level of confidence for this statement. That is not a good reason for ignoring the results.

Yes, a significant difference does not guarantee that others will report similar results; that is what the Curie Project is about. The more participants we have the better. IT IS NOT TOO LATE TO BECOME A PARTICIPANT.

P.S.
Intuitive evidence is not a guess; it must also be justified. Here is the justification for my use of the term "intuitive level" above:

Richard tells us that experiments from #4 to #11 were made with CR-39 where mean background control density was 26 tr/cm^2. This amounts to 8 experiments, or 16 measurements of track densities. Among these I see only one result where the density was less than 26. All other track densities, including the four from the very impressive experiments #5 and #11, are larger than 39. Please let me know why an honest skeptic would reject this argument.

The last 14 experiments were made with CR-39 where the mean background control density was 14 tr/cm^2. This amounts to 28 measurements of densities. Among these I see only three results where the density was less than 14. All other densities are larger than 25 tr/cm^2, including ten densities higher than 100 tr/cm^2. Please let me know why an honest skeptic would reject this second argument.

How can anybody doubt that the difference between Richard's control chips results and his results from electrolysis experiments are dramatically different?

Is it not true that Richard's evidence (that post-electrolysis densities are higher than control densities) is very satisfactory? "

My message posted on July 10, 2009
"Some phenomena are reproducible but rare. In other words, one does not always observe them. How to test for reproducibility of such results? By comparing what happens with what should happen if results are reproducible (reproducible results are expected to to satisfy the Poisson distribution should). The shape of that discrete distribution depends on only one parameter L (lamda). Both the mean value and the standard deviation are equal to L. Calculations are no longer tedious and error prone; use the online calculator at:

http://rockem.stat.sc.edu/prototype/calculators/index.php3?dist=Poisson

Suppose a reproducible event is claimed to occur only twice per day (on the average). I attempted to detect it in ten consecutive days nothing was found. One does not need to be very sophisticated to conclude that the claim was not validated by me. But this can also be demonstrated by using the Poisson distribution. What is the expected (most probably) number of observations in 10 days? it is 10*2=20. What is the probability to observe 5 observations in ten days? I make L=20 (most probable result) and x=5. According to the Poisson distribution (use the calculator above) the probability for X=5 is 0.000005. And the probability that X=0 is practically zero.

But in some cases intuitive predictions are not obvious. Suppose 16 observations were made. Is this consistent with the expected value of 20? For X=14, the probability is 0.039. This is not much smaller than 0.089, the probability for X=20. In other words observing something only 14 times in ten days, instead of 20, is not very unlikely.

Let me apply this approach to Richard's observations. According to my summary (posted two days ago) based on Table II in

http://csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

Richard examined 42 CR-39 surfaces and found 15 cases in which densities exceeded 100 tr/cm^2. According to Poisson's distribution (L=15 and X=15), the probability of observing exactly the same result as Richard is 0.102. Another researcher also measures track densities on 42 surfaces and finds that track densities exceeding 100 are on only 12 surfaces. Is this consistent with Richard's observations? The probability of that outcome (L=15, X=12) is 0.083. That is not much smaller than 0.102, the probability of the most likely result. I would say that 12 is in good agreement with Richard's result, 15. But what about X=5? In that case the probability would be 0.002. I would say that Richard's result, 15, is not reproducible.

Is there anything wrong with this approach? It is highly desirable to have an agreed upon test for reproducibility before The Curie Project results are known. The same approach, by the way, can probably be used for other skewed CMNS distributions. Excess heat, for example, as summarized in Ed's book, was more often observed at low powers than at high powers. That is also a very skewed distribution."

Ed Storm responded:
"You have two questions to address, Ludwik. Do the pits result from a random process or are they the result of rare special conditions? Your approach assumes the former, which is not very interesting. You are trying to show that a rare process is initiated under certain conditions. This is not a random process, hence the math you use does not apply. You need to show that the effect is larger than would be expected from a prosaic process. You need to identify the likely prosaic processes and show that they do not occur at the observed level. This requires changing the conditions in a known way and seeing the effect of these changes. Simple duplication to which math is applied based on a random process means nothing."

My posted question (after quoting the first question above):
"What kind of random process would produce many more tracks on experimental chips than on control chips? Richard's control chips are treated in exactly the same way as experimental chips, except for one thing. Control chips are in the bottle with unused electrolyte when experimental chips are exposed to a thin Mylar film supporting the cathode."

P.S. (after several hours of silence from the list). What preoccupies me does not preoccupy others. Each researcher has his own priorities. But researchers do not share their experimental problems with each other very often, except at formal meetings. That is rather unfortunate. :-)

I am deliberately trying to separate two questions: (a) How to decide, that The Curie Project results (when available) are consistent with Richard's results, and (b) whether or not a given set of results provides evidence of a nuclear process due to electrolysis. The second question is more interesting and more important. But, for the time being, I am focusing on the first question. This should not prevent us from addressing the second question, in view of what was reported by Richard in

http://csam.montclair.edu/~kowalski/cf/368TGP_oriani.pdf

Our sets of results might be consistent with each other without providing evidence for a new nuclear process due to electrolysis. Another possibility is that our sets of results are not consistent with each other but each provides evidence of the desired CMNS process. What is wrong with trying to answer the first question before addressing the second one?

Please help me to find an agreed-upon method for objectively answering the first question. Richard results were presented at ICCF14; The Curie Project results will probably be available in September. It would be useful to agree on the method before the results are known. Should we count cases where the mean density exceeds 100 or should we count cases where it exceeds 60 tr/cm^2 ? How to objectively justify the choice?

In principle, our two sets of results should be consistent (or inconsistent) no matter what lower limit is chosen. But our sets suffer from small numbers of measurements. What is true for a large set of measurements, such as 400, is not necessarily true for a set of only 40 measurements. Here is one possible choice (upper limit 60 tr/cm^2) and its justification:
a) 16 control chips (older CR-39) showed the mean of 26.4 tr/cm^2 (stdev=12.1).
b) 13 control chips (newer CR-39) showed the mean of 13.6 tr/cm^2 (stdev=6.8).
c) The weighted average is very close to 17 tr/cm^2. We chose 60 because it is more than three times larger than 17. (Also note that 26+12+14+7=59)."

On July 11, 2009, at 9:33 AM, Edmund Storms wrote:

"Ludwik, although the electrolyte does not attack CR-39 before electrolysis, the situation can change when electrolysis starts. Many very reactive molecules are made during electrolysis and I expect some will pit CR-39. This pitting will be tricky to separate from radiation because pitting, like radiation, will be less the further the distance from the cathode. In addition, a protective covering designed to stop chemical attack will also partially stop radiation. Of course, a careful study can separate these effects. Consequently, some of the pits could be caused by chemical attack and their number will appear to be random depending on a combination of many uncontrolled conditions. Since chemical attack is the most likely source of pits, the experiment needs to explore conditions that can clearly separate this possibility from the effect of particles. When very few pits are seen, this becomes a difficult problem. Because CR-39 accumulates any effect, the rate can be very small, hence might be caused by any number of prosaic processes, some of which might be hard to predict without careful study. In contrast, the Spawar study produced so many pits that many of these effects could be eliminated. The Curie Project needs to see a similar large number of pits."
justify the

My reply: “Thanks Ed,
1) I am neither a chemist nor a material scientist. But I will be very interested in what other electrochemists are going to say. In some Oriani's experiments control chips were actually in the cell with the Mylar, the cathode and the electrolyte above. The only difference was that the current was zero, rather than many milliamps. In other words, the effect is due to the electric current. You are probably thinking about chemical reactions caused by the current. Right? The electric wattage is very low (about 0.6 W in my case) and the cathode is in thermal contact with the cold electrolyte.

2) Are you aware that according to the most recent article, published by the SPAWAR team, about 90% of pits disappear when the chip is protected from the electrolyte by a thin layer of Mylar? Yes, the remaining 10% is still a lot of pits; probably "too many to count."

3) The codeposition procedure (covering chip #7 with Mylar), used by F. Tranzella, on the other hand, yielded a very different result, as reported in Catania (Lipson et al., page 182 of our proceedings). The track density became comparable to what is typically observed by Oriani. Furthermore, Tanzella's tracks were identified as protons of several MeV. What should we think about this apparent controversy? Would you agree that your suspicion of chemical effects is also applicable to Tanzella's results? If not then why not?

4) Two days ago you asked for the tiny bubbles in my cell. The electrolyte becomes "milky" about two or three seconds after the current is turned on. But it remains "milky" for about six to eight seconds, after the current is turned off.”

Ed’s reply:

“1) The current creates many unstable chemical species that can react with relatively inert material during their short lifetime. For example, stainless steel is stable in H_2O+LiOH but is not stable when the fluid is electrolyzed. These unstable, reactive compounds are made at the electrodes which result in a concentration that is greatest near the electrodes. Therefore, the drop off in pit density away from the cathode can be caused by how far the reactive material can diffuse before it decomposes rather than by absorption of particles in a greater length of fluid.

2) Thanks, this is important information. The question is whether the Mylar is fully protective or whether some of the reactive species can eat through the Mylar. However, the pits on the opposite side that is not exposed to the electrolyte would seem to be caused by real energetic particles. Also the pits that show clear characteristics of particle effects also provide good evidence for particle emission. As long as a large number of such pits are present, I would accept the conclusions.

3) I agree, possible chemical effects become more important when the pit density is low.

4) Interesting. You are obviously making many very small bubbles that seem to be related to the nature of the cathode surface.”

A message posted by Scott Little (7/12/2009)

“Ludwik, One of the problems you face in interpreting results from your Curie project is this: Do the pits look right? IMHO, this subjectivity is one of the greatest shortcomings of CR-39. Please look at some of the photos we posted in our SPAWAR report here:

http://www.earthtech.org/CR39/index.html

Alpha particle pits are fairly uniform in size and have a random spatial distribution over the surface of the CR-39. As a result of this distribution, overlaps are relatively rare. Compare this with what we called the "soap bubble" appearance which we are virtually certain is the result of chemical attack on the CR-39.

Note also the appearance of pits caused by various mechanical damage means. Again there is a high number of

contiguous groups of pits, which is not expected from energetic particles. Yes, I understand that there is a hypothesis for multiple tracks from energetic particles but surely the overall cross-section (nuclear cross-section multiplied by a geometric cross-section expressing the probability that the event is properly located/oriented so that it develops during etching) for that interaction is very small compared to the prevailing interaction that produces a single pit.

If you find lots of pits in your Curie experiments, please post lots of good microscope photos so that all of us can help you judge whether or not they look right.....)"

Ludwik's reply:
"Thanks Scott,
1) Pictures of our pits will be shown at my website. And I will be happy to send etched chips to those who might be interested. The goal of The Curie Project is to verify Richard's results. His pits are very similar to those produced by alpha particles. That is why only such pits will be counted. Will we also observe pits (on both sides of CR-39), at densities exceeding what is typical for his control chips (mean about 20 and st.dev. of about 10 tr/cm^2)? That remains to be seen.

2) Referring to Oriani-type experiments, you wrote: "We performed background counts on both sides of 8 chips. The average was approximately 100 pits/cm^2." That amounts to 16 surfaces. I suppose your four-years-old CR-39 chips were from the sheet whose part you sent me four years ago. Please confirm this. My recent measurements show that the mean background, for that sheet, is 26 tr/cm^2. I plan to verify this. If confirmed then your average of 100 tr/cm^2 would be consistent with Oriani's measurements, at least qualitatively."

Ludwik (replying to Ed's message; 7/13/2009)

"Dear all,
1) My suggestion is to start using electronic detectors instead of CR-39, as mentioned in an earlier message. Placed in air, next to a Mylar window, they would not be sensitive to "unstable chemical species." The same, by the way, would be true for a CR-39 detector placed about 1 mm away from the Mylar window. Furthermore, as many of you know, silicon-surface-barrier detectors do much more than to accumulate signals produced by nuclear projectiles; they provide information about their energies and about times of emission (does it occur in bursts? does it change when the electric current is increased? What happens when the current is turned off? What happens when the magnetic field is applied? What happens when the electric current is redirected to another cathode, when several drops of something is added to the electrolyte, when .....? etc. etc.).

2) Why should several hundred tr/cm^2 be called "very few pits" when Oriani's mean control chips density is about 20 tr/cm^2 (and the stdev is about 12 tr/cm^2)?

3) The problem with artifacts is more general. Three kinds of CMNS phenomena: "excess heat," transmutation," and "emission of radiation" were already named in 1989/1990. I am thinking about Bockris, Fleischmann, Jones, and Pons. Two objections were immediately raised by skeptics: (a) results are not reproducible. (b) results might be due to artifacts. How do you know that your excess heat is due to a nuclear process, and not to another (known or unknown) phenomenon? How do you know that the so-called "new elements" were not already present in your materials (or introduced during manipulations)? How do you know that CR-39 pits are not due to "reactive molecules," as suspected by Ed?

4) The immediate goal of The Curie Project is to verify that Oriani effect is reproducible. This is much easier than to provide convincing evidence against artifacts. Why is it so? Because the number of conceivable artifacts is practically unlimited. Knowledgeable people can always invent reasonable artifact scenarios. A claim for a discovery should be tentatively accepted after satisfactory arguments were presented against two or three most obvious artifacts. That is what usually happens in most areas of science, as far as I know. It is practically impossible to answer all conceivable objections when a discovery is first announced. We do not except a newborn baby to walk or run; this comes later. Tentative acceptance is a beginning of more refined investigations. Some initial benefit of doubt is usually given to qualified researchers. Why shouldn't we tentatively accept Oriani's effect as real? Convincing evidence

against obvious artifacts has already been presented by both Richard and the SPAWAR team.

5) Yes, I know that reproducibility is a precondition for all debates about experimental data. A negative result is not convincing unless experiments are known to be reproducible. Reproducibility is mother of science.

6) SPAWAR results, as far as I know, are reproducible when CR-39 chips are exposed to electrolyte, as in The Galileo Project protocol. But they are not reproducible when Mylar is used. This observation is based on what was known two years ago (Catania workshop) and what was published last April (by the SPAWAR team). Why was the controversy not mentioned in their last paper? What is the present status of reproducibility of SPAWAR results? ”

Appended on July 17, 2009
I do not know why numerous attempts to obtain information about the status of the Tanzella-SPAWAR discrepancy did not produce any new information on our CMNS list. Authors of papers to which I was referring are the list subscribers. Something important is probably behind this strange situation; what is it? Giving up on this, I posted another message. Last night I wrote:

“Theoretical topics, such as HUP, QM, QCD, standard model, existence of energy states below the ground state, etc., are certainly interesting to those who are qualified. But where should serious debates on such topics take place? Michal Gryzinski, for example, debated his ideas at gatherings of theoretical physicists well equipped to understand him, to criticize him, to help him, to explain his ideas to others, etc. Is the CMNS field appropriate for debating such issues? If I had something to contribute in the field of "quantum chromo-dynamics," for example, I would address those who know this theory well, and who use it routinely. I would not address a conference (or list) for geologists or chemists. Yes, QCD might eventually help them; but that would not be my excuse for addressing them, instead of addressing theoretical physicists. I am afraid that only a small fraction of us is equipped to discuss advanced theories.

Our field is already very controversial. We were not able to convince mainstream scientists that our claims (excess heat, transmutations, and nuclear projectiles) are valid. In order to succeed, we should stay away from additional controversies, at least for the time being. I am afraid that embracing other controversies could hurt us at this time. Do you agree?

P.S.
The interview with Fleischmann, published today, reminded me of another case where a premature attempt to explain experimental facts backfired. Suppose the claim made by Fleischmann and Pons were limited to "we discovered unexplained heat." This would lead to arguments about experimental techniques, the field in which Martin was a recognized first class international authority. But he said "therefore it must be nuclear," as quoted in the interview. Undesirable consequences of this premature statement are still with us. It would be better if they waited for independent confirmation of excess heat before speculating about its origin. Linking an interesting experimental fact with a controversial theoretical topic is an open invitation for troubles, with referees and with other scientists. Do you agree?”

A nearly-immediate reply, from Ed Storms, was about importance of understanding. How can I disagree with this? Science is not only a collection of experimental results; it is also a set of explanations. According to American] “National Science Teachers Association”

http://www.nsta.org/about/positions/natureofscience.aspx

“A primary goal of science is the formation of theories and laws, which are terms with very specific meanings.

*) Laws are generalizations or universal relationships related to the way that some aspect of the natural world behaves under certain conditions.

*) Theories are inferred explanations of some aspect of the natural world. Theories do not become laws even with additional evidence; they explain laws. However, not all scientific laws have accompanying explanatory theories.

*) Well-established laws and theories must be internally consistent and compatible with the best available evidence; be successfully tested against a wide range of applicable phenomena and evidence; possess appropriately broad and demonstrable effectiveness in further research..... While science and technology do impact each other, basic scientific research is not directly concerned with practical outcomes, but rather with gaining an understanding of the natural world for its own sake."

That answer to "what is science?" has not changed substantially since the times of Gallileo and Lavoisier. Science was, and continues to be, the basis for our technological civilization.

A discovery can be made experimentally or it can be made theoretically. Here is one well-known example. A Danish astronomer Tycho Brahe studied motion of planets and recorded their positions. His assistant, a German scientist Johanes Kepler, analyzed the result and formulated lows of planetary motion. His first law stated that trajectories of all planets are elliptical. His second and third laws were about the dependence of velocities of planets on their distances from the sun. Results of Brahe's investigations were raw experimental facts about individual planets; Kepler's law were generalization based on these facts.

Knowing about Kepler's laws, Isaac Newton discovered a theoretical explanation of planetary motion. He showed that Kepler's laws can be mathematically derived from only one assumption. That assumption is the existence of an attractive force between all objects in the universe. To be consistent with Kepler's laws, this gravitational force must be directly proportional to masses and inversely proportional to distances between the objects. Newton's theory was an important contribution to our understanding of nature; it is now used to design man-made satellites. Progress of science is based on experimental and theoretical investigations; they go hand and hand with each other, in a long run.

But neither Ed, nor other who responded, addressed the issue of negative effects of premature explanations. Instead of focusing on replications, readers (and referees) are likely to focus on explanations. Experimental data and theories go hand in hand but, in the final analysis, theories which disagree with facts are rejected, not the other way around. Ed Storms wrote:

"The issues involving QED, QM, and the Mills approach are basic to understanding LENR. Any theory must be consistent with what has been observed outside of CF, which includes phenomenon related to and explained by QM and the other models. Once a nuclear reaction takes placed in a CF environment, the consequence must be consistent with what has been observed to happen when similar nuclear reactions occur in other environments. In short, we need to see a link between CF and other observations. This means the theories used to explain these observations need to be understood before they can be applied to CF in order to show a relationship to other phenomenon.

Clearly QM is not fully understood by conventional science even when it is applied to accepted observations, hence the debates. In the process of understanding CF, I expect an understanding of the basic ideas behind QM will increase. I'm sorry to say we need to understand these ideas whether we like it or not. Also, we should not compromise our discussion just because some scientists are closed minded even about subjects that do not involve CF. These narrow minded people are clearly not able to evaluate reality even in normal science much less when it comes to the challenge of CF. We should not let their deficiencies influence our approach."

Two people wrote that they enjoy theoretical discussions on the CMNS list. Responding to them, I wrote:
" Me too. Yes, we do need theories to guide us. In the long run, experimental data and theories go hand and hand. Keep in mind, however, that a scientific theory conflicting with experimental data must be rejected, not the other way around (as some skeptics do). My main point was that readers and referees should not be distracted from experimental procedures by controversial theories.

Let me bring another illustration. It is the ICCF11 paper of G. Lochak and L.Uretskoev. They described a procedure by which the isotopic composition of a Ti wire was changed. What was the purpose of trying to explain a reproducible laboratory result with magnetic monopoles? This was not a good strategy. My articles would have a different title, for example, "A distorted isotopic ratio in Ti: how can it be explained?" I would keep my theory on the back burner, waiting for additional replications, and for the acceptance of my experimental claim. Why? Because the topic of magnetic monopoles is known to be controversial. An explanation based on a non-controversial theory, on the other

hand, would be worth mentioning. That would probably be helpful.

Linking an interesting experimental fact with a controversial theoretical topic is an open invitation for troubles, with referees and with other scientists. That was the main point of my message."

Ed Storm responded: "I agree with you, Ludwik. Experimental papers should not attempt an explanation unless the experiment was designed to answer a question theory raised. Speculation about an experimental result should be reserved for a paper devoted solely to that effort. Mixing theory and experiment invites trouble as you say. Unfortunately, people seem to be unable to restrain themselves in trying to explain rather than simply reporting."

This prompted me to post a short observation: "Why are people unable to restrain themselves? Because we were trained to think that science is much more than factology. And this is true." Elaborating on this Ed Storms wrote:

"The problem is in our genes. The human species has a need to explain. We can't help it. This allows great progress but also creates the conflict in all subjects because we take our explanations too seriously as the truth. In fact, the explanations are very seldom correct much less being the truth. Unfortunately, our species was also given an ego that has no humility. Consequently, most people will defend their chosen explanation to the death no matter how simple-minded the explanation might be. You can see this process operate most clearly in religion and politics. Science tries to overcome this problem with limited success."

Appended on July 29, 2009
A CMNS researcher posted a message about hypothesizing. Responding to this message I wrote: " (1) What is a scientific HYPOTHESIS ? It is neither a theory nor an experiment. It is a preliminary ASSUMPTION made by a scientist. Some call it intuitive thinking, others refer to it as a speculation, educated guess, hand-waving, etc. (2) I think that linking our field with controversial assumptions, such as hydrinos, is likely to result in more harm than good (at this stage)."

Scientific method in physics consists of techniques to investigate natural phenomena. Does a particular reported phenomenon exist? What is known about that phenomenon? How to explain it in terms of truth already accepted by scientists? The first two questions are addressed by experimentalists; the third is addressed by theoreticians. Experimental data are validated in terms of reproducibility. Theories are validated not only on the basis of logical (mathematical) correctness but also on the basis of specific predictions verified in reproducible experiments. Some theories are closer to reality than others.

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376) Arata type experiments

Ludwik Kowalski

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September 17, 2009

Introduction
Arata and Zhang persentation, described in (1), took place in May 2008. A recent paper by Kitamura et al (2) describes attempts to replicate experiments of Arata and Zhang. Actually, their paper is more than a replication. Arata's experiment did demonstrate simultaneous generation of excess heat and helium. But no quantitative results were produced (how much thermal energy and how much helium). Kitamura's paper does provide information about the amount of thermal energy but no information about helium. Hopefully amount of helium produced inside the Pd powder will be measured and results will be reported at the upcoming CMNS conference. If helium is the "nuclear ash" from "cold fusion" then the amount of excess energy per atom of created helium should be close to 24 MeV. Quantitative correlation between excess heat and helium has been reported by several investigators, for example, by Mike McKubre et al (3).

Arata's Experiments
As mentioned in my item #23 (at this website) Arata used the powdered palladium. Initially the powder was placed into a cavity of a palladium cathode. The deuterium atoms, released during electrolysis, migrated into the cavity through the thin wall of that double-structured cathode. This project was summarized in (4). In more recent experiments (1) the powder (mixture of Pd and ZrO_2, an alloy of Zr+Ni+Pd, or pure Pd) was placed into a large cell. The cell was first evacuated and then exposed to pressurized gas. Excess heat and helium were produced when the gas was pure deuterim; these byproducts were not produced when the gas was common hydrogen.

Presence of excess heat was inferred from a small difference of temperature (about 0.5 C) between the inside and outside the cell. That difference of temperature remained constant for about 2500 minutes (after the powder was saturated with deuterium). Presence of ^4He--most of it in the powder and not in the gas--was demonstrated by using a high-resolution mass spectrometer. The current due to ionized ^4He was about 25 pA. This number, unfortunately, cannot be used to calculate the number of helium atoms. To accomplish this one must know the R ratio (the number ions reaching the mass spectrometer's collector divided by the number of helium atoms produced during electrolysis.) Suppose R=1.0, which is highly unrealistic. In that case the charge collected in 2500 minutes would be $25*10^{-12}*2500*60=3.75*10^{-6}$ coulombs, corresponding to $3.75*10^{-6}/1.6*10^{-19}=2.34*10^{13}$ produced atoms. The $1.6*10^{-19}$ is the net charge of a single helium ion in the spectrometer.

How much excess heat would be produced if 23.8 MeV were released with each helium atom? The answer is 89 joules. The corresponding power would be 0.00059 watts. In reality R was probably much smaller than unity, corresponding to more atoms and to higher power.

Kitamura's Experiment
This experiment (2) was similar to that of Arata and Zhang. But the heat generated in the cell was measured by using a flow calorimeter. Three kinds of powder were used: pure Pd (0.1 micron grains), Pd-black and a mixed oxide of Zr and Pd. The highest heating power, close to 0.1 watts, during about 800 minutes, was for the mixture of Pd and Zr oxides. It is significant that excess heat was produced when deuterium was used; it was not produced when ordinary hydrogen was used.

How many atoms of helium would be produced during that time if all excess heat was due to D+D nuclear fusion? The answer is $1.3*10^{15}$, or about as little as in one cubic centimeter of normal air. Is this detectable? Probably not. But I am only guessing. I agree with authors that "it is crucial to confirm the phenomenon of heat and He generation with fully quantitative reliability." But is this possible when excess heat is generated for less than 1000 minutes, at the rate of about 0.1 W? The answer depends on the limit of sensitivity of the helium detection method.

Nuclear or not nuclear?

Absence of a nuclear signature, such as "helium ash," suggests that a chemical fuel of some kind might be responsible for the measured excess heat--thermal energy released in 800 minutes was close to 0.1*800*60=4800 joules. This is about the same as the amount of heat released when 0.1 grams of gasoline is burned. But absence of a chemical fuel in 99.5%-pure gas is not the only argument against chemical origin of excess heat. All possible artifacts, except deuteron-specific, can be ruled out; excess heat was not generated when ordinary hydrogen, rather than deuterium, was introduced into the cell.

An attempt to detect neutrons and gamma rays was made, but as stated in (2), results were negative. What other nuclear reaction, besideds cold fusion, could have produced excess heat without emitting detectable neutrons or gamma rays? I am pleased that the article was published in a very prestigeous mainstream journal.

References

1) Jed Rothwell and Ed Storms, downloadable from:
http://www.lenr-canr.org/acrobat/RothwellJreportonar.pdf

2) Akira Kitamura, Takayoshi Nohmi, Yu Sasaki, Akira Taniike, Akito Takahashi,
Reiko Seto, and Yushi Fujita; "Anomalous effects in charging of Pd powders with high density hydrogen isotopes; Physics Letters A 373 (2009) 3109-3112.

3) Michael McKubre, Francis Tanzella, Paolo Tripodi, and Peter Hagelstein; "The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd Systems; Evidence for 4He and 3He Production;" 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy. (Also see Section 3.1 in http://www.lenr-canr.org/acrobat/Hagelsteinnewphysica.pdf)

4) Mike Carrell; "Arata and Zhang's Cold Fusion: Excess Heat and Helium Production;" Infinite Energy, Issue 18, 1998 (Also see Section 3.1 in (downloadable from http://www.infinite-energy.com/images/pdfs/arata.pdf))

Added on 11/21/2009

Commenting on the above, Ed Storms wrote: "Your comments about the heat having two possible sources is correct, but incomplete. All heat that is generated by a CF reaction can have 5 sources.

1. Heat from fusion,
2. Heat from other exothermic nuclear reactions,
3. Heat released when the NAE is created, (This creation process must be exothermic.)
4. Chemical heat not related to CF.
5. Error

The measured relationship between energy and amount of He shows that source #1 can be a major source of heat. However, the other sources can operate when conditions do not cause fusion. As we understand the process better, we need to more carefully determine the amount of energy caused by each of these sources. Such values will go a long way to determine which theories are correct and which are only imagination."

The term NAE, invented by Ed, stands for Nuclear Active Environment. Yes, to make a new kind of nuclear energy commercially useful the cost of released energy must be lower than the cost of required NAE. But scientific understanding can be made when this condition is not satisfied. Consider the NAE in a tokamak; it is extremely costly. The break-even point, for a sustained tokamak operation, has not bean achieved. But hot fusion scientists continue to believe that a lot can still be learned from very expensive experiments. That is a healthy attitude. Decisions about how

much money to spend on research are not made by scientists; each individual scientist wants as much money as possible, for his or her project. Decisions on what fraction of GNP should be spent on research, and how much money should be allocated on different projects, should be make collectively.

Added on 11/23/2009

It is interesting that the CMNS list is sometimes quiet for weeks, and sometimes intensive. It did become intensive in the last last week. The topic was p+p fusion. Due to the energy and momentum conservation, this two-body reaction In stars is extremely slow; it must be proceeded by the slow p+e conversion of p-into-n. But it can be direct in the deuterium lattice; where surrounding atoms act as a large third body. One researcher reminded us of claim (made by Mills in 1991) that measurable excess heat was produced in a cell whose electrolyte was based on ordinary water. The cathode of that cell was nickel, as in Oriani-type cell I am using. That is what caught my attention. In a subsequent message I wrote:

The amount of energy released per D atom would be:

[(2*1.007852-2.0141022)]*931.5 = 1.49 MeV.

I am using the $E=m*c^2$ formula with $c^2 = 931.5$ MeV/amu. Suppose excess heat from the CMNS p+p reaction is generated for 1000 hours at the rate of 0.1 W (or 100 hours at 1 W, or 10 hours at 10 W, etc). Then the total amount of produced energy would be

$0.1*1000*60 = 6000$ joules $= 6000*6.24*10^{15} = 3.74*10^{19}$ MeV

The corresponding number of D atoms (the cold fusion ash) would be $3.74*10^{19} / 1.49 = 2.5*10^{19}$. Suppose the "ash" is dissolved in 100 cc of ordinary water (in the used electrolyte). That water contains $3.3*10^{25}$ molecules of H_2O. Concentration of heavy water (mostly DHO_2) is about 5 molecules per million molecules of H_2O. In other words 100 cc of ordinary water would contain $3.3*10^{20}$ atoms of D. This is much more than $2.5*10^{19}$ atoms in the nuclear ash. The task of identifying nuclear ash would thus be very difficult if the excess heat were only 6000 joules. But suppose the excess heat is 600000 J (10 W and 1000 hours, or 100 W and 100 hrs, etc.). In that case detection of nuclear ash would be easier because its relative concentration would be more favorable.

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378) Cavitational effect on radioactivity

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September 30, 2009

What follows was part of the previous unit #377. But the unit became too long and I decided to split it into two. This is the second half; the first half is still in unit 377

1) Fission versus fusion

Responding to a solicitation, I wrote a short essay (about 800 words) entitled "Nuclear fission, discovery of" for Salem Press. They will probably publish the essay in the book entitled "The Thirties in America." Unfortunately, I am not allowed to share the draft at this time. What I would like to do instead is to compose a longer essay entitled "Fission versus fusion," or something like this. Here are some observations; ; they were also posted at the private Internet list for CMNS researchers.

A) Nuclear fission, discovered in 1938, was quickly confirmed in several laboratories. Hahn and Strassmann's paper was published in January of 1939; the mechanism of the reaction was understood weeks later (by Lise Meitner, who coined the name "fission" and predicted about 200 MeV by event). This was at once confirmed experimentally (by Frish in Denmark, by Joloit Curie in France, and by Fermi in the US). The preexisting-accepted theory of Niels Bohr (the liquid drop model) was at once used to explain why fission is possible in the most massive nuclei, such as uranium (high values of Z^2/A). The discoveries of secondary neutrons, first by Joliot Curie and then by Fermi, were also made in January of 1939. The discovery that fission induced by slow neutrons takes place only in U-235 was also made in 1939 (Fermi and his collaborators). That opened the path to well-known applications, first military and then civilian. Spontaneous fission was discovered in 1940 (by Petrzhak in USSR). Note that these discoveries were made at universities, years before government-sponsored programs were created.

B) Basic nuclear physics facts behind hot fusion (exothermic nature of reactions, their cross sections etc.) have also been known for very long time, mostly from research conducted with low energy accelerators. The preexisting-accepted theory was able to explain the energy dependence of cross sections. Progress from knowing and understanding to the first practical application (hydrogen bomb) took about five years. But progress toward civilian practical applications (hot fusion reactors) continues to be very slow.

C) Discovery of excess heat, attributed to a nuclear reaction, took place twenty years ago. A large number of other CMNR discoveries, such as emission of nuclear particles and transmutation, were announced since that time. But the world is still waiting for a protocol yielding a "reproducible on demand" demonstration of a strong nuclear effect due to a chemical effect. The word "strong" is important because two kinds of nuclear forces are “strong” and “weak.” Strong forces are associated with nuclear reactions while weak forces are associated with beta decays. One kind of beta decay, capture of electrons from an atomic K orbit, does depend, to some extent, on the chemical composition.

D) What used to be called "cold fusion" is now called "condensed matter nuclear science" (CMNS). Other widely known names are “low energy nuclear reaction” (LENR) and “chemically assisted nuclear reaction” (CANR). New names specifically refer to “nuclear reactions,” such as emission of neutrons, protons, and alpha particles.

2) Importance of reproducibility: chicken-and-egg dilemma

Reproducibility on demand is essential in all scientific fields, especially in fields expected to influence technological applications. How can a proposed CMNR theory be validated? A theory is expected to make specific verifiable

predictions. How can predictions be verified without phenomena being reproducible?

The CMNS field has been controversial since the discovery of "cold fusion" was announced; it is still waiting for a generally accepted theory. There are many interesting CMNR reports, as described in numerous postings at this website, and in papers posted at www.lenr-canr.org. Some discoveries are spectacular. What the field needs, however is at least one experimental protocol yielding reproducible-on-demand results. This is essential. The discovery of "cold fusion" was announced two decades ago but experimentalists are still waiting for an accepted theory to guide them. Theoreticians, on the other hand, are still waiting for reproducible (reliable) experimental results to validate logical conclusions. CMNR investigators are fully aware of this chicken-and-egg dilemma. How can this vicious circle be broken? How can it be explained?

Responding to what I posted at the CMNS list, Dennis Cravens reminded us that theories do not have to have reproducible on demand effects. He wrote: ". . . For example, there are theoretical models of super nova and sun spots that do not require 'on demand' effects to be verified or established. All that is required is that the observations obey predictions whenever they happen..... " Another CMNS researcher gave the cancer treatments as an example. These are valid observations. But most technological applications, especially those used in generation of electricity, and in transportation, are based on reproducible phenomena. Cancer treatments would probably produce nearly the same results if all patients were nearly identical.

3) Speeding up the radiactive decay of thorium?

Another CMNS researcher informed the list about a recent claim (1) that the rate of decay of alpha-radioactive ^{228}Th was speeded up "by a whopping factor of 10,000." This claim was not independently confirmed in other laboratories. But shortcomings of the report were described in (2). The authors of (1) wrote, in the abstract:

"We show that cavitation of a solution of thorium-228 in water induces its transformation at a rate 10^4 times faster than the natural radioactive decay would do. This result agrees with the alteration of the secular equilibrium of thorium-234 obtained by a Russian team via explosion of titanium foils in water and solutions." They refer to Urutskoev and other researchers who reported "nuclear reactions induced by pressure waves."

I agree with (2) that a better experiment can now be designed. Here is my message about this, posted on the CMNS list: *"I just finished reading additional two papers (Cardone responding to Ericsson's criticism, and Ericsson responding to Cordone's response). Valid points are presented by both sides. The time seems to be perfect for designing a better experiment to verify validity of the initial claim of Cardone (and those who conducted similar experiments before). I would be happy to collaborate with someone who has experience with cavitation processing. My experience in working with nuclear detectors, most recently with CR-39, might be useful. Let me know."*

In an earlier message I wrote: *"Authors of the critical paper (Ericsson et al.) are probably not aware of many puzzling CMNS results. By the way, they did not try to replicate the Cardone's results; they described the shortcomings of the procedure, etc. Replication experiments conducted in different laboratories would tell us how reproducible the result of Cordone et al. are. Most people do not like to replicate experiments performed by others; they prefer to test their own ideas. This is one of the reasons that progress toward recognition of CMNR is very slow. Do you agree?"*

I read two additional papers (Cardone responding to Ericsson and Ericsson responding to the response). The fact that all four papers were published in Physics Letters, a prestigous mainstream journal, is significant. They also published the Kitamura et al. article this year. Does it mean the end of discrimination is approaching? I hope so.

4) More about speeding up the radioactive decay of thorium.

At first I thought that CR-39 played an essential role in backing the claim made in (1). But I changed my mind, after reading the paper again. I have a bunch of comments and questions about this.

a) Sizes of photographed areas (in their Figure 1) are not specified and track densities are not given. Why is it so?

b) The authors wrote that radiation from Th-228 was recognized on the basis " unmistakable 'star-shaped' look of

some tracks." This would indeed be the case if Th-228 was in a solid. But thorium dissolved in a liquid should not produce such stars.

c) The explanation has to do with the fact that emission of an alpha particle from Th-228 is followed by delayed emission of additional four alpha particles (from Ra-224, Rn-220, Po-216, and, most often Po-212) separated in time. The second alpha particle, from Ra-224, can be emitted five or ten days after the first particle. In a liquid source (thorium salt dissolved in water) separation in time also implies separation in space. That is why no "star-shaped" tracks are expected in a CR-39 kept in the liquid. Ra-224 atom will usually decay far away from where it was produced. But this is not true for a solid source. In a solid source sequentially emitted particles most often originate from the same point. That is why alpha particle tracks often form multi-prong star-like patterns in nuclear emuslsions. Why was at least one star-like pattern not shown in (1)? Circles in Figure 1 are not at all informative. Are they more informative in the printed article than on my screen? I do not know. In any case, referees should have asked for at least one better resolution photo.

d) The authors write that "the ratio of the number of traces and the number of solutions is therefore 3/4 for the reference solutions and 3/8 for the cavitated ones." This is not clear to me. What is clear, however, is that three tools available to support the claim--only one half of thorium remains in the vessel after 90 minutes of cavitation--were highly appropriate. The first tool was the magnetic spectrometer, the second tool was the NaI gamma spectrometer, and the third tool was CR-39. The magnetic spectrometer results were presented clearly. The gamma ray spectrometer results (showing that cavitation reduces the radioactivity of the solution by the factor of two) were not shown at all. The CR-39 results were not presented clearly, as far as I am concerned. A clear presentation would consist of track densities (or the total numbers of tracks) on all photos shown in Figure 1.

e) My recollection is that seven strong peaks are clearly identifiable in the gamma decay of natural thorium in equilibrium with its daughters. A change in the shape of the spectrum could provide information about which daughter, if any, is destroyed more efficiently by cavitation.

f) According to my rough estimate (see point 5 below), the pit density, due to Th-228 dissolved in water (at the 0.03 ppb concentration), would be between 12500 and 2500 tracks per square centimeter, after the exposure lasting 90 minutes. This is much higher than what is usually found on unexposed chips; typical radon and cosmic rays densities are close to 10 tr/cm^2.

g) The authors wrote that the ratio between the half-life of Th-228 (1.9 years) and duration of cavitation (90 minutes) is 10^4. This is correct. In the next sentence they conclude: "this means that cavitation brought about the reduction of Th-228 at a rate 10^4 times faster than the natural radioactive decay would do." I am puzzled; what additional information was used to justify the conclusion? Was the answer obvious to referees? Note that the factor 10000 is the ratio of rates at which Th-228 disappers (via natural versus artificially induced decay). It is deduced from the fact that one half of the initially present Th-228 is destroyed during 90 minutes of cavitation.

5) Appendix (Estimation of a blank chip track density)

Consider 1 cm^3 of the 0.03 ppb water solution (of Th-228 atoms) on top of the 1 cm^2 surface of CR-39. How many tracks to expect, after 90 hours ? The exact answer can be obtained by a Monte-Carlo code. A simple estimate, see below, is also possible. I will assume, to simplify calculations, that only Th-228 is able to form track. In other words, I will ignore alpha particles emitted from daughters of this isotope.

a) 1 cm^3 of water contains 3.3*10^22 molecules; the number of Th-228 atoms in it is:

(0.03/10^9)*(3.3*10^22)=10^12 atoms.

b) The range of alpha particles in water is about 40 microns, or 4*10^-3 cm. Only some alpha particles emitted from the 40-micron layer of solution will be able to create pits on the CR-39 surface. Others will be either absorbed in water or be intercepted at a too large angle.

c) How many Th-228 atoms are in the 40 micron layer? The answer is

N=(10^12)*(4^10^-3)=4*10^9

d) The activity A (alpha particles per minutes), of the layer is N*L, where L is the probability of decay per unit time.

L is given by ln(2)/T=0.69/T, where T is the half-life. For Th-228 T=1.9 years, or 10^6 minutes. The probability of decay per unit time is thus

L=0.69/10^6=6.9 *10^-7 per minute

e) The activity A of the layer (alphas per minute) is

A=N*L=(4*10^9)*(6.9*10^-7)=2760 alphas per minute

The number of alpha emitted in 90 minutes is close to 250000. Suppose that only 5% of these particles are able to produce tracks. This is probably a reasonable assumption. In that case the pit density would be 12500 tracks/cm^2. The density would be 2500 tracks/cm^2 if only 1% of alpha particles emitted from the 40 micron layer were able to produce tracks. And it would be 250 tr/cm^2 if the fraction were only 0.1%. [P.S. reflecting about this again, I think that 0.1% is more realistic than 1%. Fortunately, even 0.1 % would be OK, from the point of view of feasibility.

6) Reading the first criticism of Cardone (1) by Ericsson (2)

a) The following observation of Ericsson et al. is worth thinking about. They wrote: "if the decay of ^{228}Th is actually accelerated as (possibly) claimed during the cavitation, and such decay can be registered by the CR39 detectors, then the detectors monitoring the cavitated solutions should not show fewer but four orders of magnitude more events." That would not be true if detectors were placed into the vessel before cavitation or after cavitation (not during cavitation). Track densities after cavitation would be two times lower than before cavitation, for the same exposure time. I agree with (2) that the reported ability to identify decays taking place during cavitation is highly questionable.

7) Reading the first reply of Cardone (3) to Ericsson et al.

a) Cardone et al. think that "the main shortcomings of the criticism by the Swedish authors are due to their omitting of inserting our experiment in the wider research stream of piezonuclear reactions, and to the statistical analysis they used, which does not comply with the rules generally accepted for samples with small numbers." Relevance of earlier studies, in particular to investigations of Russian scientists, was well described in (1). That is why I would also not discuss it in an article written to criticize the methodology used in (1). That would only be important if the same mistakes were made by other authors.

b) Referring to their gamma counter results, Cordane et al. conclude that they are dealing with processes in which transformations of elements take place "without significant emission of gamma radiation." That is a very important clarification. It means that absence of gamma rays is not a valid argument against absence of nuclear reactions. But it has nothing to do gamma rays emitted from the solution, either before or after cavitation. A gamma spectometer would show what fraction of radioactive isotopes is destroyed, and which gamma emitter are destroyed more effectively. The authors of (1) are correct that they are not the first to claim reality of nuclear processes without emission of gamma rays.

c) Photos in reference (3), unlike those in reference (1), show readable scale lines near the corners. Now I know that photographed areas were 2.9 by 2.2 mm. The left photo shows that white dots, presumably tracks of alpha particles, are localized in a nearly circular area whose diameter is about 2 mm. That is not what I would expect to see on the surface of a CR-39 immersed in an alpha-radioactive liquid. Why is the distribution of tracks not uniform? I wish the etching conditions of CR-39 were specified (solution, its molarity, its temperature, and the duration of etching). This would help me to decide whether or not white dots are tracks due to alpha particles. My experience in working with CR-39 detectors suggests that thin and long lines are not tracks of alpha particles. I have no idea how to explain chain-like clusters shown in the left side photo. The right side photo (the chip was outside the vessel) does not exhibit such

clusters.

d) A little later the authors refer to the "inhomogeneity of the initial concentrations of all used (both cavitated and reference) samples." Why a thorium salt dissolved in water would not be distributed uniformly in the solution? Unfortunately, no information about non-uniformity is provided. Does it have anything to do with the nature of the thorium salt? If so then what was its main composition? What was its purity? Where was it obtained?

8) After reading the second reply of Ericsson (4) to Cardone (3)

a) It is clear to me now that the question about non uniformity of the thorium distribution is related to the first comment in reference (4)--how do the authors of (1) know that samples of the same initial volume contain the same initial amount of thorium? A large systematic error can indeed be made when the distribution of thorium is not uniform, for example, then the liquid is not stirred. My impression was that cavitation is like stirring.

b) The authors of (4) also feel that not enough information was presented about CR-39 methodology.

c) My impression, in looking at the two pictures in (3), was that the left one is in the water containing thorium while the right one is in the air, outside the chamber. But I was apparently wrong; the caption is clear that the left one is in water. All the questions I asked were about the left picture (see point 7 above). Now the situation is even more obscured. Why so many tracks were found in CR-39 immersed in the ultra-pure water?

d) I agree with (4) that arguments presented in (1) and (3) are not very convincing, and the experimental procedure can be improved. Only one of three available tools, the magnetic mass spectrometer, was used to directly support the extraordinary claim? Why was the gamma rays spectrometer not used at all to compare levels of radioactivity before and after cavitation? Why so little information was presented the CR-39 results?

e) A better experiment is worth performing, to obtain a definite yes-or-no answer about the claim made by Cardone et al., and by Russian investigators who they quote. The important point is that Ericsson et al. did not perform any experiments; they only stated that results could possibly be due to trivial mistakes. Possible mistakes are not demonstrated mistakes. The topic is too important to declare the article be worthless. Yes, it would be better if Cardone et all were more experienced, and if referees were more demanding. But that does not exclude a possibility that the claim is valid.

f) Let me share a trivial mistake suspected by Fabrice David, a French CMNR researcher. In a message posted at our private Internet list he wrote about a possibility that Th-228, in the experiment of Cardone et al., is redistributed rather than destroyed. He wrote: "sonication induces cavitation, and cavitation induces reactive chemical species like ozone or hydrogen peroxide. Thus, thorium can change its oxidation state, and (perhaps) stick to the glass surface." That would indeed produce the results reported in (1). Fortunately, CR-39 chips can be used to verify whether or not thorium that is no longer in the solution is on the glass, or in another suspected region, such the cavitaror.

g) The following statement from (1) puzzled me. "We measured the ionizing radiation in the empty Duran vessel both before and after cavitation. The radiation measurements were carried out by means of two Geiger counters with mica windows (one of which equipped with an aluminum filter 3 mm thick), and of a thallium activated, sodium iodine gamma-spectrometer. The results turned out always compatible with the background level." Why did they measure radiation inside an empty vessel; I would measure it in the vessel with the solution, first before and then after cavitational treatment (in order to show that the level radioactivity was significately reduced by cavitation). Is it possible that they were thinking about a possibility mentioned by Fabrice? Nothing of that kind is mentioned in (1) and (3).

h) The authors of (1) do not provide information about what mass was selected to measure the decrease in the concentration of thorium. Were the measurements based on the mass 228 peak only or were they based on several peaks? My guess is that only one peak was selected. The gamma spectrometer should display several energy peaks, some due to Th-228, others due its daughters. It is very unfortunate, that no gamma radiation spectra were shown in (1). Such spectra, as indicated above, could provide very valuable information about changes resulting from cavitation.

The CR-39 will record alpha particles from both Th-228 and its daughters (assuming Th-232 does not contribute to what is observed.) For that reason the measured reduction of radioactivity, resulting from cavitation, might depend on which of the three methods is used. All three methods, however, should produce the the same result if transmutations induced by cavitation are not isotope-specific.

i) There is a lot to learn about the claimed effect, after it is shown to be reproducible on demand. Unfortunately, I have zero experience with cavitators. Otherwise, I would have used all three instruments described in (1), actually a much older version of a mass spectrometer (not with a laser beam). Assuming cavitators are not too expensive, I am suggesting that reproducibility on demand is tested by using CR-39 detectors only. In that way many researchers, even students, could participate in gathering information about reproducibility.

References:

1) *F. Cardone et al. /* Physics Letters A *373 (2009) pages 1956–1958*
2) G. Ericsson et al. */* Physics Letters A *373 (2009) pages 3795–3796*
3) *F. Cardone et al. / Physics Letters A* 373 (2009) *pages 3797–3800*
4) G. Ericsson et al. */ in press (2009)*

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379) A rejected manuscript.

Ludwik Kowalski

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October 2, 2009

1) My manuscript with comments on new SPAWAR results, submitted last spring, has been rejected today. The pdf version of the manuscript can be downloaded from

http://csam.montclair.edu/~kowalski/cf/379spawar2.pdf

I hope it will be useful to those who are studying tracks observed in codeposition experiments.

2) Here is my general observation:

Conclusion
SPAWAR experimental CR-39 results are reproducible, as demonstrated in The Galileo Project [10]. Participants disagreed about interpretation of results [11,4] but everyone who used the SPAWAR protocol observed the same kind of pits as reported in [1]. That is very significant. It means that an acceptable explanation of reported facts will be found, sooner or later. Experimental data, presented by competent scientists, must be taken seriously, even when they seem to conflict with generally accepted ideas. The accepted idea, in this situation, is that a nuclear process cannot be triggered by a chemical process, such as electrolysis. The author of this note believes that rejections of claims, made by Ph.D. scientists, who publish regularly in refereed journals, should not be made only on the basis of conflicts with currently accepted ideas. They should be made on the basis of hard evidence. Experiments supporting rejections should be examined as critically as those validating unexpected claims. What is needed is an organized attempt, similar to The Galileo Project, to replicate the protocol in which the detector is located outside the electrolytic cell.

3) The letter of rejection (see below) contains extracts from comments made by three referees.

Dear Dr Kowalski,

Your manuscript: “Chemically-induced Nuclear Activity or an Illusion ?” has been carefully considered by the referees of The European Physical Journal-AP. As you can see on the enclosed reports, the referees have raised serious concerns regarding its suitability for publication.

I therefore regret to inform you that your manuscript has not been accepted for publication. Thank you very much for having submitted your article to our journal and I hope that you will nevertheless consider EPJ-AP for the publication of your future articles.

****
Referee 1:
This paper criticizes a work done by P.A. Mosier-Boss, S. Szpak, F.E. Gordon, and L.P.G. Forsley showing the presence of pits in CR39 foils during electrolysis. The point raised by the author is the interpretation of the pits diameter. According to the original work, they estimate that they are due to alpha particles, whereas the author of this paper shows that they are due to heavier particles.

This paper does not object the nuclear origin of the pits, but only the origin of.

Referee 2:
The paper represents a criticism on the works of SPAWAR group on in-situ charged particle detection with the CR-39track detectors during electrodeposition of Pd, published in EPJAP earlier {see ref. [1-3]). Desipte the fact that, in my opinion, these works are really deserve to be critisized, the manuscript AP09225 is not suitable for publication in EPJAP in its present form. Indeed, the presented paper brings nothing new either from theoretical, or in terms of experimental point of view. No new experimental data are presented. So, the graph in Fig.1 is representing a rebuilding of the experimental data obtained with Van DeGraaf accelerator for protons and already published by A.G.Lipson, A.S. Roussetski, and E.I. Saunin. ? Analysis of #2 Winthrop Williams' CR-39 detector after SPAWAR/Galileo type electrolysis experiment?. Proc. of 8th International Workshop on Anomalies in Hydrogen / Deuterium Loaded Metals. 2007. Sicily, Italy. (in the same book as ref. [9], cited by author).

Thus, this paper cannot be considered as original one. Moreover, the paper is written in the style of popular article (not acceptable for serious publication in scientific journal). In a much shorter version this paper could be considered for submission as a Comment on the works [1,2,3] by P. Moseir-Boss et al, recently published in EPJAP. However, Kowalski has already published his similar comment on some of those papers in EPJAP {see ref. [5] of this reviewed paper). Therefore, it is clear that presented paper does not correspond to the EPJ standards. This is why, the presented manuscript cannot be published in EPJAP.

Referee 3:
In fact I spend some time trying to find out what are the arguments and real conclusions. They speak about nuclear effects, but the observation looks like a simple electrolyses. I showed the image to a chemist: he said all our bathes look like that. I just suggest to submit the article to a chemist journal.

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380) ICCF15 will start tomorrow in Rome

Ludwik Kowalski

Montclair State University, New Jersey, USA
October 4, 2009

1) The 15th International Conference on Cold Fusion, ICCF15, will start tomorrow in Italy. The conference website is

http://iccf15.frascati.enea.it/

Going there I just downloaded the abstracts. I have no idea when the proceedings (with full articles) will be published. Proceedings from the ICCF14, which I attended (Washington DC, October 2008), have not yet been published. What follows are the notes I made by browsing throug selected ICCF15 abstracts, quickly.

2) I. Dardik et al. referred to their already-known spectacular excess heat result (experiment 64). The electric input power was "less than 1 watt," the thermal energy output power was "up to 30 W." The total energy gain was "approximately 25." That record result was obtained in the open electrolytic cell. New experiments are performed in close cells, containing recombiners. New results "re-confirm the validity of the Energetics open cell results."

3). Letts et al., are studying low power excess heat influenced by the laser illumination of the cathode. Two laser beams (of different frequency) are used. Theoretical aspect of this approach was presented by Peter Hagelstein. This was at the University of Missouri LENR Seminar in May 2009. All presentations at this seminar are very interesting. Link to them are at

4) Y. Iwamura et al.: " Transmutation reactions of Sc into Pr has been confirmed" by using X-ray fluorescent spectroscopy. The item below shows that results are not always reproducible.

5) Bazhutov et al. calibrated CR-39 detectors with hydrogen and nitrogen ions with energies up to 5 MeV. This was done in connection with new investigations of the erzion model.

6) K. Grabowski et al.: "Thus, we concluded that the transmutation of Cs to Pr when deuterium permeated MHI's multilayer structure could not be firmly established, as contamination during the foil production or during the foil analysis could not be ruled-out."

7) A. Lipson: "The presented data on observation of real nuclear signatures accompanying deuterium loading/deloading in metals, open the way to convince physicists in nuclear origin of excess heat effect."

8) Y. Toriyabe et al. bombarded Li with deuteron beams (energies 10 to 80 keV). Preliminary experiments show that acoustical cavitation affects emission of alpha particles. "The reaction rate varies in cycles which synchronize with the frequency of the ultra sonic wave, although the enhancement of the reaction rate is not so large."

9) A. Frumkin et al. observed nuclear projectiles (3 MeV protons, 1 MeV tritons and alpha particles of higher energies.

10) A Roussetski et al. describe a new low-energy (up to 0.05 MeV) ion beam installation HELIS, in Moscow. This accelerator of ions of light elements (with atomic number in the range Z=1-54) is likely to play an important role in CMNS investigations. The deuteron current density can be as high as 2 A/cm^2.

11) S Tsvetkov et al., detected emission of neutrons (at the rate of 10^5 per second) correlated with excess heat. Neutrons were emitted when deuterium was loaded into titanium.

12) A. Takahashi discusses nuclear reactions which result in emission of neutrons.

13) V. Kirkinskii, also reports emission of neutrons from titanium loaded with deuterium. The counting rate (57 +/- 13) exceeded the background by the factor of about 1.8. The emission rate is estimated to be between 100 and 1000 per second.

14) 14) A. Roussetski et al. referring to a SPAWAR type experiment performed at SRI, presented "evidence for fast neutron emission (En ~ 2.0-2.5 MeV) during SRI's SPAWAR-type PdDx electrolytic co-deposition experiment." Their experimental findings, already reported in Catania, show that dominant CR-39 tracks were due to protons (presumably recoiling protons from collisions with neutrons) and not to alpha particles of very low energy.

At the May 2009 seminar (see point 3 above) Pamela Boss said that majority of tracks they observe are due low energy alpha particles. Larry Forseley, also from SPAWAR team, thinks that tracks observed, especially on the back sides of CR-39 chips, are due to neutrons. No SPAWAR talks or posters appear on the list of ICCF15 presentations. Frank Gordon, the leader of SPAWAR team, made an excellent review of two decades of their research. How can anyone disagree with their experimental evidence (published in 23 pier review papers) that signatues of nuclear processes do occur when their codeposition protocol is used? Or how can anyone disagree with his last last remark, that progress toward understanding of CMNS phenomena would be much faster if research were coordinated (all researchers performing the same experiments at the same time)?

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381) Free ICCF4 proceedings

Ludwik Kowalski

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October 6, 2009

WHAT FOLLOWS IS THE MESSAGE I POSTED ON THE CMNS LIST TODAY. WILL IT RESULT IN SOME ACTION? THIS REMAINS TO BE SEEN.

1) Jed Rothwell deserves to be appreciated many things, including making proceedings of the 4th International Conference on Cold Fusion freely available at:

http://www.lenr-canr.org/News.htm

2) I wish proceedings from all conferences were available in the same way. Perhaps Google will do this; I read somewhere that they plan to digitize all rare valuable books. I suggest that someone who has all volumes contacts Google and offers the books to be immortalized. That would be a very valuable contribution to history of science.

3) The pdf files (four volumes) are searchable. For example, I opened Volume 3, entitled "Nuclear Measurements," and used the FIND command (from the EDIT menu). In the search box that appeared I typed CR-39 and pressed RETURN at my keyboard. The document scrolled on my screen to the paper of JIN Shag-xian et al. Figure 3 of this paper shows tracks of nuclear projectiles in the CR-39 detector. I did not know that CR-39 were used in CMNS research in 1994. The reported track density was 3*10^5 per square centimeter. Only a small fraction of tracks had the same diameters as those due to alpha particles from americium; most diameters were much smaller.

Here is the abstract: "The experimental studies of YBCO-D system indicated that YBCO high temperature super-conductor (HTSC) was shown to have a similar effect on deuterium absorbability and anomalous nuclear effect like palladium('). We found that Y1Ba2Cu307-6 could absorb deuterium at normal temperature and forms D,Y~Ba&usO.r-s. We also found that the deuterated YBCO could produce high energy charged particles far larger than background. The influence of the absorbed deuterium on the characteristics of YBCO HTSC and the mechanism of the anomalous nuclear effect are not clear and needed to be further studied."

4) This was published 15 years ago. What is the current status of investigations of particles emitted from superconductor's? I haven't seen any recent reports about this phenomenon. Before continuing my search I would like to make another suggestion. We already have Storms' book summarizing scientific aspects of CMNS research

http://www.worldscibooks.com/physics/6425.html

What we also need is a book about CMNS history, preferably written by another high caliber researcher. I am thinking about something that all scientists can read with understanding, about a mixture of semi-popular scientific summaries and sociological aspect of research. For example, how were announcements of different discoveries received by other researchers? Were they confirmed by other CMNS researchers or were they ignored? How can this be explained? Why do people who were very active long time ago stopped contributing? Such questions can probably be answered only by someone who was part of CMNS since the very beginning. Many of these people will not be around in a decade or two.

OCTOBER 7, 2009. NO ONE REPLIED SO FAR. THAT IS NOT SURPRISING; MOST ACTIVE CMNS

RESEARCHERS ARE AT A CONFERENCE IN ROME (ICCF15).

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382) Data, facts, laws and theories

Ludwik Kowalski

Montclair State University, New Jersey, USA
October 17, 2009

The abbreviated version of the text below was posted on the CMNS list yesterday.

What follows was prompted by Nicholas Wade's New York Times Book Review (October 11, 2009). My comment is about scientific DATA, FACTS, LAWS and THEORIES.

Cognitive Structure of Science

a) Any recorded piece of information, about a natural phenomenon, is DATA. It can be the result of an observation or an experiment; it can be qualitative or quantitative.

b) A piece of data recognized as valid, by most practitioners in a given field, becomes a FACT. Laboratory facts are confirmed data; any scientist using the same protocol is expected to obtain the same data; at a specified level of reproducibility. Science is not mathematics; that is why the level of reproducibility, for example 90%, should be reported.

c) A LAW is a generalization of facts. I am thinking about Kepler's Laws (how planets move), about Mendeleyev's Law (how elements are ordered in a chart), Faraday's Law (how changing currents can be induced), etc.

d) A THEORY is an explanation of a law. I am thinking about Newton's gravitational theory, about Maxwell's equations, about quantum mechanics, about Heizenberg's principle, etc. Distinctions between laws and theories (and between facts and theories) are worth recognizing. Unfortunately, this is not always done.

e) Theories are based on assumptions which may or may not be valid. Assumptions can be based on facts, on laws, or on intuition.

f) Scientific theories are validated not only on the basis of their logical (mathematical) correctness but also on the basis of their ability to guide to discoveries of unknown facts. Scientific theories evolve; some theories are more powerful, more general, more useful and more elegant, than others. A theory is not acceptable to scientists unless it is susceptible to refutation. In other words, a scientific theory must be falsifiable.

g) The concept of 'absolute truth,' often used by mathematicians and theologians, does not belong to science. Wade wrote: "A theory, however strongly you believe it, inherently holds a small question mark. The minute you erase the question mark, you've got yourself a dogma."

h) An hypothesis is a tentative idea to be confirmed. It is a common instrument of human thinking. Hypothesizing is used by all those who collect data, who turn data into facts, who generalize from individual facts, and by those who explain facts of nature. Tentative assumptions are made by all thinking people, not only by physical scientists.

i) To avoid confusion, scientists try to define their terminology. The term ‘theory,’ for example, does not mean the same thing as the term ‘hypothesis.’ But non-scientists often confuse these two terms. In common language the words ‘theory’ and ‘speculation’ are often used as if they were synonyms.

j) ‘Force,’ ‘energy,’ and ‘power’ are commonly used interchangeably by non-scientists. Novice physics students often find this confusing.

Several researchers commented at once; the above text contains additions made to accommodate comments. The entire text shown above was posted this morning on the Phys-L list for physics teachers. This generated three interesting replies, as shown below in blue.

Teacher 1:
I also read Wade's article.

I disagree; I don't think a meaningful distinction can be drawn between fact, theory and law. For starters, Newton's laws are wrong but Einstein's theory is correct. Kepler's laws are only approximate. So we don't use these terms consistently (perhaps we should but it is too late, we don't).

Second, facts are contingent on theory in at least two ways. The fact that we observe current flow is contingent on our theories of how ammeters and voltmeters work. There are few observations made today that don't depend on starting with some assumed (theoretical) knowledge. This is ok, we can't test everything at once so we assume some things are true while we test other things. Theories also tell us where to look for facts. We don't just go out and start measuring stuff to see if there is some correlation somewhere. For example the data supporting black holes only makes sense because we have a theory of black holes. We have to know to look for gravitational lensing before we can discover the fact that black holes exist. It doesn't make sense to try to separate facts from theory.

I think Wade in the NYT book review was trying to get at how science can be right at the same time that it can change over time. Here is a better way to think about it. We are perfectly rational to believe (and I mean really believe) that something is true if all the data supports it and, upon trying to find counter evidence we find none (this is important- finding data in support is often easier but one good counter example destroys the theory). It is equally rational to change your mind if you get better information. I really believe my breakfast cereal is nutritious. Then I read in the news about a contamination problem and change my mind. I am not being silly or irrational, rather I am being very rational.

Now, any good scientific concept is fruitful- it suggests new ideas to try. So sure, the testing process is never done but just because you can think of some new test doesn't mean you should abandon the concept. In the history of science we almost never abandon a theory or law until we have something better to replace it. This also is rational; a well established theory is worth keeping, even if it might suggest new ways to test it.

We should believe (I mean really believe it is true) in evolution as the best truth that is available. Until we have evidence to the contrary. And it also makes sense to keep testing it, even while believing it is true.

Oh and hypothesis are a nice idea but I don't think very many scientists actually operate that way. We start with something that looks interesting or puzzling and start playing around with it; our intuition outruns our rational mind at first. Usually after the fact we can come up with a hypothesis but more often we start with a hunch or a dream or an idea. The guy who discovered the benzene ring didn't have a hypothesis initially- he dreamed of a snake rolling down a hill by biting its tail and forming a hoop. But it doesn't matter where we get ideas from (hypothesis or dreams). What matters is if we can back them up with physical data and, after trying hard to knock them down, they are still standing. I have noticed that biology papers tend to state a formal hypothesis but I don't see this done in many physics papers.

Teacher 2
I disagree with almost all of Wade's list and agree with Teacher 1's criticisms.

One more point: According to my understanding of the term, a hypothesis may be made without regard to whether it is true or not. Sometimes a hypothesis is assumed, expected, or hoped to be true but sometimes not. In particular, at the start of a proof by contradiction, we make a hypothesis that we expect to be entirely false.

To repeat: offering a hypothetical statement says *nothing* about the truth, falsity, probability, improbability, etc. of the statement. For more on this, see

http://www.av8n.com/physics/hypothesis.htm

There are other words we can use, such as conjecture or supposition, when we wish to suggest some marginal or provisional belief in a statement. Of course at this point we are dealing in shades of gray, since all scientific laws are to some extent provisional.

Here's my take on truth, belief, and knowledge:

http://www.av8n.com/physics/truth.htm

I agree with KF that real-world day-to-day scientific activity is _not_ well described in terms of hypothesis testing. It is often the case that scientific results can _retrospectively_ be analyzed in terms of hypotheses supported and hypotheses rejected ... but this is usually not necessary or even helpful at the beginning or middle of a project.

It just cracks me up when people who have not ever done science, nor even seen it done, insist that "THE scientific method" equates to hypothesis testing. I wish non-scientists would refrain from telling scientists how to do science.

Teacher 3
Hi all- I've NOT read the NY Times article, but have read the postings. It's my undestanding that the accepted, currently-used term is "model". The take is, that it is too presumptuous to claim that we are seeking truth, as physicists our job is to predict the results of experiments. We do this by formulating models which, if successful, become "standard models." Thus one occasionally hears reviewers say "the standard model appears to be working embarrassingly well" - meaning that we expect that there is "physics beyond the standard model" that should reveal itself in precision tests of the standard model.

We know, of course, that there are observations that don't seem to fit within the framework of our standard models - we characterize these as "unexplained".

Currently, where the support dollars are going, the following are objects of attention:

*) Dark matter - what is it? Supersymmetry is a popular guess.
*) Dark energy - is it just Einstein's constant? Is there a Higgs that can be identified with a mechanism for giving particles mass (I claim that a well-defined particle is not needed for the Higgs mechanism)
*) What are the neutrino mixing parameters?
*) Does supersymmetry exist?
*) Do the weak interactions become strong at high energies?
*) Can accelerator designors find acceleration schemes that will permit the construction of small high-energy colliders.

There are also some questions left over from the 'sixties, but these are no longer popular.

Ludwik's Reply:
I hope that there will be more contributions to this interesting thread. I do not disagree with the above comments. Let me make one thing clear; I did not try to write an essay on the so-called scientific method. It is impossible to write such essay without omitting some aspects of scientific methodology. Furthermore, what one writes depends on the audience. In the CMNS field, which used to called “Cold Fusion,” a distinction between data and facts, as I defined these words, is very important; the amount of data is very impressive we are still waiting for recognized facts. In my

opinion, the most promising data should be identified and all efforts should be concentrated on turning these data into facts. Agreeing on the meaning of essential words is an important preliminary step for discussing scientific methodology of validation.

I hope that my description of four essential concepts, DATA, FACT, LAW and THEORY is clear. But I am not sure that the above four words are the best for the corresponding concepts. Which words would be more appropriate (leading to a minimum number of possible confusions)? .

I also like the term “model;” it is a synonym for “theory.” But I always add the adjective “theoretical,” to avoid confusion with a physical model, for example, of a building, a bridge, or airplane. The term hypothesis is part of common language and scientists are not forbidden to use it. A hypothesis is an assumption that something is either true or not true. Tracing the origin of assumptions might be interesting; but that is a different topic. Validations of scientific assumptions should not be based on their origins.

P.S. (10/18/2009)

M.B. who I encountered on another forum, asked a question about the so-called “sociological science.” I think she is a not-a-very-young journalist. She wrote: “I personally wondered what science is in poli sci.” Then she specified:

I have a Bachelors (1944) and already was skeptical of the "science" aspect. My motive to study such was more for the philosophy of international politics. Regardless, how does one measure? There are hints such as candidate polling (and George Gallup had U of Iowa connections.) Like nowadays, we studied wars and new peace pacts. Hardly a topic to quantify. As I watch what modern students study, it seems they take the courses to engage in jobs with government or the media. There is merit, but I question the science.

Something different might be said for economics. Of all the social sciences, here we have measurable data and I forgive those who call it science--the "dismal science" they say.

Sociology falls in an iffy category for me. It seems that studies on prison recidivism, for example, may use quantifying (therefore conclusion) methods.

I believe social sciences, balanced against physical sciences, have relevance. However, deep study requires more than a few studies which pit two human conditions against each other. Most issues relating to international affairs--war and peace, for example--relate mostly to economics. I guess that's why I was called an economic determinist, even before I was old enough to vote.

This is an interestiing topic. Sociologists, political analysts, historians, psychologists, economists, mathematicians , philosophers, etc. do often call themselves scientists. Many people thing that scientists are all those who use numbers. I am not sure that this is correct; the methodology of validation is probably a better yardstick, to distinguish scientists (as a group) from nonscientists. That topic, by the way, is likely be of great interest to most students.

Teacher 4:
I would assume the article in question is "Evolution All Around" criticizing the book " THE GREATEST SHOW ON EARTH The Evidence for Evolution" By Richard
Dawkins.

1) The review is quite reasonable and actually only differentiates between theories and facts. It says that Dawkins strays into saying evolution is a fact and not a theory or model. This looks like a very reasonable criticism of Dawkins. As far as I can see it makes no general definitions of the other words.

2) As to a law being a generalization of facts, that is not exactly how the term is used. Virtually all laws are equations or relationships which may be either general or specific. If you look at all of the laws they are generally equations such as Boyle's law, Newton's laws... Sometimes the equation is merely implied as a statement of a principle.

3) A theory is a consistent framework for relating various laws and facts so that one can make predictions about unknown situations. Sometimes you can also call a theory a model. But in no sense is it an explanation. The theory itself can often not be explained, but it can be used to generate predictions. If it can be explained that would be in terms of another theory and eventually you get to postulates or assumptions which have no explanations, and are sometimes do not even look reasonable, but they work.

4)

I numbered the paragraphs and eliminated the last one; it has nothing to do with the topic of our thread. The above message is a good illustration of disagreements resulting from imprecise terminology. My terminology of the four basic terms (data, facts, laws and explanations) has been introduced.; let me stick to it.

1) Evolution is a theory explaining many facts recognized by biologists. Dawkins, on the other hand, according to the above, states that evolution is a fact. Yes, I know that the term 'fact' can refer to different things in common language. That is why scientists must first agree on definitions of basic terms.

2) This paragraph is another illustration of the same thing. I suggest that the term "fact" should refer to validated data (accepted by most practitioners), not to anything else. Likewise, I suggest that the term "law" refers to nothing else than a generalizations of facts.

What is wrong with this approach? How else can we deal with imprecise terminology of common language? As I already mentioned, some people use the words "force," "energy," and "power" interchangeably. As physicists we introduced precise definitions of these terms to students, and we try to stick to these definitions. What is wrong with assigning precise meaning to the four concepts needed to discuss scientific validations? Three questions should probably be answered:

a) Do we need precise definitions of basic words used to discuss scientific validations?

b) Are the four concepts I identified (by assigning specific four words to them) indeed essential, as far as scientific validation is concerned?

c) Are the words assigned to these concepts appropriate? If not then what are better words?

P.S.
Note what I am referring specifically to scientific validations. This term does not cover as many topics as the often-rejected term "scientific method." I am not proccupied with generalizations about how discoveries are made (what comes first, etc.).

I believe that the four concepts I defined are essential, as far as scientific validation is concerned. But I suspect that better words can be chosen to name these concepts.

Teacher 5
I will admit that I cringed a bit when I saw a video in which Dawkins used that phrase, but if one looks at "evolution" as a term simply meaning "change over time," then it seems much more reasonable to assert that evolution is a fact. The observed fossil record is clear--by whatever method one can imagine the fact is that species have changed over time. Now when one extends the idea to "evolution by natural selection," then we have moved into the realm of theory (beyond hypothesis, since evolution by natural selection is a synthesis of an overwhelming body of evidence) or model by which we explain how the "fact" of evolution came about.

It isn't clear from Dawkins' comments if he does mean evolution in its more simple sense, or in its more complete and common meaning. I'm sure however, that if questioned about it, he would either modify his comment or clarify his

definition of evolution to conform to the standards we are discussing here.

His insistence on using the word "fact" is, I believe, more of a rhetorical device than a logical one. He is trying to emphasize that, based on the overwhelming evidence and the interlocking methods by which evolution is taken to occur, evolution by natural selection is as close to a "fact" as almost anything scientific can be. The idea is over 150 years old, and all of the evidence so far developed supports it fully. This is a rather long lifetime for a theory to stand in the modern world, where challenging a prevalent idea is a widely accepted scientific activity. After all, few scientists are going to go down in history for confirming Darwin's work, it is the ones who successfully upset that apple cart who will be remembered.

Teacher 4 again:
While the definition proposed for a fact aligns with common usage in science, the proposed usage of law does not. Just look at all of things called laws and in general they are relationships between variables. Everyone may propose a definition, but to be used it must be accepted by the community that uses it.

Historically the definition of a law has been taught as something at variance with the way in which it is actually used. The old definition of law as a verified theory still appears in many texts, but it is going away as it is completely out of alignment with actual usage. Come up with a list of laws, so we can find out how often it is not a relationship between variables. If there are no counter examples or they are few, then the definition that I have proposed is the common usage and should stand.

Language is not definable by logical rules, but relies on common agreed on definitions. Many arguments about "what is" are often just people insisting their definition is correct and the other person is using it wrong. Dictionaries now recognize that and are no longer purely prescriptive.

As to imprecise terminology there is no use telling students definitions that are at odds with how people in the field of study actually use the terms. What about the historians of science? How do they define the terms in question, or what definitions have they found tend to be prevalent now?

Ludwik's reply:
1) Each Kepler's law is an example in which the term "law'" means "a generalization based on facts."

2) What would be a better term for "a generalization based on facts." Should we say "Kepler's generalizations?"

3) Newton's law of gravitation, on the other hand, is not "a generalization based on facts." That is why I think it is better to say "Newton's theory of gravitation." It explains Kepler's Laws in terms of postulated F=G*M*m/r^2 .

4) Yes, what I am trying to do is full of traps. But is it worth trying? Please help me to choose better words, to make definitions more acceptable. I believe the four selected concepts are essential, as far as scientific validations are concerned. What names are better than DATA, FACT, LAW, and THEORY?

Teacher 6:

Not to take away from Teacher 5's point, which I agree with, but I wince a little bit whenever I hear appeals to the amount of "supporting evidence" for a theory without an accompanying assessment of the degree to which the theory may be *vulnerable* to evidence. After all, proponents of intelligent design can also point to overwhelming supporting evidence. The difference is that it is entirely inconceivable that any evidence could ever be found that is inconsistent with ID.

Teacher 5 again:
Valid point. I was thinking about the possibility of evidence against evolution that could be conclusive (e.g., finding fossils from widely different time periods co-located in the same rock layer), but decided to leave it out to keep my response short. It is certainly true that, even as a "fact" evolution is still subject to refutation by evidence, although, so

far, all the evidence found supports the evolutionary model we presently have constructed. ID [Intelligent Design], on the other hand, as John points out, is not susceptible to refutation and therefore cannot be considered a scientific theory in any sense. And furthermore, not only can it not be refuted in principle, it can also make no useful predictions about what we should expect to find, while the evolution model makes many of them.

Ludwik's comment:
According to point (f) in:

http://csam.montclair.edu/~kowalski/cf/382terms.html

"Scientific theories are validated not only on the basis of their logical (mathematical) correctness but also on the basis of their ability to guide to discoveries of unknown facts. Scientific theories evolve; some theories are more powerful, more general, more useful and more elegant, than others."

I will add another sentence "A theory is not acceptable to scientists unless it is susceptible to refutation. In other words, a scientific theory must be falsifiable.

The attribute of usefulness was suggested by someone who is not on our list. I am open to constructive suggestions.

Teacher 4 again :
Boyles law, Newton's laws, Kepler's laws, Hooke's law, The law of conservation of energy... have very little in common except for the fact that they are called laws and that they are relationships between variables.

One might call conservation of energy a universal law meaning that there are no known exceptions, while Hooke's law is an empirical law which certainly has exceptions. Why not have E=mc^2 as a law? But for some historical reason it is not. The term law would seem to imply that it is absolute, while many laws are only obeyed under certain conditions.

Hooke's law can be easily discovered by a student in a short lab, while Kepler's laws took years and much struggle to create. The term law seems to be applied to various equations almost by accident. I could just publish an experiment and generalize it and call it a law according to your definition. Is Hooke's law general? Is F=mg also a law? Or how about Ff=mu Fn? Sometimes they may be considered laws, but sometimes not.

I object to the word explanation because ultimately one comes up against the< fact that your explanation is just a statement "that is the way it is". Theories are in a sense complete reasonably consistent models which cover a whole range of phenomena. As such they are merely what we use to make predictions. They explain thing in terms of other things, but ultimately they are not complete explanations. The term explanation leads students and the public to think that they are final complete explanations rather than tentative models.

Part of the problem in the past has been the rigid definitions of these things promoted by textbook authors. These are not strictly scientific terms, but partially social and historical. Making rigid narrow definitions will be confusing because each scientist has a slightly or greatly different view of these terms. So I propose that law is a historical term applied to a relationship between variables. It is very difficult to say much beyond that. It is easier to say why one is Sir Soandso than to say why something is called a law. He was knighted by the queen or king, but laws are decided by convoluted accidents of history.

Teacher 7 :
One of the main problems involved in discussing whether evolution is a fact or theory is the context. There are two which tend to be commingle.

One issue is the evolution of life forms. There are many robust lines of evidence that life forms have evolved from common ancestors. Unfortunately, some have shortened the idea of the evolution of life forms to the single word, evolution. So in this context, evolution is a fact.

The other issue is the means by which evolution has occurred. Darwin's theory is that of evolution by natural selection. Unfortunately, some have shortened the theory of evolution by natural selection to the single word, evolution. So in this context, evolution is a theory.

This was most eloquently stated by Stephen Jay Gould who wrote: "Well, evolution is a theory. It is also a fact. And facts and theories are different things, not rungs in a hierarchy of increasing certainty. Facts are the world's data. Theories are structures of ideas that explain and interpret facts. Facts do not go away when scientists debate rival theories to explain them. Einstein's theory of gravitation replaced Newton's, but apples did not suspend themselves in mid-air, pending the outcome. And humans evolved from apelike ancestors whether they did so by Darwin's proposed mechanism or by some other, yet to be discovered." [Stephen Jay Gould, "Evolution as Fact and Theory," May 1981; from Hen's Teeth and Horse's Toes, New York: W. W. Norton & Company, 1994, pp. 253-262.]

Teacher 8 :
Teacher 4 wrote: “Boyles law, Newton's laws, Kepler's laws, Hooke's law, The law of conservation of energy... have very little in common except for the fact that they are called laws and that they are relationships between variables.” And that they are all generalizations of facts. In physics, we tend to generalize by making equations with variables.

“However, given that the genesis of this discussion was evolution, one might like to choose definitions that apply in biology. How do biologists use the term law? A little googling reveals that although there is some disagreement about whether biology can have laws (see

<http://www.springerlink.com/content/g0gj47u3q5716250/>),

the term is used for generalizations that cannot be expressed in terms of variables
(see

<http://en.wikipedia.org/wiki/Dollo%27s_law>).

Teacher 4 also wrote: “Why not have E=mc^2 as a law? But for some historical reason it is not. I could just publish an experiment and generalize it and call it a law according to your definition. Is Hooke's law general? Is F=mg also a law? Or how about Ff=mu Fn? “ I think it is clear that Ludwik is using 'generalize' in the sense of 'summarize,' not in the sense of 'extrapolate.' Perhaps there is a wording that is less open to misinterpretation.

Nevertheless, I would have no trouble calling E=mc^2 and F_f= mu F_N 'laws.' They are not traditionally associated with someone's name, but I can easily imagine using the word 'law' while describing them to a student.

Teacher 4 also wrote: “Sometimes they may be considered laws, but sometimes not.” Taking this in context, I believe what is intended by this sentence is "Sometimes they may be considered accurate, but sometimes not." The usage of "law" here would then be consistent with “The term law would seem to imply that it is absolute, “ But of course Hooke's law famously demonstrates that this is not consistent with normal usage -- as John explicitly states. Whether some generalization is 'sufficiently accurate' in any _specific_ situation is not pertinent to whether it can appropriately be called a law.

Ludwik’s new suggestion”
Thank you for these illustrations and observations, John and James.

1) I wrote:

DATA = what is reported
FACTS = validated data
LAWS = generalized facts
THEORIES = explanations of laws and facts

I am glad that we are not arguing about the concepts appearing on the right of the equations. They are essential and

obvious. I am certainly not the first to identify them. We are arguing about the words appearing on the left.

2) And yes, we have to deal with "social and historical" factors. Different people assign different meaning to words. This does create confusion. How can the situation be improved? One possibility is to invent totally new words, such as:

AADO=what is reported
BADO=validated AADO
CADO=generalized BADO
DADO=explanations of BADO and CADO

Yes, this is terribly artificial and probably impossible to impose.

3). Another approach would be to use existing words which are not likely to generate confusion. Here is one such possibility.

FIND=what is found and reported
CONFIRMATION=validated FIND
GENERALIZATION=generalized CONFIRMATIONS
MODEL=explanations of GENERALIZATIONS and CONFIRMATIONS

For example:

John's find and Mary's confirmation.
Kepler's generalizations and Newton's model.

That is probably better than artificial words. Perhaps someone will suggest better words; I am not a native English speaker. The third option is to accept the existing chaos and suffer the consequences.

Teacher 4 again :
No matter what we decide to do, there is no way to impose a specific set of terminology on physicists, let alone chemists and biologists. The only thing we can do is to try to communicate to students a fairly rational view. We don't have a prescriptive language academy as the French do. Despite that the French still do not follow the orders from above.

So there is no sense getting upset about some variant terminology as long as it is not too far from the mark. It is fun to argue about it! And our discussions have very little effect on the larger community who is oblivious of them.

However, it would be good to get the outrageous mistakes out of the textbooks in regards to this terminology. While the texts are very poor at teaching students, they are used as a resource by teachers and the teachers often slavishly follow them. When the latter is done science ends up being a hash.

It is sort of like dancing. One can accept some mistakes as long as the person does not go in a totally wrong direction and mess up everyone. So whether they do a proper hay, or just a circle is not disastrous.

Teacher 7 :
The Clausius statement of the 2nd Law of Thermodynamics? Or the Kelvin-Planck statement?

Teacher 4 again :
I did say that a law can be an equation, and the 2nd law of thermo can be an inequality equation. But laws are sometimes stated in such a way as to obscure the equation such as NTN1 or NTN3. One can play the game all day with looking for exceptions, but can anyone make a fairly good list of the known things that are all called laws and see if there are any rules that govern the preponderance of them. Then a better definition might be found. We don't make

the rules for what is called a law, common usage is the true deciding factor, and there are always exceptions. It the alternately proposed definition of a law seems more reasonable where do laws stop and theories begin? Another thing one can say about laws is that they are very specific, and that a theory often encompasses a number of laws. But we are dealing with classification which is somewhat alien to physicists, but not to biologists.

So can someone come up with a comprehensive list? The majority of examples prove the rule, not the exceptions because the definition of a law is not a physics definition.

Teacher 8 :
How are the Law of Conservation of Momentum or the Law of Conservation of Energy a relationship between two variables? The two take very different forms depending on what types of energy are being converted or what type of collision is being discussed. Newton's First Law of Motion? Newton's Third Law of Motion? Lenz's Law?

Wow, speaking of Lenz's Law, I came across this very cool video:
http://www.youtube.com/watch?v=fxC-AEC0ROk

Teacher 9 :
I think I know what "Data" is. And I'll even accept "Confirmed Data" I suppose as a comment that the experimental results are reproducible.

I don't know what "generalized data" is.

I don't know the difference between a theory, a law, a model, a framework, a [insert name]. How does this affect the way I teach or learn physics? It just seems to me that it is an accident of the historical development as to which term is associated with which development in physics -- maybe a measure of the term in vogue at the time of the development. Is the standard model of particle physics a theory? A model? Is there some semantic arithmetic law: 2 theories + 1 law = 1 model? (And if that's true and no one has claimed it, can it be Keller's Law? No wait, Keller's Theory? No wait, arg!)

No one who does physics or who teaches physics should be up at night worrying about these distinctions. So I wonder about the biologists. Is evolution a theory, a law, a model? I question the motivation of those who would have us ponder this, with a line from the movie 1776: "Whatever it is, I'm against it!"

Teacher 10 :
I was taught to use "The Energy Principal" by Peter Redmond whenever possible, as it is easier. This was some time ago -- have laws been substituted for principles?

I instinctively used law for the relationship of extension and force in my hardening magnetic repulsion oscillator. Not a generalization; is this wrong?

Ludwik Kowalski:
1) In a private message B.K. wrote:
Jules Henri Poincare (1854-1912) observed that “Science is built with facts as a house is with stones, but a collection of facts is no more a science than a heap of stones is a house.”

2) Let me paraphrase this : Laws of physics are relation between variables, but a relation between variables is not necessarily a law of physics.

3) And here is something similar. Each medieval stonecutter, working on a cathedral, was asked "what are you doing?" The first said: "I am chipping stones," the second said: "I am supporting my family;" and the third said: "I am glorifying God."

Teacher 11:
. . . The word "believe" often has a semi-religious overtone. To many people "believe" is equivalent to "faith in something that will never change" or even "my strongly held opinion that I have no intention of changing no matter what".

But most often scientists have something slightly different in mind. If I say I believe in something, for example evolution or the theory of relativity, what I really mean is that I think there is good evidence supporting it and no credible evidence against it. So it is reasonable to believe in it, at least until there was credible evidence to the contrary.

The idea that we can (and ought to) change our beliefs when we get new, better evidence seems to be a foreign idea to most people. Yet most people do this all the time, at least for beliefs that aren't so central. We believe our neighbor is honest and then we find his name in the paper as a crook so we change our belief.

I have no problem if someone uses the word believe in the above sense, either for belief in god or belief in evolution or even belief that evolution doesn't occur. Once we understand "belief" as something open to change based on evidence we can start talking about evidence.

Ludwik:
The word ‘believe’ does belong to common language. But it is not used in scientific validations of facts and theories.

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383) Two older sets of experimental results

Ludwik Kowalski

Montclair State University, New Jersey, USA
October 23, 2009

Unit 368 described a still ongoing CMNS project. The purpose is to validate experimental results that Richard Oriani presented at ICCF14. More recently, Oriani shared older results from experiments in a U-shaped electrolytic cell. These results are also waiting for independent validations. What follows was posted at a private list for CMNS researchers, (on 10/12/2009):

The tabulation below is a summary of my past experiments on the generation of charged nuclear particles in the anode compartment of electrolysis cells of U configuration. CR39 detectors suspended in the vapor above the electrolyte in the anode compartment (the anolyte) are enveloped by the oxygen generated at the anode. The fact that charged particles can be produced in an environment devoid of hydrogen and far from the electrode is something that theorists in the LENR field should be aware
of.
The electrolyses were carried out with H_2O/Li_2SO_4 as the electrolyte, Pt as the anode metal, and Ni as the cathode material, and lasted two to four days. The listed data are from experiments whose CR39 chips are still available for counting the nuclear tracks. The chips from additional experiments can not now be found for counting, and some of these experiments may in fact have produced no larger numbers of tracks than the controls. Moreover, I know that some of the experiments produced only few tracks. The generation of nuclear tracks on CR39 chips held over the anolyte is not reproducible. Nevertheless, the production of many tracks in an oxygen environment in these ten experiments should persuade theorists to attempt to elucidate the underlying mechanism.

exp #	tracks/cm^2	
	Side A	Side B
1	253	349
2	306	250
3	272	301
4	258	307
5	612	385
6	238	200
7	326	228
8	300	245
9	160	190
10	193	237

Controls:
Mean=5.9 (tracks/cm^2),
Std. deviation= 2.7tracks/cm^2)

These are indeed remarkable data illustrating emission of charged nuclear projectile caused by electrolysis. I hope an attempt to independently validate these results will be made. Equally interesting is Oriani's report that charged nuclear projectiles are emitted from the cathodes after the electric current, flowing through the electrolyte, is turned off. What follows was also posted at a private list for CMNS researchers, (on 10/13/2009):

I report here results of experiments done in 2005 and 2006 that reveal a residual effect, a sort of "life after death" in the words of Professor Fleischmann. This is the production of nuclear tracks by metal sheets that had previously been used as cathodes in electrolyses. I urge theorists in the LENR effort to attempt to formulate a mechanistic interpretation of these data. Even though the generation of nuclear particles during or after electrolysis can not be the agent for the thermal power that has been often measured calorimetrically, this phenomenon is well worthy of study in its own right as well as for a possible connection with the nuclear reactions responsible for excess power production.

Electrolyses were done in two different cells usually with H_2O/Li_2SO_4 as electrolyte and nickel as the cathode material, although a few were done differently, as indicated in the tabulation below. In all cases the cathode was a metal sheet held between the two o-rings of a glass joint (see Fig.1 of Ref.1) and the cathode served as the bottom of the electrolyte pool. A platinum spiral served as the anode positioned about 2 cm above the cathode sheet. The principal purpose of most of the experiments was to observe nuclear track production on CR39 chips held within the electrolyte or suspended in the vapor above the electrolyte. These measurements have already been reported (Ref.1). In many of these experiments the cathode sheet was dried directly after the electrolysis by placing absorbent filter paper upon it. After this, pre-etched and pre-counted CR39 chips were placed on either side of the cathode sheet and the whole assembly was tightly wrapped in aluminum foil for a few days. Etching in caustic and counting of the nuclear tracks followed the duration of the exposure of the chips.

The track number densities here tabulated represent the differences between the tracks before and after the exposure of the detector chips to the used cathode. For simplicity I report only the track densities developed on the chip positioned upon the side of the cathode sheet that during electrolysis had been in contact with the electrolyte (here called the wet side). In all cases the track densities from the wet side were greater than those from the opposite side (here called the dry side), although the dry sides produced track densities much larger than those on the control chips. The controls were of two kinds: pre-counted CR39 chips suspended over electrolyte solution without ongoing electrolysis, and pre-counted CR39 chips placed on fresh nickel sheet and tightly wrapped in aluminum foil. In a few experiments a second exposure of the used cathode to CR39 detectors was carried out immediately after the first exposure was terminated.

Reference.1: R.A.Oriani and J.C.Fisher, Proc. ICCF10,pp. 579-584, 2006.

= =

Days of exposure	Tracks/cm^2 Exposure 1	Tracks/cm^2 Exposure 2	Controls type	Controls Tracks/cm^2
4	369			
4	145			
	111			
2	128		S	18, 27
2	149			
2	133			
3	107*			
1	61			
	145			
2	X		S	22, 16
	63		S	26
	118			
	190			
	84			
	115		N	10, 25, 16, 21
	96			
	79			
	57		N	27, 0, 10, 0
	74		N	45
2	71	65		
2	43	61		
2	81			
2	53		N	20, 12
2	47	66	N	18, 15

3	67**	62	N	18, 12
2	94***			
4	91			

NOTES: For the controls: Mean=18 tracks/cm^2 Std. deviation= 10 tracks/cm^2

X means few tracks

* Pd was the cathode material

** Pt-plated copper was the cathode material

*** Pd as cathode and D_2/Li_2SO_4 as electrolyte

S Control chips suspended over electrolyte solution, no electrolysis

N Control chips placed upon virgin Ni sheets, wrapped in Al foil

A) Four experiments in which two exposures were made show that the rate at which particles are emitted after the end of electrolysis remains nearly constant during two or three days.

B) With one exception, labeled as X, track densities on experimental chips are larger than on control chips.

C) Referring to the first set of results (on 10/24/09) John Fisher wrote:

Group,

These experiments support polyneutron theory. According to polyneutron theory the nuclear reaction responsible for LENR in electrolysis experiments occurs in the bubbling region near an electrode where a polyneutron chain reaction is maintained. Bubbles that originate in this region rise and drag along electrolyte and the waste products it contains, and maintain an inflow of fresh electrolyte to replace it. Waste products include double nuclei, in which a large polyneutron and an ordinary nucleus (oxygen-16 or helium-4 in Oriani's experiments) are bound together. Double nuclei are not stable, and undergo alpha decay of the polyneutron component. These energetic alphas are responsible for Oriani's etch pits. It does not matter whether bubbling is driven by evolution of hydrogen atoms near a cathode or by oxygen atoms near an anode. The LENR action occurs in the electrolyte, which has the same composition at both electrodes, leading to double nuclei and alpha decay at both electrodes. The theory also predicts excess heat at both electrodes.

D) Referring to the second set of results (on 10/25/09) John Fisher wrote:

Polyneutron theory offers an explanation for the "life after death" nuclear tracks observed by Oriani and described in his communication of 13 October (I have lost that communication save for a printout, so cannot reproduce it here). In my 24 October communication I suggested that double nuclei, composed of an oxygen-16 nucleus and a large polyneutron bound to each other, are formed in regions of active LENR near electrodes in electrolysis experiments. And I suggested that the polyneutron components are unstable and emit alpha particles as they decay. (Two beta decays are associated with each alpha decay.) Because the oxygen-16 double nuclei are chemically oxygen those that reach a cathode can join an oxide film on the cathode surface, or otherwise chemically attach to the cathode. There from time to time they briefly transmute to fluorine and neon with two beta decays and then back to oxygen when an alpha particle is emitted. These alphas can be recorded on CR39 chips. After each alpha emission the polyneutron is reduced in size and the coulomb barrier for alpha decay (caused by the charge of the oxygen nucleus) grows larger. The alpha decay rate slows. The rate of production of CR39 tracks and of energy declines. These are the late stages of "life after death" for both tracks and heat.

E) Also relevant (?) is a message posted by another CMNS researcher. It listed possible paths for the exothermic p + Li reactiones.

$^2H + {}^6Li \rightarrow n + {}^7Be + 3.381$ MeV

$^2H + {}^6Li \rightarrow {}^1H + {}^7Li + 5.025$ MeV

$^{2}H + {}^{6}Li \rightarrow {}^{3}H + {}^{5}Li + 0.592$ MeV
$^{2}H + {}^{6}Li \rightarrow {}^{3}He + {}^{5}He + 0.901$ MeV
$^{2}H + {}^{6}Li \rightarrow {}^{4}He + {}^{4}He + 22.372$ MeV

Note that 7.5% of lithium (in Oriani's electrolyte) is ^{6}Li and his ordinary water contains a small amount of heavy water, and thus ^{2}H. Naturally, one can think of several other exothermic nuclear reactions able to produce nuclear projectiles, for example, $^{1}H + {}^{6}Li \rightarrow {}^{4}He + {}^{3}He$ (Q=3.7 MeV).

Yes, I am fully aware of Coulomb barriers involved. But this is a problem common to all CMNS reactions. An interesting paper about how to addres the Coulomb barrier issue was presented by E. Storms, at the last international 'cold fusion' conference in Rome (ICCF15).

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384) Loose Ends

Ludwik Kowalski

Montclair State University, New Jersey, USA
November 7, 2009

1) I did not participate in ICCF15 which took place in Rome last month. Next International Cold Fusion Conference, ICCF16, will be in India, in February, 2011.

Even the Pope is up to date on our research (photographed by F.D.).

2) A journalist, specializing in history of CMNS, Marianne Macy, published an interesting article about ICCF15:

http://www.infinite-energy.com/images/pdfs/macyiccf15.pdf

I was fascinated by the recovery of Martin Fleischmann, resulting from medical treatment by Irving Dardik. Dardik is an American physician who believes that CMNS phenomena can be induced by super waves (electric current of a complex frequency distribution). I heard him speaking about this at ICCF14. He has been using super waves to treat patients for many years. Suffering from Parkinson's disease, Martin Fleischmann was in rather poor shape recently.

According to Macy, *It took a year for Martin Fleischmann to make the decision to come to the U.S. to work with Irv Dardik. When arrangements were made for the trip, Fleischmann had classic symptoms of Parkinson's disease and could not fly. He and his wife Sheila boarded the Queen Mary in June 2009 to sail to New York. By the time he flew back to England, it was a less than a week until ICCF15 began. In the three months of his stay, he had worked with Irv Dardik and his team faithfully. "I never saw Martin say, 'I can't do it, I'm going back to bed,'" Dardik related.*

Fleischmann's daughter Vanessa, who had "not expected much," was struck by the change when she saw her father this fall. "It's as if we are going back in time some years," she said. Fleischmann committed to having local trainers

and supervisors continue his program from his home in England, working with the Dardik Institute via a computer setup. Martin Fleischmann, exposed to a flu before he left the U.S., was tired when he returned to England on September 29. He went to bed and rested for a few days. But at the end of the week, he got on a plane again, this time to Rome.

3) There was a brief exchange of messages about the so-called zero-point energy, at a private Internet discussion list. A.M. wrote: "I am dubious about getting useful energy from the ZPE (just as I'm not sure that you can get useful energy from the thermal motion that drives Brownian motion - and for the same reasons)." S.L. added: "We've spent a lot of time considering the possibility of extracting energy from the vacuum. It is important to realize that the same physics (QM, QED) that predicts the existence of the zero-point field also forbids the extraction of energy from it. Specifically, the ZPF is the lowest allowable energy state of the vacuum."

4) At the same time I received a private message from Ken Schoulders, the author of numerous articles about tiny clusters of zillions of electrons, named EVs. His most recent manuscript can be downloaded (it might take a minute or so) from
http://pages.csam.montclair.edu/~kowalski/cf/384shoulders.pdf

Ken believes that electric sparks consist of such clusters. This is another topic which does not belong to CMNS. I do not see benefits from explaining one controversy (a nuclear effect due to electrolysis) with another (existence of EVs). I think that each effect should be studied independently, at least at this stage of knowledge. In any case, I am not equipped to study EVs. What keeps zillions of electrons together?"

Responding to this, Ed. Storms wrote:
You advise not involving the unknown EVO in cold fusion because you can't understand how so many electrons can be held together. Cold fusion has a similar problem. No one can understand how the Coulomb barrier is reduced enough to allow such a high reaction rate. I think we should consider that a relationship exists between these two phenomenon, not just that both are unaccepted. Both require a process that offsets or neutralizes an electronic charge. In the case of an EVO, the neutralized charge is negative, while it is positive in the case of CF. In both cases, the charged particles are able to get unnaturally close to each other. In the case of CF, this close approach allows a nuclear reaction to take place, which is not possible in the case of electrons. In addition, It is easy to imagine that the presence of an EVO might help neutralize the charge between nuclei and help cause a nuclear reaction. In fact, Ken's observations suggest such a process.

On another topic, I would like to know how people view such speculations as done above. Is this theory or just BS? Where does BS end and theory begin? Does theory always have to involve math? If so, what is the value of math that is applied to the wrong mechanism and conditions because such ideas as above were ignored?

Ludwik Kowalski responded:
Ed's speculation is interesting. In my opinion speculations, even dreams, etc. are worth sharing. They can be, and probably often are, powerful motivational factors. That is why I do not think the vulgar BS term is an appropriate descriptor. But loose speculations are not part of our acceptable scientific validation process.

Let me refer to a well known example. Calorimetric measurements, in 1930s, revealed that the amount of thermal energy released in beta decay is much lower that what was expected. That prompted Pauli to predict existence of neutrinos. Was his publication only a speculation (motivation for the 1940's experiments in which neutrinos coming from a nuclear reactor were discovered) or was it part of an acceptable validation process? I agree with Ed that this topic is worth discussing.

Andrew Meulenberg wrote:
Ludwik, I partially agree and partially disagree.
(1) Dreams and fantasies can be very powerful motivators; but, they can also be very dangerous to the dreamer who can't cut loose or distinguish them from reality. (2) Sharing dreams is more often negative because most listeners can't see the same picture and thus respond appropriately (3) Remember, BS is good fertilizer. However, it does no good just to sit on it. You have to spread it around (in the proper places).

Another CMNS researcher added:
Why vulgar? Ed was probably referring to Bold Speculations.

Ed clarified:
I was actually referring to Bad Speculations. :-)

Ludwik added:
Bold scientists make bold speculations while bad scientists make bad speculations. But what kind of speculations are made by bogus scientists. How to distinguish bogus speculations from those that are useful? This seems to be the essence of Ed's question.

Ed Storms wrote:
The issue is how to separate loose speculation from usefulspeculation, as your example below describes. I submit that all theory starts with speculation. The speculation shows where to look for the operation of a mechanism or what kind of mechanism is worth exploring with various mathematical tools. I get the impression that speculation is called a theory only after math has been applied even though the math adds nothing but a restatement of the speculation. In fact, most of the math in "theories" applied to CF are simply a restatement of the basic assumption. The fact that such a mathematical restatement is possible is used as "proof" that the initial assumption is correct. People then attempt to calculate reaction rates based on this approach. However, the results are impossible to apply to observation because in the real world, conditions are not uniform or under control. Consequently, it is impossible to test such a theory.

I propose a different approach. Enough is now known about the phenomenon to make logical deductions. These deductions reveal where the nuclear events are occurring, some of the conditions that must exist at these sites, and information about the mechanism. When does such an approach rise to the level of theory?

Akito Takahashi wrote:
Does theory always have to involve math? Who can make quantitative estimates without any mathematics? I have no other ideas than believing that mathematics is indispensable to make a theory "plausible". We have no other tools than mathematics (primitive or elegant does not matter) for quantitative estimates. However, it does not always mean that the usage of mathematics makes up theory so beautiful and complete. We need cross-checking its consistency from many angles. The quantification of a theory makes the cross-checking clear and definitive. . . .

Ed Storms responded:
I agree, Akito, math is necessary to make quantitative estimates. But is it necessary for an idea to be called theory? For example, is it necessary to apply math before plate tectonics or Darwin's Theory of Evolution are called theories? More to the point, must math be applied before a proposed mechanism about where and how cold fusion operates is called a theory? For example, at ICCF-15 I proposed a mechanism for cold fusion but in many minds this does not rise to the level of theory. Why not?

You have raised another question - how does a person judge whether an idea is plausible or not? Math can take two roles: it can be the entire basis for a model or it can be used to compare a proposed mechanism to other measurements. In the first case, the ability to achieve internal mathematical consistency alone is used as a demonstration that the initial assumption is correct. String theory comes to mind as an extreme example. In the latter case, the theory is considered correct only when the math shows quantitative consistency with many independent measurements. The theories of thermodynamics are good examples of this approach.

The problem is that math alone does not make a theory plausible. For example, in the case of cold fusion, most math simply restates the initial assumption without adding anything new. Following the initial assumption, a series of ad hoc assumptions are made to move the math to the next level. I don't think this process adds plausibility even though it is necessary to explore a proposed mechanism. To be clear, I do not object to this process. I'm only objecting to the process being used to add plausibility to a proposed idea. The initial idea is only plausible after it shows a quantitative relationship to many other kinds of measurements or makes exact predictions. In my opinion, no theory in cold fusion has reached this level. I believe the basic reason we have this problem is because the mechanism and where this

mechanism operates in a material have not been correctly identified.

You wrote" We have no other tools than mathematics (primitive or elegant does not matter) for quantitative estimates. However, it does not always mean that the usage of mathematics makes up theory so beautiful and complete. We need cross-checking its consistency from many angles. The quantification of a theory makes the cross-checking clear and definitive." I agree. This process is necessary. The question is, at what point does a proposed model become plausible? When I ask this question, the answer has to apply to a person who has not proposed the initial idea. For a person to go to the considerable effort to develop a mathematical model, they must be certain in their own mind that the basic idea is correct. Therefore, they have an important emotional stake in the idea being correct. This is natural and required for an idea to be fully defended, but it can result in conflict and differences of opinion that can never be resolved. As a result, discussion between advocates of particular models, theoreticians, is seldom productive. . . .

Ludwik wrote:
1) It will probably be useful to separate arithmetic (use of numbers) from the rest of mathematics, such as calculus, etc. Arithmetic is part of common language; it is widely used outside science. That is why a publication in which arithmetic is used is not necessarily scientific.

2) To non-scientists the word "theory," often means "a questionable speculation." It is only a theory, they often say. But we use this word differently. Scientists, for example, are often characterized as either experimentalists or theoreticians, depending on what tools they use to make discoveries, or to explain discoveries. Those whose primary tool is mathematics are called theoreticians; those who use other tools are called experimentalists. (This is not the only case in which a word means a different thing to a scientists and a non-scientist. 'Force,' 'energy,' and 'power' are commonly used interchangeably by non-scientists).

3) In my definition, a law is a generalization of facts (for example, Kepler's Laws). Some laws, as emphasized by Ed, are qualitative while others are quantitative. A theory, in my definition, is an explanation of a law (for example, Newtons law of gravitation) or of a fact. It can also be either qualitative or quantitative. A qualitative theory, as emphasized by Akito, is less useful than a quantitative one.

4a) In the last paragraph Ed asked: "at what point does a proposed model become plausible?" I suppose that the word "plausible" refers to a mathematical model "to be taken seriously." Those who are not able to penetrate details of a model have no other choices than to rely on the authority of the author, or on the authority of those who say that no mathematical mistakes were found.

4b) Note, however, that scientific theories, unlike mathematical models, are validated not only on the basis of their logical correctness but also on the basis of their ability to guide to discoveries of unknown facts. That how I understand Ed's statement that "math alone does not make a theory plausible." Experimentalists look for quantitative predictions of scientific theories and try to validate them with their own tools--laboratory instruments. I agree with what Ed often repeats--the value of a scientific theory depends on the number of verifiable predictions it makes.

Ed responded:
1) I agree we can break math down, but I would not make the break where you place it. The models being applied to CF use a level of math much different from calculus, which I suggest is a form of arithmetic. For example, I would put the Hamiltonians, Eigenfunctions, wavefunctions, and Schrodinger equation separate from calculus and arithmetic. These are based on concepts and assumptions about Nature and are not truly like calculus and arithmetic. In any case, these concepts seem to be an essential feature of anything that is called a theory in CF even though their use requires numerous ad hoc assumptions.

2) A theory only becomes useful and valuable when experimentalists verify it, yet theory seems to hold a higher role than experiment. We saw this approach operate when people rejected CF because they could not explain it.

3) Is being able to calculate a hypothetical reaction rate more valuable than being able to say that if you mix X with Y, the CF reaction will work better?

4a) Very few theories in CF have mathematical mistakes. The issue lies in the assumptions on which the math is based, which keep changing as objections are raised.

4b) That's right. A model is useful only when it guides work successfully. The model might even be incomplete, hence not plausible. My only goal in this discussion is to encourage people to show exactly how their model relates to the real world and to variables that can be modified to have a beneficial effect. Short of that application, a model is only a logical game.

Ludwik wrote (quoting (2) above:
a) History of CMNS would be very different if Fleischmann and Pons announced nothing more that the discovery of unexplained excess heat. In that case only qualified electrochemists would probably perform replication experiments. Confirmation of of the discovery would lead to theoretical speculations, most probably only by qualified theoreticians. In other words, influence of bogus experimentalists and bogus theoreticians, on the outcome of the validation process, would probably be negligible. Premature speculations about nuclear origin of excess heat was a mistake with devastating consequences.

b) Suppose I am clever or lucky to discover a new phenomenon. Suppose I suspect that it belongs to cold fusion field. I would keep the suspicion for myself. In a submitted paper I would describe the procedure and emphasize my inability to explain the results. (If the discovery were excess heat then I would have tried to convince readers that known chemical reactions are not responsible for it.)

c) If I were a theoretician I would not have tried to validate a theory with irreproducible experimental results. I would have only said that such and such assumptions lead to such and such consequences. That would be a mathematical model, not a scientific theory. Yes, I know that the word "theory" means different things to different people. Distinguishing a mathematical model from a scientific theory is probably useful. To turn a mathematical model into a scientific theory I would make verifiable predictions, hoping they will be confirmed by experimentalists. A model becomes a theory when it's predictions are confirmed. This is not the same thing as "predicting" what is already known.

d) P.S. (on another thread):
I wish I were sufficiently knowledgeable to debate the topics mentioned by D.S. The only option I have is to trust high energy physicists. Reliance on authority of experts is much more common than most laymen believe. In fact, that is what makes today's science very different from what it was, for example, in the second half of 19th century.

e) P.P.S. (indirect measurements):
The repulsive force between two atomic nuclei (when their surfaces are separated by at least two fermi of empty space) is proportional to 1/r^2. How do we know this? By performing scattering experiments. Here is one example. Rutherford was observing alpha particles scattered by a thin gold foil. Assuming the Coulomb law is valid he derived a mathematical relation between the probability of scattering, P, and the angle of scattering T. The theoretically calculated P turned out to be inversely proportional to [sin(T/2)]^4). This relation, and other theoretical relations, was confirmed by experimental data.

Measuring one thing and theoretically inferring something else from it, can be called an indirect measurement of that "something else." Another illustration is determination of the radius of an atom, also via the Rutherford scattering formula. What is actually measured is the angle at which the experimentally measured P becomes smaller than that predicted by the theoretical formula (derived under the assumption that the nuclear radius is zero). The "indirectly measured" atomic radius is calculated from that angle. Most measurements in atomic and subatomic physics are indirect. In other words, theoretical and experimental instruments are inter-linked in atomic and subatomic physics. Indirect measurements are also used in classical physics, for example, when radar is used to measure a distance.

John Fisher (who developed a CMNS theory described in units 227 and 364) wrote:
I have been asked several times to comment on LENR theories. Quantum mechanics has been developed and refined in hundreds of thousands of experiments and by tens of thousands of experimenters and theoreticians over a period of a hundred years. It is the most comprehensive and quantitatively accurate description of nature ever achieved. A truly

remarkable accomplishment of humanity. I accept quantum mechanics as the basic and essential framework for understanding the physical world, including LENR. Experiments beginning with Fleischmann and Pons have persuaded me that the LENR phenomena are real, that they indicate nuclear reactions of a previously unappreciated nature, and that they offer promise of potential social benefits. I have been searching for an interpretation of these phenomena that can explain what has been observed, and that will predict what can be observed in future experiments. Quantum mechanics provides the basic methodology, and I do not consider any theory that is in conflict with it.

Ed Storms responded:
1) John, quantum mechanics is a very broad field with many variations and assumptions, some of which are not universally accepted. While I agree, the basic idea needs to be accepted and used, the problem comes in the details. Just how should QM be applied? What if a novel electron energy level or state is required to explain CF, but this has not been considered by QM, hence is rejected. Should this rejection be ignored?

2)QM has been very successful in describing the hydrogen atom. However, it has less success in describing other atoms without using ad hoc assumptions.

3)Which assumptions should be accepted and which should be rejected? On what basis should the choice be made? These are the questions I'm trying to answer.

John Fisher responded (point by point):
1) A state that has previously been considered by QM and rejected should continue to be rejected even if "needed" for LENR. A state that has not previously been considered by QM should be considered. It can be tentatively accepted while its QM implications are explored. Depending on whether the QM implications agree or disagree with observation, its tentative acceptance may be strengthened, or it may be rejected as failing to agree with observation. This is how much of our understanding of nuclear physics has been accumulated.

2) The assumptions behind the theory of the periodic table of elements are not ad hoc. They are the same assumptions as for the hydrogen atom. However the determination of physical parameters has usually been done by experiment rather than by theory because experiment is so much easier. (As an example from nuclear physics one needs to know the masses of the various isotopes in order to calculate the energetics of nuclear reactions. We use measured masses because we do not have the computing power to calculate them.)

3) An assumption should be rejected if its QM implications disagree with observation. Otherwise it can be tentatively accepted until further observations with broader implications may lead to disagreement and rejection. If the proposer is skilled or lucky his assumption may never lead to disagreement with observation and may win enduring acceptance.

Andrew Meulenberg wrote (addressing John Fisher):
. . . Thus QM, is not wrong. It is just not self sufficient. As Ed has stated, it depends on what is put into the model. One extreme is GIGO - garbage in, garbage (or, as Dean would say, Quarks) out. The other extreme, just as bad, is to not allow new ideas in. [Dean is a CMNS researcher who wrote that the idea of quarks is nonsensical.]

Ed Storm wrote: (addressing Tom Barnard.)
I'm trying to think of some way to get through this impasse. I have found trying to critique a theory with some one who has a committed view to be impossible. The logic structure is complete in their mind. Like a builder, each beam and brick has its place and a failure of any part threatens the whole structure. Consequently, no part can be recognized as being in error. The result is an increasingly detailed argument about increasingly small details until the big picture is lost and patience runs out. I have also discovered that QM can be molded to fit almost any notion about reality and to arrive at almost any conclusion. If you examine the many ways other people have used this tool to explain CF, you will see what I mean. To add to the problem, different people have different ways of explaining the same ideas in QM. As a result, a lot of time has to be invested to discover just what is being said. Furthermore, QM is not the only way, or perhaps the best way, to describe CF, at least not in its pure form. So, rather than beat a theory to death, my attention is directed mostly to discover just what is known so that we all have a common reality to which the theories can be compared. . . .

Ludwik:
Attempts to turn a mathematical model into a scientific theory are usually made after at least one experimental result is recognized as reproducible on demand. That is why I would replace the term “a common reality,” in the last sentence above, by “a reproducible on demand experimental result.”

Tom Barnard wrote:
I very much feel your frustration Ed. I respect very much the work you have put in and continue to. I respect everyone's time. I would not persist here for trivial reasons. I have worked very hard to attain a certain intuition based on QM. Many years solving the Schrodinger equation for known phenomena in order to gain basic competence. My ideas are really quite simple QM. Basic phenomena in the QM world, but voodoo in the Newtonian world. I can't help that.

I know it is a matter of trust and frankly, I haven't earned any. That is why an adherence to the basics; to alleviate the trust factor a little. Everything I say can be fairly easily checked in the texts, of course, necessitating trust in the texts. . . .

Andrew Muelenberg wrote:
Ed, Please lighten up a bit. Your questions and challenges are pertinent; but, they are also premature and discussing them in detail at this point detracts from others obtaining the background information that they (and you) may require.

You are too anxious to get to the goal. Your goal is 20 feet up and you refuse to see the ladder as an option because it has a few broken rungs. Please don't block the ladder, because you think it is unsafe. Let others use it if they wish. Once they are there, then you might reconsider using it. . . .

When I first saw [Tom’s approach], everything fell into place. This concept is similar to KP's lochon model to overcome the nuclear Coulomb barrier and, when properly extended, is sufficient to explain all of the other CF data observed. Tom hasn't gotten to that latter point yet, but my ICCF-15 presentation (to be posted here after Tom finishes his model) describes the rest of this story. Please, note your objections (for future discussion); but, let Tom continue unless someone else finds it difficult to continue without more detail.

Andrew also commented on so-called “zero point energy” (ZPE), discussed in another thread. Recognizing reality of ZPE, someone compared proposals for using this energy to attempts to get work or electricity from Brownian motion. Referring to this Andrew wrote: “. ....Brownian motion again: the moving particles can do work; they move other particles. If micro-resonators were put into motion by the thermal activity, then they could generate fixed-freqency AC currents that could be rectified to give useful energy out. Even if all the waste heat were recycled to the thermal bath, the bath would cool unless compensating heat was added. While this can be done in theory, it is not cost effective. ZPE is similar to Brownian motion, but at an even smaller scale....."

Ed responded:
Point noted Andrew. My problem is Tom's inability to explain his idea in a way I can understand or agree with. He knows only the language of QM, which is the only viewpoint he will accept. Reality can be explained many different ways and each is equally valid within its limitations. I find the best description to be a mixture of methods, especially when QM moves too far from easy understanding. As you noted, my patience is thinner than it should be because I have studied all of the various theories and find them filled with unsupported assumptions, therefore a waste of time. I'm especially impatient with claims that a viewpoint based on QM must be accepted just because QM is too complex to understand or because the description is textbook correct. I'm presently writing a paper in an attempt to bring together all of the relevant experimental understanding so that some of the theories can be properly tested against reality. I'm of the belief all math must be related to what can be measured, hence it must be understandable in terms of the variables and conditions we can control and measure. Anything short of that requirement is hand-waving, which is a very common practice in theory.

Meanwhile, I agree with Tom, a special kind of bonding is required to create a structure that can shield the Coulomb barrier. QM might have a role in explaining this bond provided it is applied to the correct conditions and assumptions. Mills proposed a similar bond that has been rejected using QM. It would be great irony if QM could be used to justify

such a bond. Anyway, I will stop critiquing Tom's description as you suggest and wait patiently for him to complete his description.

I might add, I find this discussion very useful because it forces me to consider approaches and understanding I would normally ignore. So, I encourage Tom in his efforts and wish him success.

Andrew added:
Ed, I don't want to discourage your critical and careful analysis. Please continue to comment on areas that you have difficulty accepting. Just don't expect them to be resolved immediately. A brief comment won't slow progress and it will help Tom, and others, to note areas that may require more work.

My understanding is that Tom acquired QM recently - by a lot of hard work. He might appear to have the convert's enthusiasm, but I believe he spoke the classical language previously. (I may be wrong, but I don't believe that his critical revelation came from QM.) However, I think that he also recognizes that if a model, right or wrong, is not couched in QM terms, it will not be considered valid by the rest of the Physics community. So he is doing a triple service:

1) he's teaching some of us, and bringing many of us up to speed on, the language of QM.
2) he's giving us the language that might make CF acceptable to modern physics.
3) he's proposing a model, which may not be completely correct or complete, with aspects of what I consider to be the best chance of breaking the CF-modeling dilemma.

I also look forward to seeing your paper. From some of your comments, I feel that there are areas on which I would like to see more data.

Ed added:
Tom, as Andrew suggests, please go on with your explanation and I will hold my comments until you have provided the entire picture. In the process, please be aware of the assumptions you are making. I agree, a novel electron bond is required. If you can prove, using QM, that such a bond exists, you will beat Mills at his own game. However, as you discovered from my comments, this will not be easy. Also, remember QM is not the only way this bond can be explained if it is real. Anyway, I look forward to your description.

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385) Cavitational effect on radioactivity (continuation).

Ludwik Kowalski

Montclair State University, New Jersey, USA
December 7, 2009

1) **This is an update on what I wrote in Unit #378,** about the report of Cardone et al. In addition to the first four publications listed below.

I subsequently discovered a short note (June 8, 2009) on the same subject in CERN Courier . The author, John Swain, wrote: "It is a common belief that radioactive decay rates are unchanged by external conditions, despite many examples of small shifts (particularly involving external pressure and K-capture decays) being well documented and understood. However, Fabio Cardone of the Institute per lo Studio dei Materiali Nanostrutturati in Rome and colleagues have shown a dramatic increase – by a factor of 10,000 – in the decay rate of thorium-228 in water as a result of ultrasonic cavitation. Exactly what the physics is and whether or not this sort of effect can be scaled up into a technology for nuclear waste treatment remain open issues."

2) Unable to attend ICCF15, I submitted my own set of observations (about Cardone cavitational effect) to Physics Letters. This was two days before the conference. The note I published (6) can be downloaded from my website.

http://csam.montclair.edu/~kowalski/cf/ . . . . . .

Comments on what I wrote would be highly appreciated. I plan to update this unit when new information becomes available.

3) APPENDED ON 12/7/2009

Here is the message I posted two days ago on a private Internet list for CMNS researchers:

"According to an ICCF15 report of A. Carpinteri et al. at

http://iccf15.frascati.enea.it/ICCF15-PRESENTATIONS/S6_O5_Carpinteri_Lacidogna.pdf

neutrons are emitted when iron-containing marble is mechanically crashed. [Google found this paper for me because F. Cardone is one of the three authors.] Their detector, located about 10 cm away from the marble block, recorded ten times more neutrons during the crash than during the crash of marber that did not contain iron. The authors believe that neutrons are emitted from this fission reaction:

$^{56}Fe \rightarrow {}^{27}Al + {}^{27}Al + 2n$

The arrithmetic is correct, 27 + 27 +2 = 56

But the energy threshold of this reaction is 42.3 MeV. Do the authors believe that mechanical energy supplied to the crashing machine is somehow used to make the above transmutation possible? Please explain."

X responded:
"I wonder if the crushing of marble not containing iron produces neutrons? This would correspond to a "placebo test" for drugs. In my--possibly crackpot--theorizing, there may be a rather ubiquitous oscillator present which can be split

to an electron and proton and shock-wave-deformed into neutrons. A crushing force could set up a shock-wave, making neutrons appear "magically." That is, if "Zerotron" isn't just a figment of my overactive imagination."

Ludwik responded:
"(1) The link I provided has a photo of two marble blocks that did not produce neutrons and two blocks that did produce neutrons. Only the second set of blocks contained iron. (2) Hitting a granite stone with another granite stone often produces sparks. This is plasma, between two surfaces. That is what came to my mind. (3) I have no idea how was the fission of iron idea justified. Perhaps it was based only on the 27+27+2=56."

X responded:
[You wrote] 'Plasma between surfaces, spark formation, Iron needed for the neutron formation.' None of this rules out my little speculation of the "Zerotron" involvement. The "Zerotron-splitting" in pair formation supposedly only occurs near 'matter' Deformation would require a smash into something which wouldn't get out of the way or deform easily, perhaps Iron atoms or ions fit that requirement."

Ludwik responded:
In other words, you do not believe their idea that fission of iron is responsible for neutrons. In any case, it is too early to speculate; let us wait till results are independently confirmed by others.

X responded:
As for me, I simply don't know, I tend to be skeptical about neutron formation because of my skepticism as to the presence of neutrons per se in atomic nuclei... The fission of iron could be the explanation. I'm pointing out that there may be some other explanation for neutron formation in a lot of cases. ..... "

Ludwik responded:
No, this kind of fission could not be the explanation. The energy threshold for that particular reaction is 42.3 MeV. Where would it come from?

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386) Integrity or hypocrisy?

Ludwik Kowalski

Montclair State University, New Jersey, USA
December 19, 2009

The web site of The Physics Today had a discussion thread on integrity:

http://blogs.physicstoday.org/newspicks/2009/12/opinion-scientific-integrity.html

On December 18, 2009 I submitted the following comment:

Fred Dylla wrote: "There are of course interesting physical phenomena with fractoemission in solids and within cavitating liquids but not as nuclear energy sources."

Yes, something seems to be going on but it is not understood. Furthermore, experimental results, even those reported by recognized experts, are often not reproducible. I think that attempts to verify claims made by experts should be encouraged.

In a recently published book, “The Science of Low Energy Nuclear Reactions,” Ed Storms summarizes cold fusion claims and provides references. The book is available at www.amazon.com .

The so-called “cold-fusion episode” is unprecedented in many ways, especially from the point of view of sociology. It will become an important part of the history of science, no matter what.

Ludwik Kowalski
Professor Emeritus (Ph.D. in Nuclear Physics, 1963)

After being informed that my comment has been posted (by Paul Guinnessy
Manager, Physics Today web site) I checked that all four short paragraphs were posted, as above. But going to the web site later I discovered that two changes were made:

a) The third paragraph, referring to Storms’ book, has been removed.
b) The last line, below my signature, has also been removed.

Who decided to change the content of a message that had already been posted and why? I can only speculate about the motivation; probably someone did not want readers to know about Storms’ book. The name of the thread, by the way, is “Scientific Integrity.” The term “integrity,” according to Wikipedia, is

in contrast to “hypocrisy.” Was the third paragraph removed for the sake of integrity or was it removed for the sake of hypocrisy?

Added on December 20, 2009

Another message on the ongoing thread about Integrity was prepared. But I was not able to submit it. I tried about ten times and each time the error message told me that the displayed words were not typed in properly. That is practically impossible; they were easy to recognize and I was very careful in typing them. It must have been something else. In any case, here is what I wanted to post:

Like many others, I was very excited when the discovery of the so-called “cold fusion” was announced in 1989. Then I was convinced that no sufficient evidence was presented for a claim that a nuclear phenomenon, like emission of nuclear particles or transmutation, can result from a chemical process, such as electrolysis. In 2002 I was surprised to discover that the cold fusion field is not dead. I started reading reports of recent investigations and attended several international conferences devoted to cold fusion. I am still waiting for at least one cold fusion protocol reliably producing a nuclear effect due to a chemical process.

But I also discovered that many cold fusion researchers validate their findings in the same way as mainstream scientists. They perform experiments, they discuss them, they speculate about theoretical models, etc. In other words, they are not voodoo scientists; they stick to scientific methodology of validation. Unfortunately, they are often treated as if they were voodoo scientists. That is very unhealthy. Starting this thread Frederick Dylla wrote “Science is by its very nature an exploratory, trial-and-error venture which is also—sooner or later in every case—a self-correcting exercise.” He referred to a Soviet agronomist Lysenko, whose false theory was finally rejected. Stalin supported the theory by silencing qualified opponents.

The cold fusion controversy would be resolved much faster if investigations in that area were treated like other projects of qualified scientists. Silencing the proponents is not helpful. My objective reports and comments, on what is going on in the cold fusion field, can be found at

http://pages.csam.montclair.edu/~kowalski/cf/

The last item (#386, at the end of the long list of links) is devoted to this thread. In my opinion, Physics Today should occasionally inform its readers about new claims made in the cold fusion field. This would probably lead to useful results (clear yes or no answers about reproducibility). Scientific controversies, as we all believe, should be resolved by scientific, rather than administrative, means.

Storms’ book, available at www.amazon.com , is the only recent systematic review of the controversial cold fusion field, as far as I know. Ed Storms is a retired LANL nuclear chemist. I strongly recommend his book to serious scientists. The new name for “cold fusion," by the way, is CMNS; it stands for Condense Matter Nuclear Science. Another often used name is LENR; it stands for Low Energy Nuclear Reactions.

Ludwik Kowalski
Professor Emeritus (Ph.D. in Nuclear Physics, 1963)

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Click to see the list of links

387) Future of CMNS

Ludwik Kowalski

Montclair State University, New Jersey, USA
January 31, 2010

1) Future of cold fusion has been mentioned in the private discussion list for CMNS researchers. Last night Ed Storms wrote:

". . . Physicists seem to think they are the only evaluators of reality and they have applied their approach to cold fusion. This approach involves ad hoc assumptions about ideal conditions and application of mathematical models. Such an approach cannot be tested because it does not describe actual reality in this case. If they had been the evaluators of early chemistry, early genetics, or even the invention of the light bulb, these fields would have been slow to develop as well. While you might think this conclusion rather extreme, nevertheless, no theory will be accepted because none can be tested until the nature of the required material is understood. Once Fleischmann and Pons claimed a nuclear process, which let physics into the discussion, they were doomed. The field will continue to be slow to develop until this attitude changes or until someone finds the active material by accident."

2) Responding to this I wrote: "Another very powerful trigger would be a single reproducible-on-demand demo of a strong nuclear effect, such as undeniable transmutation or emission of tritons, resulting from an atomic (chemical) process.

What would I do if I had the power and the means to proceed toward such a goal? I would start by selecting (in consultation with others) several most promising CMNS protocols. The protocols would be ranked as #1, #2, #3, etc. Then several teams of competent researchers would be asked to verify the claim made by authors of Protocol #1. Sooner or later they would come up with the answer. Their "yes" would mean that the goal has been achieved; their "no" would mean that Protocol #2 should be tried, by selected groups of competent researchers. They too would deliver a clear yes or no answer., sooner or later. Then the same for Protocol #3, etc., etc, till the last protocol is tried, as schematically illustrated in Figure 1. Yes, I know that this would not be easy in practice. But is it possible in principle? I think so. Do you agree?

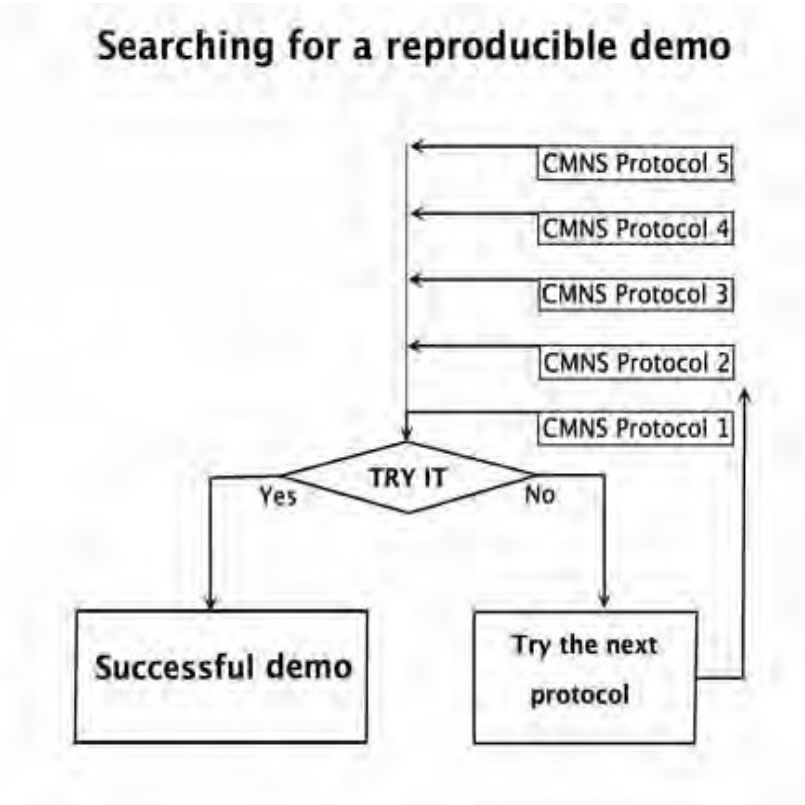

Figure 1
Desirable strategy for making progress in CMNS field.

3) Responding to the above, Larens Imanyuel wrote "I find your approach too rigidly methodical. A successful scientific debate in a difficult field is more like the case study of a Sun Tzu battle." [According to Wikipedia, San Tzu (6th century BC) wrote "It is said that if you know your enemies and know yourself, you will not be imperiled in a hundred battles; if you do not know your enemies but do know yourself, you will win one and lose one; if you do not know your enemies nor yourself, you will be imperiled in every single battle."]

4) Responding to Larens, I asked:

(a) What kind of battle are we fighting?
(b) Who are our enemies?
(c) What should we know about them?
(d) What should we know about ourselves?

5) Responding to the above, Larens Imanyuel wrote:

(a) "The battle is to have LENR recognized as a fundamental physics breakthrough within the international scientific policy process, so that LENR get the scientific resources they deserve.
(b) Scientists who have not been open-minded about LENR.
(c) We need to know which scientists to win over with which arguments, so as to destroy the alliance against us.
(d) We need to know those biases within us which have prevented us from winning the battle."
6)
6) One researcher wrote that CMNS "cannot be explained by conventional physics." Another wrote that "nothing about CF requires new physics. I agree, some physics will have to be modified in ways that have not been considered." This led to an argument. Referring to the second researcher, the first one wrote: "You are asserting your belief as fact here. It is your obligation within the discussion to present some type of proof that new physics is not required."

The proof will be in the pudding. A theory predicting reproducible experimental data will be accepted. Will it be based on conventional physics or will it be based on something totally new? This remains to be seen.

7) The most important task, for the time being, is to find at least one reproducibl-on-demand protocol yielding a strong nuclear process resulting from an atomic process. A challenge to theoreticians will be not only to explain that nuclear process but also to show that a successful theory predicts new reproducible phenomena. That is what I learned from those who described scientific methodology of validation and acceptance. I strongly believe that the discovery of the first reproducible protocol will speed up process of understanding of CMNS phenomena.

8) No one contributed so far. That shows that what is on my mind is not necessarilly on the minds of others. Here is how this unit was announces, about 15 hours ago: Capturing what we think about the future of CMNS (for future generations) is important. With this in mind I started composing a new unit:

http://pages.csam.montclair.edu/~kowalski/cf/387future.html

So far this link is "secret;" please do not share it with others. I will let you know when the unit becomes public. Ed already gave me permission to quote. I hope Larens will do the same. Please contribute to this thread. Or send me a private message with something to insert. In that way no one on this list will know who the author is. Unless I have permissions, names will be replaced with X, Y, Z, etc., if you prefer. But let me know.

Added on 2/2/2010

9) A question about resonant tunneling was asked by X. Not being a theoretician, I usually stay away from mathematical analysis of CMNS problems. What follows is quoted to show that, contrary to what many outsiders believe, disagreements and personal friction, also exist in CMNS community. We are not a mutual admiration society. Responding to X, Professor Y wrote:

In Professor Li's work, unlike mine, he solves Schroedinger's Equation for a complex wave function, which has a phase rather than merely an amplitude as I have. Therefore he can compute a reaction RATE rather than get a simple YES/NO answer.

Every one of the half-dozen theorists whom I mentioned in my MIT Colloquium presentation is doing some aspect of the problem in greater detail than am I and "my" answer could be improved if they would examine the same scenario that I did but with more detail.

For example, Drs. Talbot & Scott Chubb usually consider a 3D lattice whereas I [like Schiwinger] have simplified the problem to a 1D approximation in order to be able to get an answer more easily.

In Prof. Yeong Kim's work, he is solving the actual Schroedinger equation EXACTLY rather than just using the widely accepted standard approximate solution which is easier to use and is what I used. I have told Prof. Kim that if he considered the boundary conditions of the lattice (rather than just the two isolated nuclei) he would get an improvement in the theory which I have been advocating.

Likewise "my" theory could be improved if one included the phonon aspects, as does Prof. Peter Hagelstein, in order to include the subject of the post-fusion energy being transferred to the lattice in the form of lattice excitations or heat.

But NONE of the other theorists in the field has ever even cited the Y's work, nor any of my four "proofs" of varying degrees of simplification, much less improved upon it as I have often expressed the hoped that they would!

Added on 2/3/2010

10) X2 wrote: "There is a conflict here between political styles. I see the necessity of an initial political phase of building personal relations, before trying to win a political battle in a large forum. These behind the scene personal relations are what is going to insure victory in the large forum. If newcomers feel a "get to the back of the bus, we old-timers are running the show" attitude, the necessary positive social relations will not be built. That is why I am

working first to build working groups that can move our agenda up the scientific bureaucracy."

11) X3 responded:
"I agree with you, building trust and inclusion is essential. As far as I can tell, this is the general policy of the field, including my own. Relationships are currently being built between a wide variety of institutions. In the process, two other dynamics are operating. Efforts are being made to bring a higher standard of experiment and theory to the field. These efforts involve criticisms that can cause resentment. At the same time, some people care only about their own self-interest, which they promote by criticizing efforts to improve the quality of the work. Therefore, some conflict in the field is to be expected, as exists in all fields of human effort. The solution is to collaborate with those people who are working to advance the general field and who take a completely objective approach to the effort.

12) Another exchange between X3 and X2 (X3 in blue and X2 in red).

The November 13 DIA technology assessment makes clear that LENR experimentation has been rapidly expanding, and that this is bringing a "higher standard of experiment".

True, but I'm talking about the field policing itself by peer review. This process is happening as people exchange preprints and ask for critiques. Unfortunately, everyone does not do this and some take the criticism personally.

A comparable increase in resources has yet to occur on the theoretical side. Since Julian Swinger resigned from the APS over their hostile CF policy, people wanting to pursue the fundamental physics of LENR have had to be self-supporting.

True, with a few exceptions.

By "fundamental" I mean new simpler mathematical formulations that are correct by Occam's Razor. These do not require any new experimental LENR breakthroughs, though cleaning up the existing results will be useful both for theory and for being more persuasive to the larger scientific community. The theoretical breakthrough, can only occur, however, if the "newcomers" are granted enough resources to effectively collaborate on cleaning up relevant known fundamental theory. Their efforts to obtain these resources is currently at the stage of negotiating on the "shape of the table and the height of the chairs".

I agree, not much resource is being applied to theory. However, for a person to make a useful contribution, they need to master what is known, which is not an easy job.

When you talk about people "who take a completely objective approach" you are talking about an empty set. You by your own statement are strongly in the camp that new experimental results will be necessary.

By an objective approach I mean not being in love with your own ideas to the exclusion of other possibilities. I find many theoreticians are so sure they are right that no argument, no matter how well researched, has any effect on their approach. Of course, a person should be an advocate for an idea, but this has to have limits.

13) The issue of money was raised. Responding to this, X2 wrote: "If i were given 10 M$ to set up a laboratory and 5 years of guaranteed support, I would have no problem getting the people. This economy and the situation at LANL would provide many well trained and talented people if the lab were set up in Los Alamos or Santa Fe. This effort could also involve creating a department of LENR at UNM. No, the problem is the willingness to risk money on this subject. Money will flow into the field cautiously and it will be focused on the big laboratories such as NRL and SRI. Eventually, the so-called national laboratories will want a piece of the action. As this money produces success, more will enter the field and be directed by the conventional scientific establishment because they have the experience and the trust to properly direct money. However, a lot will be wasted because, as X3 says, a special knowledge set is required. The conventional money managers will not recognize this requirement. Talent is available, only awareness at the management and funding levels is lacking, at least in the US."

Added on 2/4/2010

14) As I often repeat, money and administrative support will probably materialize very quickly after at least one reproducible-on-demand demo of a nuclear process due to an atomic process is offered. An example of a nuclear process is emission of energetic photons or p rojectiles, changes in isotopic ratios, transmutation of elements, etc. An example of an atomic process is electrolysis or another chemical reaction, diffusion, glow discharge etc.

The best candidate for such demo seems to be creation of helium during electrolysis. This kind of nuclear transmutation was independently reported by at least three teams of highly qualified researchers, two in the US and one in Italy. These are the only studies in which excess heat and the nuclear effect were shown to be correlated. More than several MeV of excess heat is generated for each atom of produced helium. In one study (M. McKubre) the reported value was close to 23 MeV per helium atom. If I had the expertise needed to perform such investigations, and access to required instruments, I would certainly try to independently verify results reported McKubre, Miles and De Ninno. Focusing on reproduction experiments seems to be more desirable, at this stage, than searching for new CMNS phenomena.

Sustained commercial success--for example, in generation of excess heat, even without understanding of the mechanism, would also precipitate interest and support. Such scenario is possible, at least in principle. But it is very unlikely. I am thinking about the proverbial blind chicken trying to find a grain of corn by pecking randomly. Most new applications are now based on scientific understanding of underlying processes.

Added on 2/5/2010

15) The last comment above made think about two kinds of research projects, replication-oriented and discovery-oriented. The first kind is more likely to be successful than the second kind. Why is it so? Because in the first kind situation success depends only on the skill of researchers. To succeed in the second kind of situation one must not only to be skillful but also lucky (in choosing the topic). My thesis adviser told me this in France, nearly five decades ago.

My definition of “replication experiments” is broader than “strict replications” of what has already been done. It includes “analytical experiments” in which the goal is to learn something about a phenomenon know to exist, or to measure something. Trivial examples are (a) measuring the resistivity of a new alloy or (b) learning how that resistivity depends on temperature. What we need in CMNS field are strict replications verifying reported results, preferably in different laboratories (to confirm reproducibility).

By the way, most experiments performed in school laboratories belong to the first kind. The same is true for most experiments performed by professional researchers. A question is asked and an experiment is performed to answer it. For example, (a) “how does the cross section of a particular nuclear reaction depends on the kinetic energy of projectiles?” or (b) does a theoretically-predicted particle (such as neutrino) exit?” Success in performing such experiments depends only on qualifications of experimentalists. A success means to produce a convincing yes or no answer. Discoveries of unexpected things usually occur during analytical experiments. Most experiments in which I participated were analytical. Only once was I lucky to participate in a discovery of something unexpected.

Addead on 2/6/2010

16) Larens Imanyuel wrote:
“Since people have been accustomed to thinking in 4-D physics, they generally ask, "Where are all those new dimensions? when presented with a higher dimensional theory. In this posting I will present the answer to that question intuitively before I get into any formal mathematics.

3-D is exceptional, because the number of degrees of rotation equals that of translation. Instead of points, we can take helices to be the fundamental geometrical object, each with an axis of translation, a plane of rotation, and a phase. We may then use a three dimension array of numbers to develop the dynamics, which gives a 3 cubed = 27-D space. This is best decomposed into subspaces as 27 = 8 + 8 + 8 + 3. One 8-D space is the double of our conventional 4-D space, arising because helices, unlike points, exist in both left and right handed chiralities. Time and temperature are a new pair of dimensions. This pair easily admits a thermodynamic direction to time, unlike in conventional physics. Since in

27-D space there are multiple degrees of freedom for time, observed time is an average over them - the same as temperature is the average over degrees of freedom of energy. Time and energy are conjugate, of course, because their product is action.

The existence of three 8-D spaces may be associated with the temporal and philosophical triality of actors who exist in the present, observe the past, and act upon the future. The 16-D projection into the past and the future exhibits strong dualities in which there are equivalent ways of expressing the same physics. One of these is the particle/wave duality of conventional quantum mechanics. There is a cosmic one in which in one frame the observed universe is always the same size, and in another the universe is expanding from a Big Bang. A fundamental one is that the change from future to past may be represented by either the change in the sign of a number, or by switching between two completely different metrics.

The structure of one 8-D space describes the length preserving transformations in its associated 16-D space. Within a 16-D space one 8-D subspace represents a transformation of the other one. The Standard Model of Particle Physics may be derived from this structure. The triality appears alternatively as three generations of particle, and as the tripolar charges of the strong force.

The 3-D subspace of the 27-D space is a nonparticle space in which scaling effects more than one dimension at a time. For instance, scaling the area of the plane of rotation of a helix, will give you one nonparticle dimension in a set of three. This 3-D space is essential for the simple description of condensed matter science. Each dimension admits solutions to the general quantum three-body problem giving fractal binding energies. Canonical states of matter may be constructed using the known properties of isotopes and matched to important physical features. One of these relates to the geometrical mean between the largest and smallest times in the universe, i.e., the age of the Big Bang and the reduced Planck time. This ratio may be calculated from information theoretic considerations and matches observation to the accuracy of the condensed matter physics involved. Virtual states are needed to construct the cold fusion reaction. The "magic numbers" for atomic number and mass may be calculated from the number of elements in the 8-D space structure. LENR extends beyond these magic numbers, however, so a higher order theory needs to be constructed. Presumably this will use virtual light isotopes arising from heavier isotopes.

This theory differs from conventional string theory in using commutative operators over 8-D division algebra, rather than anticommutative operators over "geometric" algebra. By a long known result 27-D physics is an extension of conventional quantum physics. The closest conventional string theory is the exceptional 16-D compactification of the heterotic string."

17) Responding to the above I wrote: "I agree with Ed Storm that such mathematical speculations should not be part of CMNS struggle for acceptance. They should first be used to explain non-controversial experimental data. Mixing two not-yet-acceped considerations is likely to counterproductive. "

18) Larens Imanyuel responded"
"Ludwik, Science is not a popularity contest. All the declarative sentences of my "intuitive" explanation of 27-D physics are backed up by some type of more formal mathematical explanation. It is more likely to be proven correct than is the Higgs boson likely to be discovered. The double standard whereby the consideration of my theory is dismissed as "speculation" and the hypothesis of the Higgs boson presented as "fact" must be rejected itself for scientific progress to proceed. As I explained the 3-D nonparticle subspace of 27-D theory is essential for CMNS; and it does not exist in the old theory.

In case you haven't followed, the existence of the Higgs boson is being challenged by nonparticle Higgs mechanisms and the "un-Higgs" pseudo-particle, so conventional particle physics is having to get into this realm. There is no other serious, useful fundamental theory of physics other than the one I gave, because I have based it on fundamental mathematics. Another theory may use a somewhat different mathematical form, but must have basically the same physical content. Some of my intuitive statements will probably have to be modified once all the proofs have been worked out, but there is no good reason to think that the basic content will change much.

Only trying to first explain "non-controversial experimental data" would be a strategic disaster. To gain acceptance a

new theory must explain evidence that conflicts with old theory. All such evidence is by nature "controversial", because it does conflict with the old theory."

19) Ludwik responded: "I am probably not the only one on this list who knows very little about Highs boson or string theory. That is why I think that such topics do not belong to CMNS. The same applies to other exotic topics: vacuum energy, magnetic monopoles, etc. Such interesting topics are, or should be, debated outside of CMNS. Let us focus on four nuclear claims: (a) transmutation of elements, (b) changes in isotopic ratios, (c) emission of energetic radiations (alpha, beta, gamma, protons, etc.), and (d) generation of unexplained excess heat. What is wrong with this suggestion? What can be gained by making string theory part of CMNS? "

Responding to a comment made by another researcher, I added: "Yes, we do need some kind of a limit. But where should the limit be placed and how to implement it in practice? By the way, I do understand creative people's desire to share. Seeking readers and listeners is part of human nature. And I do not mind reading what they have to say, provided I able to follow it. The message to which I was responding was indeed quite interesting."

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Added on February 12, 2010

20) The discussion initiated several days ago restarted when X5 asked "Larens, how about educating an old man a little. Can you define for me, each of the 27 dimensions which are involved?" Responding to this I wrote " I know that there is a deep answer. But let me give you a naive one: Larens will probably reject it.

Suppose we use Newton's frame of reference; it is fixed with respect to distant stars. You need only three coordinates to identify position of the center of gravity of an object. To deal with velocity and acceleration of the center of gravity you also need time; that is the fourth coordinate. You also need coordinates associated with possible orientations of a rigid object. Suppose the object is not rigid, like who rigid spheres connected with a spring. You need to describe possible oscillations. Suppose the object is biological, for example a human being. Then you need a coordinate for the body temperature, a coordinate for heart beating rate, a coordinate for the rate of sweating, and coordinates for rate of breathing, digesting, thinking, etc. "

21) Responding to someone else Larens wrote: "Part of the problem is the difference in the meaning of "dimension" between math and physics. What physicists call "degrees of freedom" are often called "dimensions" in math. In mathematical physics the number of dimensions of a system is generally the number required to get a "true representation" of it.

A similar confusion occurred when Einstein extended the use of the word "dimension" from 3-D "space" to 4-D "spacetime". "Temperature" is a collectively defined variable, which may be included as a "dimension" along with time when we extend from "particle" physics to "condensed matter" physics. We need to double the size of 3-D space when we replace translations by rotations to get a realistic local action. Each rotation is coupled with a nonzero differential translation to get a helix. The nonzero condition arises, because with QM no particle with a finite wavelength can have zero momentum. The space is doubled, because helices come in two different chiralities.

The resulting 8-D space is tripled to 24-D by the "strong triality" of division algebra, but the 24 dimensions cannot be labeled "exactly" in conventional terms, because of multiple interpretations. One of the simplest to understand is that there are three "generations" of particles differing only in mass. The nonparticle subspace of 27-D physics is a 3-D translational space, the same as conventional 3-D space, but applies only to condensed matter physics and not to particle physics."

22) X6 responded: "Thanks, I now understand. In this definition, an equation of the form X = A +By +Cy^2+Dy^3 + Ey^n would have n dimensions. By 27-D you mean that 27 individual quantities are needed to define all aspects of a material. Being a chemist, I suggest this is a gross under estimation."

23) Larens responded: "You are coming from a position of dogmatic Western dualism. This is a temporary cultural

phenomenon arising from the fact that the physics of Western culture has not yet been mathematically sophisticated enough to address the existence of consciousness in the universe. It is time to move on.

Western physicists have also insisted on putting the labels of "length" and "time" on mathematics that is intrinsically "dimensionless". Correcting this problem results in multiple interpretations of the mathematical dimensions - which are in turn denied, because the physicists are not yet philosophically aware of their problem. To give an "exact" definition of all the dimensions will probably take mathematical physicists several decades. . . .

I have stated that my theory includes "actors that observe the past, act upon the future, and relate to one another in the present." This statement includes the concepts of cognition and volition that define consciousness. .....I stated earlier that 27 is the necessary and sufficient number. To get into a detailed explanation would require more mathematics than I can get into in an "intuitive" explanation of the physics, for my audience is not familiar with the mathematics.....I explained earlier that I am using the definition implicit in modern geometry, but that these dimensions may relate to different meanings in terms of conventional physics."

24) Ludwik wrote: "Conflicts can often be avoided when different meanings are not assigned to already-used words. In this forum the term 'dimension' should be used as defined in physical sciences. We refer to four essential dimensions: L, M, T and I. In the now-used system of unit (SI) these four dimensions are represented by meter, kilogram, second and ampere. Introducing another meaning to the word dimension can create confusion.

25) Larens responded: "By introducing another standard usage of the word 'dimension', different from either the way Ed and I were using it, you have highlighted one of the fundamental problems of language. No one can demand that their particular use of a word with multiple definitions is the 'correct' one. Everybody needs to be aware of potential confusion and to define their terms."

26) X6 responded: "No, the definition of words is universal. It does not matter what "dogmatic" view a person has, concepts cannot be communicated unless the words have shared meaning. I find that people who have no idea what they are talking about want to keep the meanings ambiguous so that their ignorance is not revealed. I'm not suggesting this applies to you. I agree, many things about this world are not easy to define because so little is known. However, simply throwing out a collection of arbitrary words does not help this situation. For example, if you use the word consciousness, then I assume you know the meaning you intend this word to have. I can understand what you mean only if you give a definition. To me, the word means being aware of self and being able to demonstrate that awareness. A rock, for example, is not conscious because it has no means to be aware of self. Even a dead person is not conscious even though he might be having an after-death experience because he cannot communicate that experience. What is your definition of consciousness?

I take your point and an exact definition of all dimensions is, I agree, too much to ask. However, some basic definitions would help. Simply stating that 27 dimensions exist is not helpful unless you have some reason to believe this is true. Why choose 27? Why not 50? No matter what number you chose, each example has to be described by the same general definition of dimension. What is the general definition?"

27) Ludwik added (referring to his last message above) : "We often use x and y axes to show how one quantity depends on the other, for example, acceleration versus time. In that case dimensions are different along each axis, [(L/T^2) for acceleration and (T) for time. But this is no longer true when coordinates are used to represent space, either 2D or 3D. In this case all dimensions must be (L).

The fourth dimension, introduced in the theory of relativity, is said to be time. This is not true. The forth dimension is c*t, where c is the speed of light. In other words, all four quantities are dimensionally identical; their dimension is (L). Is the same true when one deals with the 27D space?

28) Larens responded: In 27-D physics dimensions are labeled by factors of action or their inverses. Multiplying all the dimensions together should give you the number one. You may also interpret all these labels as coming with constants of proportionality in the manner you did with c, so that each dimension is equivalent.

29) Ludwik: I have no idea what the factor of action is. It is obvious that I am not prepared to understand Larens. The same is probably true for most people on this list. He should address people with the same theoretical backgound. Why is he wasting so much effort on us?

30) Dean Sinclair wrote. " . . . As to different Chemistry and Physics approaches, the theoreticians of both "Fields" tend to cloak their ideas in abstruse mathematics. The more abstruse it is, apparently the more prestigious it is. Actually, the separation of the field of science into many disciplines obscures the fact that all are examining the same thing, the mystery of existence."

31) Larens responded: "Ludwik, I am sure you understand what a "factor of action is". It is just that you have not seen the phrase before. Length, area, time, mass, velocity, momentum, energy, action, etc. are all factors of action, with action most often being factored as momentum times length or energy times time.

32) Ludwik: Why does he need a new term for what is usually called physical quantity?

33) Larens: " 'Physical quantity' is a more comprehensive term than 'factors of action'. Electromagnetic units are physical quantities, for instance, but not factors of action."

34) Ludwik: As far as know, electromagnetic units (such as volt, ampere, ohm, tesla, or V/m) are not physical quantities. They are used to evaluate physical quantities (how much, how strong, etc.).

35) Larens: "As long as we are splitting nanowires - unit quantities are a subclass of quantities."

36) Ludwik (not posted). I know what units are but the meaning of the new term "unit quantity" is not obvious to me. Why do we need it? I do not want to argue about definitions. That is why I am giving up responding. What the CMNS field urgently needs, at this time, are reproducible experimental results demonstrating reality of chemically-induced strong nuclear process. Speculations should be based on such results.

37) Responding to another researcher, Ludwik wrote:
"A mathematician does not need experimental data; s/he can make any set of assumptions and derive resulting consequences. Consequences are valid, unless there is a mathematical (logical) error in the derivation.
But absence of logical errors in a derivation is not sufficient in physical sciences. In that area theoretical derivations are evaluated by reproducible experimental data. Theoretical physicists make predictions and experimentalists test them, in order to validate theories. That is what X7 is saying. I agree with him. We are not mathematicians."

I made this observation before and no one objected on the list. I wish the CMNS forum were dominated be comments about new and old experiments, and by testable theoretical predictions. Unfortunately this is not the case.

Added on February 20, 2010

Larens continues posting messages which i do not understand. But that does not mean that his observations are not valid. I would be even more confused by reading observations of well known theoretical physicists, such as Pauli and Dirac.

38) Responding to X8, Larens wrote:
"The 'strength' of a particular reaction is a function of the 'range' and 'coupling constant' of the relevant gauge boson. 'Range' and 'coupling constant', however, are mathematically independent concepts."

This prompted me to ask the following question: "I suppose that the term 'strength' is the same as cross section, usually expressed in barns. But what is the 'coupling constant' and in what units is it expressed? How is it determined for typical well known reactions?" I hope this question will be answered tomorrow. I know what the spring constant is (expressed in N/m), for example, when it couples two macroscopic objects. Perhaps Larens' model consists of nucleons connected by linear springs. That would be a reasonable model. But he did not say this.

Meanwhile Larens posted another message: "OK, here is a question for the experimenters: What should theory provide for the experimenters, so that they may increase the reproducibility of LENR enough to support the design of commercial equipment? Your answers may help the theorists set their priorities in a field where theory is quite difficult." That is a good question. Pauli, for example, predicted existence of particles, named neutrinos, and described their properties. Knowing what to expect experimentalists were able to design experiments confirming existence of predicted particles. Hundreds of experimentalists, all over the world, have been studying properties of neutrinos for many decades. Neutrino experiments, for example at BNL, are reproducible-on-demand.

Responding to Laren's question (about how to help experimentalists), X10 suggested that theoreticians should tell us how to maximize the reaction rates. That would indeed be very useful if experimental results were reproducible. Unfortunately, we are still waiting for this luxury. For the time being the most important task of a theoretician (who wants to help) is to tell us what to do to make experiments reproducible. In other words, to use the term NAE (Nuclear Active Environment), introduced by Ed Storms, a theoretician should tell what the NAO is and how to create it. Naming something is not the same as knowing what it is..

Added on February 21, 2010

Not surprisingly, **Storms responded:**
"This is a good question, Larens, and at the risk of wasting your time, I will provide an answer. A theory must do the following at least:

1. Identify the materials and real-world conditions required for the LENR mechanism to operate.

2. Describe a mechanism that is consistent with all observed behaviors both with respect to LENR and to all other fields of science.

3. Describe a mechanism that does not violate any basic laws of Nature.

4. Identify behaviors uniquely related to the mechanism that can be clearly tested in the laboratory.

5. Provide a clear description of the mechanism with a rational justification of its reality. This is necessary to justify the investment of time and money to test the claims.

Without #1 being satisfied, experiments cannot be made reproducible. Without #5 being satisfied, no one will take the time to test a theory. So far, no theory has satisfied these two essential requirements. Most violate some or all of the other requirements.

I say this after having taken the time to evaluate all published theories. This has resulted in a paper that might be published someday. Meanwhile, Brian and I are searching for a mechanism that does satisfy all of these requirements. This is a difficult task unless a very open mind is used to explore ALL possibilities. I suggest, professional theoreticians have largely failed to find a useful explanation because of their obsessive commitment to a particular approach, even in the face of their failure to explain essential behavior. Theory needs the same open mind that we in the LENR field insist skeptics show when evaluating the claims based on experiment. In addition, theoreticians need to explain their ideas with tolerance and care so that their ideas can be understood and applied by experimentalists."

39) Another experimentalist, Dennis Cravens, wrote:
As an experimenter, I look for definite predictions of theories. Predictions connected to my "knobs" that I can turn. Theories that say that the reaction is via particle X, Band structure Y, nuclear reactions Z do not help me too much. I what to know things like: does it predict to turn up my temperature, increase currents, make smaller points on my cathode, make materials with magnetic properties, add some materials that allow for spin exchanges, use light of a given frequency, avoid material X, make particles of size Y, pulse the current, impose a temperature gradient, impose a magnetic field, ……

After most theories I read, I see nothing that helps me know what to do in the lab. Most don't seem to give me

anything I can use in the lab. Give me nuts and bolts.

In short, give me a theory that makes predictions and is connected with physically real and obtainable conditions and materials. Which knob do I turn and how far.

40) Edmund Storms wrote:
". . . Physics is based on a mathematical view of reality from which easily verified predictions can be made, i.e. either a behavior, event or consequence exists or it doesn't. Chemistry or more exactly materials science is different. Such systems are so complex that mathematical predictions are very limited. Nevertheless, the mathematicians make an effort. The result, especially in LENR, is not very useful because it is not related to the real world of knobs, as Dennis says.

Math is based on assumptions, which some people simply ignore. The mathematical equations simple extend and connect these assumptions. If the assumptions are wrong, the math has no meaning. So, I suggest we start examining the assumption for a real connection to reality."

Unless reproducibility on demand is achieved, the future of cold fusion can be described as in a little poem below. Yes, I know that not everything that rimes can be called a poem; I am not a poet. Note that in this context (as opposed to the context of my just-published autobiography, from where it is extracted),"good" stands for "promising expectations" why "evil" stands for "disappointments." Both theoerticians and experimentalist should focus on the absence of reproducibility on demand.

Both good and evil will survive.
To fight each other and contrive,
To show and hide, and to refuse,
To offer something and confuse.

To give and take, to kiss and bite,
To make and break, and to excite.
To promise something in the sky,
To ruin hopes and say good-bye.

To feed and starve, to love and hate,
To burn, to smash and to create.
To wreck, to torture, to destroy
To build, to cherish and enjoy.

Ludwik Kowalski
December, 2009.

41) Larens wrote (responding to my question):
" 'Coupling constants' are dimensionless numbers that express the strength of fundamental reactions with "sizes" being determined by QM relations. The problem is that they are not really "constants", but are functions of energy with different functions for different forces. There is presumed to be a grand unification energy at which the functions for EM, weak, and color forces converge to a single coupling constant. This is far above the energy of any existing instrument, so values of different functions extrapolated to zero are generally given. EM is particularly simple, so extrapolation only gives the fine structure constant."

I am lost again. In any case, the adjective 'constant" should not be used to describe something that is not constant. I still think that the reaction stregth is represented by its cross section.

Larens' 27-D theory should first be tested by comparing its results with reproducible-on-demand data, such as fission cress sections, scattering of neutrons, nuclear transmutations at high energies, etc. Consider the n+U235 fission. The cross section, for neutrons of very low energy, is several hundred barns. For neutrons of higher energies, such as 2

MeV, is becomes close to one barn. How would Larens' theory explain this? Similar questions can be asked about the energy dependence of cross sections (i.e. strengths) of other nuclear reaction. That is what I would do to promote a theory. I do not think that promoting it by using the CMNS results will be successful among typical theoretical physicists. I would discuss the theory on their websites, not of the website where most people are not theoretically oriented.

42) Storms wrote (responding to Dennis):
Physics is based on a mathematical view of reality from which easily verified predictions can be made, i.e. either a behavior, event or consequence exists or it doesn't. Chemistry or more exactly materials science is different. Such systems are so complex that mathematical predictions are very limited. Nevertheless, the mathematicians make an effort. The result, especially in LENR, is not very useful because it is not related to the real world of knobs, as Dennis says.

Math is based on assumptions, which some people simply ignore. The mathematical equation simple extend and connect these assumptions. If the assumptions are wrong, the math has no meaning. So, I suggest we start examining the assumption for a real connection to reality.

Larens responded:
a) Ed, You have made a good summary. A good theory can make rough phase diagrams for ideal materials. The material scientist can then take these, learn how to compensate for non-idealness, and measure specific values for the phase transitions. These can then be used to refine the theory to improve its usefulness. A theory that can explain the 0.875 loading threshold for PdDx has an excellent point of connection with LENR reality, for instance, if it also has good reality connections to the rest of the universe. With at least one such excellent reality connection, there is good hope for it to be predictive of previously unknown phenomena.

b) One cannot expect too much to come out of the theorists rapidly, however. To give an example, in 1955 a group of mathematicians set out to classify all the finite simple groups. After about 500 papers by about 100 authors totaling tens of thousands of pages they announced success. Some holes were discovered. These were patched by 2004. A simplified theorem of only about 5000 pages is now being worked on. The classification includes 27 "sporadic" simple groups (if one includes the Tits group). "Sporadic" means "not in an infinite family" and is equivalent to the word "exceptional" elsewhere in math. It is conjectured that the number "27" is not an "accident", but relates to the 27-D physics I mentioned earlier. This suggests that what we are likely to see in the near future will not be a complete "proof" of principle, but will be a hierarchy of theories that are increasingly ad hoc as they approach the laboratory situation.

c) Chemically assisted nuclear transformations clearly involve energy transfer between different nuclear reactions, because otherwise some of the reactions would violate the conservation of energy. What we need is a joint accounting of all the isotopes before and after a run along with the energy. The complexity of this is comparable with that of contemporary high energy physics, but the equipment for LENR will be much less expensive. The complexity of results should be a good match for the complexity of the theory, so reasonably rapid discovery and development should occur. The LENR community needs to discuss a general plan to obtain and utilize the necessary resources. The offer of resources in the 2004 DOE report might be a point of leverage for discussing the matter with the larger scientific community.

43) Tom Barnard wrote (responding to an earlier message of Storms):
Ed, I have been doing this a long time too. I have read your book. I make judgments, based on my own long experience, on what to believe. You may not like my judgments, I don't like many of yours; we're even. Most experiments are not vetted and not repeatable, so one has to be extremely leery. There aren't too many experimental results in this field that I would call "facts"; just me and my hard won experience talking. I appreciate the criticism. Keep up the good work.

Ed Storms wrote (responding to Larens):
" a) The PdD0.875 threshold has no relationship to the mechanism because this is the average composition of the cathode, not the composition where the effect actually occurs. The challenge is to identify which property is important

to a mechanism. Is it the deuterium content, the availability of important energy levels, or a magnetic effect, to give a few examples? Unless the important basic property is identified, too many materials become possible candidates.

b) I agree, the process will be slow and filled with false starts. However, some approaches are useless and should not be used to waste valuable talent and money.

c) We now know that helium production is the main nuclear product and that the energy released is close to that known to result from D-D fusion. The big issue is the unique condition that initiates the reaction and how energy is released to the outside world. Unfortunately, money is not available to explore all of the possibilities as would be the case if this were an accepted phenomenon."

Added on February 22, 2010
There were an avalanshe of messages, most of them about theories. It is not my role to record all the. Future investigators will be able to find them in the list's archive. But how can I resist quoting four papers posted by experimentalists?
44) Jack Dufour wrote:
"Dear Andrew, I think the point you raised (coupling constant of the strong force) is well documented. For distances between nucleons higher than 1 fm, experimental data are well fitted with $g^2/hc = 14{,}5$. This has been fully accepted by the referees I am discussing with. See also "Le monde subatomic" by Luc Valentin -Hermann (1986) tome 1 p.62. For lower distances, this is no longer valid, but d/d nuclear reactions imply distances higher than 1 fm. The coupling constant of the interaction mediated by the neutral and virtual electron-positron pair (range $h/2m_ec = 193$ fm) experimentally determined in the alpha disintegration case (which I did) is $3.8 \times 10^{-6}\ g^2$ (# $8 \times 10^{-3}\ e^2$). You cannot invoke higher values, otherwise you get trouble with the very well experimentally documented alpha disintegration case. So the (hypothetical) Yukawa plays only a role for energies of the deuteron of a few keV (same order of magnitude as the electrons screening). Its role is totally negligible at eV (room temperature) levels. (NB In the formulas I wrote, h stands for h bar.)"

X9 wrote:
I'm at a loss to understand your attitude, Larens. I simply told you a fact. The value you quote is the average composition of the cathode and it is a fact that the reaction is not occurring at a composition equal to the average. It does not matter to me if you believe this or not. It only matters that you understand what I actually said. Also, I have no theory and I offered none. I simply suggested several examples of properties that might be important. I'm willing to discuss CF with you, but I expect in return you will trying to understand what I'm actually saying.

P.S.
Excuse me for being unclear.

My comment concerned the statement, "The reality is that there is a tight correlation between heat output and loading above the 0.875 threshold", which was then disputed. The statement is fairly accurate, albeit I do not believe this is known to three significant figures. As to the 27-D theory, it or any other theory will have to 'put its neck on the chopping block' based upon experimental data.

Ed Storms wrote:
Tom, I share your definition of like and dislike. Of course, some data is better than others and some data is clearly wrong. That is why I rely on patterns of behavior that is supported by many studies. In addition, I expect people will have different opinions about what to accept and what to reject. I only ask that these opinions be based on logic and fact, not on emotion. My role has been to examine all the published information and apply objective criteria to its evaluation as much as possible. In my comments to you, I'm only trying to share this evaluation. Of course, you are free to accept or reject the information. Nevertheless, people in the field and especially theoreticians need to agree on what is real and what is imagined in both theory and experiment. Otherwise, we will all be going off in scattered directions with no hope of arriving at agreement.

I agree, the meaning of the word 'fact' is sometimes in the mind of the beholder. However, we need some way of distinguishing information that has good support and consistency with scientific understanding from opinion. In my

case, I try to use information that can be well defined and I call this 'fact'. If you disagree, please explain why you think something I said is not "fact" and I may change my mind.

Michael McKubre wrote:
"All, The correlation between the ability of a cathode to attain and maintain "high" loading (>8.5-9.0) and the capacity to produce excess heat was first reported simultaneously and independently by K. J. Kunimatsu's group at IMRA Japan an my group at ICCF3 in Nagoya. This result has significance both as a means of explaining earlier null results and, as Larens notes, as an insight into potential mechanisms.

For the latter it is worthwhile perhaps exploring a little further what has been reported and what that might mean for the lattice condition that facilitates or hosts excess energy production. The resistance ratio measurement that we co-opted for use to determine the D/Pd loading necessarily measures a bulk average value. In the SRI experiments (for the most part) we kept the cathode as uniform as possible* and the diffusion coefficient of D in Pd is very high (>>10^-7 cm^2 s^-1). For both reasons, at the time of interest (after long, slow loading), the activity of D in the Pd is rather uniform throughout the length and depth of the cathode material so that an average value has general and specific significance**.

Although I am not going to cite any data or sources here it seems clear that the energy producing reaction occurs not homogeneously throughout electrodes (no matter how uniform their environment) but rather in small, discrete, "special" places***. What causes these "special zones" to become specially effective after very long times at effectively uniform deuterium activity? Is it something intrinsic to the lattice - already built in? Or is it something that grows in the lattice or on the surface with the long initiation time?

I have some ideas but do not have the answers. I do appreciate the "new thinking" provided particularly by Andrew, Tom & Larens, under pedagogical probing from Ed. It is crucial to understand the environment in which the effect occurs, and then to understand what triggers it. Given both, theory will seem (in hindsight) to be obvious. But it is precisely in getting to that place of understanding that theory can help most in guiding experiment.
-Mike
- - - - - - - - - -
* Ed mentioned lattice geometry effects, and there does seem to be a difference in threshold (and consistency of heat production) between wire and foil cathodes that I attribute largely to the intrinsic inhomogeneity of current density and loading (and deuterium flux) of foil cathodes in "sandwich" anodes compared with wires with axially symmetric anodes.

** This is easily modeled. Anyone in doubt should calculate (say) the deuterium flux needed to sustain a compositional gradient of ±0.01 D/P for a 50 µm thick foil (or even 1 mm dia. wire) - and then recognize what that flux is doing to load or unload the Pd.

*** Thermal imaging, helium production (and partial retention), and "common sense" place this zone at or near the surface; Ed has called it the "Nuclear Active Environment" or NAE."

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ADDITIONAL MESSAGES FROM LARENS (INSERTED LATER AT HIS REQUEST)

On Sun, 21 Feb 2010 18:07 -0700, "Edmund Storms" wrote:

The $PdD_{0.875}$ threshold has no relationship to the mechanism because this is the average composition of the cathode, not the composition where the effect actually occurs. The challenge is to identify which property is important to a mechanism. Is it the deuterium content, the availability of important energy levels, or a magnetic effect, to give a few examples? Unless the important basic property is identified, too many materials become possible candidates.

Larens wrote:
You provide a great example of disconnection of theory from reality. The reality is that there is a tight correlation

between heat output and loading above the 0.875 threshold. You declare as a "fact", however, that this correlation is not related to causality. You insist without any supporting evidence that there is a "mechanism" elsewhere where the "effect actually occurs". Your theory moreover does not even offer a clue as to whether "deuterium content, the availability of important energy levels, or a magnetic effect", or something else is important to this hypothetical mechanism elsewhere.

On Sun, 21 Feb 2010 22:10 -0700, "Edmund Storms" wrote:

I'm at a loss to understand your attitude, Larens. I simply told you a fact. The value you quote is the average composition of the cathode and it is a fact that the reaction is not occurring at a composition equal to the average. It does not matter to me if you believe this or not. It only matters that you understand what I actually said. Also, I have no theory and I offered none. I simply suggested several examples of properties that might be important. I'm willing to discuss CF with you, but I expect in return you will trying to understand what I'm actually saying.

larens wrote:

The problem is that you are assuming a conventional 3-D geometry, and then assuming your conclusions thereof are a "fact". I choose to use the 3-D "nonparticle subspace" of 27-D physics. In this case the average composition of an electrode controlling behavior apparently at its surface is not a contradiction, but is the preferred interpretation ofevents. My choice of 27-D physics is not to fit this particular case, but is to provide explanations for many phenomena that do not have a plausible explanation using conventional geometry and physics. Your assumption of conventional physics leads you to no theory for LENR and therefore no way of optimizing it as a technology.

Added on March 2, 2010

I am skipping ma lot messages, nearly all of them theoretical and without obvious connection to the CMNS field. Some of these messages contained personal insults. One prominent CNMS contributor declared that he will stop posting comments. Here is my contribution; it was a reply to X10:

44) Ludwik Kowalski wrote:
1) As often emphasized by Ed, in order to be accepted, a new theory must not conflict with those theories that have been successful in explaining and predicting real things. Special theory of relativity, as you certainly know, is in perfect agreement with Newtonian theory at low velocities. I agree with Ed that everyone who has a new theory should first use it to explain well known facts (such as elastic and inelastic scattering, alpha decay, thermonuclear reactions, fission, etc.).

2) Elastic scattering is not the only phenomenon demonstrating the reality of Coulomb barriers. Why do you think the half-life of alpha decay, T, depends on the energy of the emitted particles? Typically, T is longer when energies are lower. I am saying "typically" because T also depends on other factors (spin, parity, etc.). Without Coulomb barriers all alpha-unstable nuclei would decay immediately (after being formed). For high energy decays (emission of alpha particles with higher energies) the barriers are less high. That is why tunneling probabilities are higher (than when decay energies are low). This tendency, known as Geiger Nuttall rule, was recognized before Gamow's theory of tunneling was formulated.

3) I also agree with Ed that everyone who has a new theory should first use it to explain well known facts (such as elastic and inelastic scattering, alpha decay, thermonuclear reactions, fission, etc.). I see three advantage in doing this:

a) You would be sure that experimental data are more reliable.

b) It would be much easier to promote your theory , i.e. to publish papers in widely circulating journals. Editor will not reject your papers without sending them to referees, as they often do with CMNS-related papers.

c) You would be addressing people who know theoretical physics, and mathematics, much better than most of us on

the CMNS list.

Added on March 8, 2010

Once again I am skipping I am skipping a lot messages.

45) Ed Storms, responding to Larens, wrote:
I see we come at the problem from different directions. You look at the problem from the bottom-up like a basic scientist and I look at it from the top-down like an engineer. I identify the environment first because the effect is obviously very sensitive to the environment. In contrast, the nuclear world that you use as your basis is not sensitive to the environment, hence the environment can be ignored. Cold fusion is an anomaly because it combines nuclear reactions with the chemical environment. As a result, nuclear physicists and chemists are needed but are incapable of communication with each other because they use entirely different concepts and words.

You probably have not spent much time in the laboratory trying to study the CF phenomenon, so that you don't appreciate the role of materials as much as I do. It is obvious, no matter what you want to measure, an instrument must be used for the measurement to be made. But first, you need to decide which behavior should be measured. So, I first ask you, what behavior should people measure? People have measured the deuterium composition, the power production, the various radiations, helium-4, helium-3 and tritium. People have looked for superconductivity, magnetic effects, and interaction with laser light. What property should we look at next? What property do you think is most important to understand?

But, to justify setting up expensive equipment, a person must be confident the phenomenon can be made to occur. That is the most serious problem right now. You say that "The characteristics of materials seem mainly to be whatever is necessary to create the necessary densities and fluxes". Yes, but what are these materials? That is the question I'm trying to answer. You ask for an accounting of energy. How would you suggest this accounting be done? What instruments should be used? These are the kind of answers I need in the laboratory before I can provide you with the answers you need. I'm must happy to provide information so that you can answer your questions, but I need my answers first.

Larens responded:
I look at the problem comprehensively like a basic scientist. You look at it with a narrow focus like an engineer. I defined the problem as LENR, with its many reactions. You redefined the problem as "cold fusion", a narrower concept that you are personally interested in. I use the entire universe as my basis, since I must maintain consistency with all proven math and physics. You, with your tunnel vision, can only see the nuclear part of my universe.

I understand the importance of your question, "What behavior should people measure?" Before that, however, is the question, "Of which phenomenon should we measure the behavior?" LENR is too complex for us to build a comprehensive model without setting priorities. Using existing data we should build a phenomenological model until we see that there are key questions that need to be answered with new experiments. With the new data we can further build the model, thus iterating the research process. At the moment the existing data is incoherently scattered about, because too many people have been pushing their own pet ideas to the point of suppression of more important ideas. Before I can answer your questions with confidence, I must stop finding so many dusty corners with important data. In the meantime, people's suggestions as to what they think is most important for model building are most welcome.

Ludwik wrote:
Neither theoreticians nor experimentalists should think in terms of their own superiority. Mutual respect and mutual help are needed. It is difficult to become a good experimentalist; it is difficult to become a good theoretician. Theories must be validated in terms of experimental data and experimental data must be explained in terms of theoretical considerations. We need each other.

Responding to Larens Storms wrote:

Once again Larens, you can't seem to restrain your judgmental compulsion. First of all, I don't define the problem with a narrow focus. To me, the title "cold fusion" is only a convenient description and not meant to be limiting. My focus is as wide as yours. The difference is one of practically. In my mind, we have to learn how to crawl before we can walk. A broad basic theory, similar to a philosophy that can't be applied, is useless. We need to start with ideas that have a direct relationship to observation. Second, I agree, the explanation must have mathematical consistency and be consistent with what is already known, as I have said many times. The challenge is to get to this universal goal. Our conflict is over the best path. So, please suggest a useful path and drop the these irrelevant judgments. You have a lot of scientific knowledge that this field could use if you are willing to debate and teach. So, please help us find a useful path.

Agreeing with the above, X wrote:
". . . For a man with a hammer, every problem looks like a nail. ..... ".

Responding to X Storms wrote:
I agree, no published theory has been useful. However, this is not the whole story. Everyone who does experimental work always has a model in their head that guides their work and is used to understand the results. These models are never published until the observations demonstrate that the final version is correct. Generally speaking, the person who has the most adaptive model that is close to reality wins the race. In my case, I'm often amazed how often the results are totally unpredicted and, as a result, send the work in a new and unexpected direction. My analogy is the exploration for gold. We are prospectors who are searching in random directions. Occasionally, largely by chance, we find a nugget. Once this happens, all the other prospectors rush to the spot and start digging to find the ore body. So far, the ore body remains elusive. Hopefully, someone will find a map to its location. So far, all the maps have sent people in the wrong direction and many have even ignored the locations of nuggets that were found. To carry the analogy further, some people even want to suggest that the discovered nuggets are not even real gold. sigh

Larens wrote:
I see that Ed is taking my "modest proposal" satire in earnest by expanding to the hijacking and derailing of this thread. Per my satire he rephrases my ideas and claims them as his, then creates a "conflict" by claiming there is a basic "difference" between our two statements. He does this well enough to get Bill to agree with "him", even though Bill is really agreeing with the basic ideas that I first presented. Having claim jumped my ideas Ed can then assert that I am not presenting anything "useful". He then presents a gold digging analogy to focus on the incoherence of the situation, thus derailing my attempt to have a thread which presents a "useful path" of coherence to CMNS by which to build a phenomenological theory.

Ed has previously stated his motives for doing this. He believes that LENR can be explained by conventional physics and that discussing the proposition that it requires new fundamental physics will inhibit the acceptance of LENR phenomena by the scientific establishment. As an editor of scientific journals he feels justified in engaging in rhetorical warfare on CMNS to block the discussion of such new physics. He has a highly committed position, for he is willing to ignore - that to the degree that new physics is required to explain LENR - he is destroying the possibility of the scientific establishment accepting LENR as theoretically well founded.

To solidify his position he takes my invitation for people to offer "what they think is most important for model building" and twists it to demand that I as theorist must offer all the suggestions. He offers no suggestions - demanding that I must tell him how energy "accounting be done" and "What instruments should be used?" - even though research engineers who have hands-on experience with contemporary equipment are generally the ones best able to handle these details of experiment design.

Ludwik wrote:
Let us face reality--the CMNS field is still waiting for a recognized reproducible-on-demand demo of a strong nuclear process due to a chemical process. That is why we have no choice but to lean on already-validated theories. Doing anything else would amount of combining two unverified things: experimental claims, and theories. This seems to be a bad strategy; it is like one blind leading another blind.

Unverified theories should FIRST be tested on experiments which are reproducible. That is what I would I do first, if I

had a new theory and wanted to offer it as a guide for experimentalists in the CMNS field. What is wrong with this position?

Lorens wrote:
Ludwik, The reality is that when establishment mathematical physicists see the type of explanations on CMNS that "lean on already-validated theories" they refer to them as "handwaving", or perhaps some more derogatory term, and to the people who believe the explanations as "idiots". You might as well be saying that we have "no choice" of strategy but to shoot ourselves in the head.

I have been pointing out that the successful extension of fundamental theory is probably too difficult to get good short term results, and that the proper way to deal with this is to build a good phenomenological theory. Establishment mathematical physics will be able to understand this problem and respect this strategy, because they well understand the complexity of modern physics.

There are two things wrong with your proposition that new theory "should FIRST be tested on experiments which are reproducible", and not in the CMNS field:

1) CMNS experiments ARE largely reproducible, so are a valid first target for new theory. To think otherwise is to concede a lack of confidence in the field.

2) The ONLY tests that validate a new theory are ones in which the predictions differ from the old theory. The relevant fields of testing are a small set, and all suffer problems of acceptance because they differ from the conventional wisdom. One of the best ways to pick a field is to find one where there are strong economic rewards for success. CMNS is clearly the best field to choose in this regard.

Ludwik, responding to Lorens

*) I wrote: "the CMNS field is still waiting for a recognized reproducible-on-demand demo of a strong nuclear process due to a chemical process.

*) You wrote: " CMNS experiments ARE largely reproducible."

1) Which demo, according to you, is recognized (by mainstream scientists) as "reproducible on demand?"

2) What is wrong with the idea of FIRST validating your theory by using recognized reproducible-on-demand data?

3) What is wrong with FIRST presenting and defending your proposed theory in journals for theoretical physicists (where it is more likely to be understood)?
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Ed Storms wrote:
Since, thanks to Larens, the discussion has drifted from science to philosophy, I would like to add my two cents to what your have described. You raise the issue of whether theory at some level has to have a relationship to reality or can stand on its own as a consistent mathematical construction. Once this construction has been created, it is maintained as a representation of reality only because the theoreticians who accept this construction will not allow any other construction to take its place. They use denial of publication and personal attack, as Larens is good at doing, to discourage debate.

I suggest the solution, as has been the case in all other fields, is to obtain so much experimental information that the accepted construction cannot stand. A change in attitude is not based on a proof, as Larens describes, but on an accumulation of so much consistent information supporting a different conclusion that rational people gravitate to the new idea. This gravitation is gradually occurring in CF. For example, the idea that clusters of deuterons are involved was not considered initially, but now is being accepted as producing fusion as well as transmutation. This is progress and it is one small step toward a basic theory. This means that a theory must examine the nature of the cluster in addition to addressing the nature of the nuclear process. Once this idea is accepted, all theory of LENR will take a

different path and be more successful.

Larens responding to Ludwik (point by point) :
1) None of them [demos] are, because mainstream scientists have been mostly seduced into a fictitious world where all CMNS experiments are the results of incompetence or fraud, and are never published in peer reviewed journals. When dealing with such fantasy, one must focus on the best means psychologically for disintegrating the fantasy. Pointing out clearly unjust machinations of editors is much better than trying to find "perfect" experiments, because experiments are relatively expensive and more difficult to interpret.

2) Because there are none, per my answer to question 1).

3) To penetrate the PURE theoretical physics world is even more difficult than promoting CMNS. Clubs of dozens of paid theorists who do not have to produce any testable results have developed in the last couple of decades around Planck scale string theory. An unpaid outsider cannot compete in such a contest, because he will not be able to produce the necessary volume of "elegant" mathematics. Indeed, because he "is too small to succeed", he will not even be able to get through the door far enough to get the feedback necessary to find the best formulation of his work for future acceptance.

On the other hand, if he goes the more realistic route of doing combined PURE and APPLIED physics by showing that his theory gives successful results where the old theory does not, that takes us right back to the problem of dealing with experiments of questionable acceptability.

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Ludwik wrote, addressing Larens:
1) I am surprised by your answer to the third question. Is this a well known fact or is it only your personal opinion?

2) The CMNS field has a reasonably well organized opposition, fighting discrimination. Does something like this exist among string theory practitioners? Please elaborate.

3) I suppose most string theorists are Ph.D.-level researchers. Is this correct?

Larens wrote (responding point by point):
1) Lee Smolin and Peter Woit have each written popular books saying that string theory hegemony is destructive for theoretical physics. I was just giving it my personal spin.

2) There is not yet an organized opposition, because, even though many people are concerned, there has been the lack of any clearly superior alternative to organize around.

3) Yes, [most of them are Ph.D.-level researchers]..

Ludwik (not posted):
1) I still think that attempts to validate theories should be made in platforms (lectures, journals,. discussion lists, etc. Most people on our CMNS list, including myself, are totally unqualified to provide the feedback needed by a string theorists. Larens is probably well aware of this.

He wrote: "To penetrate the PURE theoretical physics world is even more difficult than promoting CMNS. Clubs of dozens of paid theorists who do not have to produce any testable results have developed in the last couple of decades around Planck scale string theory." This looks like a division within the string theory community (the Plank scale group versus others), not a conflict between mainstream theoretical physicists and string theory specialists. In that sense, the situation is different from what we have in CMNS.

I do not know what the PURE is. Does it mean not APPLIED? Does it mean “mathematics only?” The “PENETRATE" probably means to have a theory accepted (recognized as valid). Mathematicians would validate any logically derived theory; the only cause for rejection would be a mathematical error. Physicists, by contrast, reject theories whose conclusions conflict with reality.

388) Energy Amplifier?

Ludwik Kowalski

Montclair State University, New Jersey, USA
March 2, 2010

(A) On February 26, 2010 Bill Collis posted the following message on the private list for CMNS researchers:

Dear Colleagues, A recently published patent

[LINK #1] www.wipo.int/pctdb/en/wo.jsp?WO=2009125444&IA=IT2008000532&DISPLAY=DESC

describes the use of Piantelli's Ni / H setup. Reading through the patent I was unable to find any disclosure of any innovatiion other than the obvious safety precautions of shielding the apparatus! The patent mentions "catalytic action of optional elements", but these are not specified.

The patent applicant is engineer Andrea Rossi, owner of a small company, Eon srl, employing 2-5 people. In the patent he claims that "A practical embodiment of the inventive apparatus, installed on October 16, 2007, is at present perfectly operating 24 hours per day, and provides an amount of heat sufficient to heat the factory of the Company EON of via Carlo Ragazzi 18, at Bondeno (Province of Ferrara)." (Italy). This suggests that power output at least tens of kilowatts!

Two other links for information about this invention, posted by others are:

[LINK #2] http://www.journal-of-nuclear-physics.com/files/Rossi-Focardi_paper.pdf

and

[LINK #3] http://www.journal-of-nuclear-physics.com

After downloading the Journal of Nuclear Physics paper of Focardi and Rossi (LINK 2) I was very excited to see a reprint of an article published in a journal. But clicking on the LINK 3 resulted in a disappointment. I became aware that the Journal of Nuclear Physics is the name of a blog. Seeing something in a blog is not the same as seeing it in a refereed paper. Let me summarize the content of what I find by following the links above:

(B) LINK #1

a) I see a list of references, including the ICCF8 report; it is the last reference.

b) They place Ni powder into a tube (temp 150-500C) and introduce hydrogen (pressure 2 -20 bars).

c) They use some unspecified catalyst to produce an isothermic nuclear reaction which “transforms” a Ni nucleus into a Cu nucleus. They produce more energy than used to run the apparatus. But how much more? This is not stated here. They ask: “Why do we believe that Ni turns into copper?” And they answer: Because atomic masses of Cu are smaller than atomic masses of Ni. Coulomb barrier is not even mentioned.

The “boron layers” were probably introduced to absorb slow neutrons. Why are they concerned with slow neutrons?

d) Lead plates are said to transform radiation energy from copper into heat. They say, “The above mentioned coatings are so designed as to restrain all radiation emitted by the exothermal reaction and transform said radiation into thermal

energy."

e) This prompted me to post a message about the coulomb barrier, at the CMNS list. Responding to mit, Bill wrote that three miracles must occur to make the reported results theoretically acceptable; absence of the coulomb barrier is only one of them. The other two are absence of prompt gammas from proton capture and absence of delayed radioactivity from beta decay. He also reminded us that gamma radiation and neutrons were reported by Pintelli, who performed similar experiment s (on much smaller scale) long time ago.

f) Responding to someone, I wrote: "Yes, they did say that samples of fluid, removed from the pipe after experiments, were no more radioactive than their tap water. On the other hand, they said that layers of lead were used to convert radiation (except neutrinos) into heat. That implies a lot of radiation during experiments. I am puzzled."

g) Another researcher is puzzled by the fact that so little is stated about the patented setup. This is not consistent with what is required; "a patent MUST disclose all what is necessary to build the apparatus for a person skilled in the art."

h) And here is what Bill wrote about radiation:

"Piantelli & Focardi's previous publications record that low levels of charged particles, neutrons and gamma radiation are emitted from certain Ni / H devices. Given that the Rossi has scaled up the power by 3 orders of magnitude he probably took the precaution of shielding the neutrons with boron and the gammas with lead. This precaution implies nothing regarding actual radiation. If he were to report measuring radiation, he might get caught up in health and safety regulations and his chances of demonstrating his device would be limited. :)

So my guess would be that Rossi and Focardi, like Piantelli before them, have in fact measured penetrating radiations but they are still at a low and safe level. As I stated earlier, lack of radiation implies that copper may not be the product of major heat producing reactions. Apart from the 2 stable natural isotopes of copper (63 & 65), all the others are very beta radioactive with short half lives.

No doubt this latest result will stimulate some thought and debate as to the nature of the primary reaction(s) and we can expect new papers will be published on the subject. I'll guess that whatever explanation becomes accepted, it will not require any new physics, it will substantially confirm most of the experimental observations of the last 20 years, and we'll all be asking ourselves, "Why were we all so blind when the evidence was staring us in the face?".

I'm expecting to present a paper myself on the Ni / H gamma spectrum next September. It's certainly a very exciting time to be working in the field!"

(C) LINK #2

a) Here I see another indicator that the authors are a rather limited knowledge of nuclear physics. It is their sentence "There exist no natural Fission processes." They are not aware of spontaneous fission of natural uranium. It was discovered in 1940.

b) The importance of the Coulomb barrier is recognized in this paper. This prompted me to post another short message at the CMNS list. I wrote:

"I see that the Coulomb barrier problem is recognized. Two arguments are presented. The first is the experimental fact; they do see a big difference between the input energy, for example 5.1 kWh, and the output energy, for example, 1006.5 kWh (line 4 in Table 1). This is a very powerful argument. Experimental results, if reproducible, should prevail, no matter what is expected. The second argument is theoretical and it has to do with screening by electrons, in one way or another. Let us hope that the commercial success, which is equivalent to reproducibility tests, will convince mainstream scientists that CMNS processes are worth studying.

I was surprised to learn that their 2009 experiments were performed "with the assistance of " both the DOE (Department of Energy) and the DOD (department of defense). That is what they say on page 4. Am I the only one

surprised of the DOE and DOD involvement? How can this be explained?"

c) So how does their Energy Amplifier work? After clicking the LINK2 read some details.

*) They say : "A practical embodiment of the inventive apparatus, installed on October 16, 2007, is at present perfectly operating 24 hours per day, and provides an amount of heat sufficient to heat the factory of the Company.....The electric resistance temperature controlling thermostat has been designed to switch off said electric resistance after 3-4 hours of operation, thereby providing self-supplied system, continuously emitting thermal energy in an amount larger than that initially generated by said electric resistance, which mode of operation is actually achieved by an exothermal reaction."

*) They say: "The heat generated by the particle decay and nuclear transformations will heat the primary fluid, comprising borated water, thereby said primary fluid, in turn, will exchange heat with the secondary circuit, in turn heated by said primary fluid and conveying the produced thermal energy to desired applications, such as electric power, heating, mechanical energy, and so on."

*) They also say that "In fact, few grams of nickel and hydrogen would produce an energy amount equivalent to that of thousand oil tons." And this is "without pollutions, greenhouse effects, or carbon dioxide increases, nuclear and other waste materials."
They mention catalysts but details are not provided."

d) Another researcher reminded us about related articles, already published in New Energy Times:

http://www.newenergytimes.com/v2/news/2008/NET29-8dd54geg.shtml#dpnr
http://www.newenergytimes.com/v2/news/2008/NET29-8dd54geg.shtml#pf

(D) LINK #3

a) Most of what one can see here is the paper already shown under the LINK2, except the format is different. Only the very beginning is very different. It shows the pattent number and other formal details. The abstract (description of the apparatus) states that the "niclel tube is filled with nickel powder." This seems to conflict with the what was in LINK2. There they said that the powered is on nickel surfaces (some kind of coating. The diagram of the apparatus is shown, but the image resolution is so bad that labels are not readable.

(E) APPENDED ON MARCH 3, 2010

a) Responding to X, Bill Collis wrote: "I don't think Sergio Focardi is named as an applicant nor inventor on the Rossi patent.

The evidence for any Nickel to Copper transmutation is skimpy. Basicly they assume that a nuclear reaction is taking place simply because a chemical reaction cannot explain the power production. On the other hand a nuclear reaction is not a reasonable explanation either because expected nuclear products are not detected.

The major natural nickel isotope is 58Ni so we would expect radioactive 59Cu as the major product of proton capture. However no SIMS measurement of any mass 59 product appears to have been made. No radioactivity was detected. There were no gammas from positron anihilation or beta decay.

Rossi and Focardi have done a brilliant job at demonstrating an industrial prototype. They deserve our congratulations. However the procedures documented in their patent and paper are not original. They rely on the previous work of Francesco Piantelli. Piantelli is the real inventor and his achievements were recognised with the award of the famous Truffle Prize at the second Asti Workshop on Anomalies in Hydrogen / Deuterium Loaded Metals in 1995.

Now that Rossi and Focardi have demonstrated that a compact device can provide tens of kW of practical high

temperature heat, it is likely that there will be a flurry of new research and industrial investment. As scientists, some of us are probably more interested in the underlying science rather than solving engineering problems. But both are necessary.

As I said during the summing up at ICCF13, Sochi, no government will licence new nuclear technologies unless the science is understood. (The public has a pathological fear of all things nuclear). A glittering prize awaits whoever provides that understanding.

My guess is that there will be a scramble of claims and counter-claims as scientists and industrialists attempt to stake out their respective territories and intellectual property - as in 1989."

b) I hope that Bill's expectation--a rush of activities in this field--will materialize. If I had a device producing so much excess heat for many days, as described in the paper, I would make regular public demonstrations, for example, each month. Competent scientists, engineers, and government officials would receive personal invitations to attend and to examine the device.

c) Another CMNS researchers, Peter Gluck, referring to an article in Italian press, wrote: "An Italian patent is threatening the energy monopolies? Are the results reported really real and controllable and upscalable? Mamma mia, if YES, that's an breakthrough! " But then he adds that this can be another hot potato in our controversial field. In other words, I am not the only one who is waiting to be convinced that the claim is valid.

d) Responding to X, Bill Collis posted a table with energies released (Q) after different stable isotopes of nickel fuse with protons.

1H + 58Ni --> 59Cu(beta+) .....Q=+3.419 MeV
1H + 60Ni --> 61Cu(beta+) .....Q=+4.801 MeV
1H + 61Ni --> 62Cu(beta+) .....Q=+5.866 MeV
1H + 61Ni --> 58Co(beta-) . + 4He .....Q=+0.489 MeV
1H + 62Ni --> 63Cu(stable)..... Q=+6.122 MeV
1H + 62Ni --> 59Co(stable) + 4He..... Q=+0.346 MeV
1H + 64Ni --> 65Cu(stable)..... Q=+7.453 MeV
1H + 64Ni --> 61Co(beta-) + 4He .....Q=+0.663 MeV

Energies of protons (1H) are extremely low, even at 500 C specified in the patent. Coulomb barriers for fusion, on the other hand, are very much higher. That is why I remain sceptical. If I had nothing else to do I would calculate the rates at which these nuclear processes take place when excess heat is produced at the rate of 10kW, for example. The rate of gamma emission, associated with the beta decay, could also be estimated; it would probably be very high, confirming that the environment would be lethal, without layers of lead.

The above table is actually a simplified version of what was posted by Bill. He managed to squeeze more information into a short table, as shown below. Note that PV stands for "Parity violation," while numbers between square brackets are spin differences. The g refers to ther ground state.

^{1}H (100.%) + ^{58}Ni(68.1%) -> g + ^{59}Cu(beta+) +3.419 MeV [0]
^{1}H (100.%) + ^{60}Ni(26.2%) -> g + ^{61}Cu(beta+) +4.801 MeV [0]
^{1}H (100.%) + ^{61}Ni(1.14%) -> g + ^{62}Cu(beta+) +5.866 MeV [0]
^{1}H (100.%) + ^{61}Ni(1.14%) -> ^{4}He(100.%) + ^{58}Co(beta-) +0.489 MeV [0] PV
^{1}H (100.%) + ^{62}Ni(3.64%) -> g + ^{63}Cu(69.2%) +6.122 MeV [0]
^{1}H (100.%) + ^{62}Ni(3.64%) -> ^{4}He(100.%) + ^{59}Co(100.%) +0.346 MeV [3] PV
^{1}H (100.%) + ^{64}Ni(0.93%) -> g + ^{65}Cu(30.8%) +7.453 MeV [0]
^{1}H (100.%) + ^{64}Ni(0.93%) -> ^{4}He(100.%) + ^{61}Co(beta-) +0.663 MeV [3] PV

(F) APPENDED ON MARCH 6, 2010

a) A suggestions was made, two days ago, that someone should visit the place where spectacular results are available on demand. The visitor would either confirm or refute what has been reported. I do not think that an outsider would be able to evaluate the setup. What is needed is a blueprint and a detailed protocol. Following the protocol a team a team of competent researchers would try to build the device from scratch and to measure excess heat with their own instruments. Only team members should be allowed to enter the room in which the device is being constructed.

Yes, I am thinking about possibilities of fraud. Fraudulent people, like those who solicit profitable partnership by email, do exist. Precautions of that type would not be necessary if data confirming commercial success (many satisfied homeowners) were available.

b) The Italian newspaper article, that was online yesterday, is no longer there, according to one person. "Strange enough !" he added. Peter Gluck responded: "As editor of a websearch newsletter I am familiar with such unpleasant events. The trouble is with the Stampa Libera site, not with this very paper. You can find it in other places searching for:

"un brevetto intaliano minaccia i monopoli energetici? di Marco Pizzuti

The paper is sensationalist but press is press everywhere. News is information with hypertension, strange hybrid of truth and lies."

Another researcher added: "If the thing really works according to the claim it should find its way to a larger audience all by itself." I agree with this.

c) A German science reporter, Haiko Lietz, wrote: "From my email exchange with Dr. Andrea Rossi I can tell you the following:

- The kW module is operating in the Leonardo facility in the New Hampshire, US, since 2008
- There is no apparatus in Italy
- The group is working on a MW module
- The group is not interested in anything that distracts them from making the MW module operational and safe
- When the MW module is operational there will be a presentation of the plant and a press conference
- They have only published a scientific paper because the patent application required it

Good luck to them and all of us"

In another message Haiko Lietz added "From what he [Dr Rossi] has told me they will not allow anybody into their facility at the moment." That is just the opposite from what I would do at this stage.

d) Responding to the above, I wrote,

"They say 'The group is not interested in anything that distracts them from making the MW module operational and safe.' This is a big mistake. Even a 10 kW unit, which they already have, would be a giant step forward. Waiting for a much more powerful device (perhaps several more years) is probably not motivated by scientific or technological considerations.

Why do I say that they already have a 10 KW device?

*) Because that is what one would need, at least, to keep a very small factory workable during a winter in New Hampshire.

*) In one of their experiments the total amount of excess heat was reported as 1006.5 kWh. Assuming that amount of thermal energy is produced in 24 hours, one gets the the average power of 1006.5 / 24 = 42 kW.

*) If the device operated for 4 days, then the average power would be about 10 kW, as estimated by Bill, several days ago.

*) If the device operated for 40 days, then the average power would be about 1 kW

e) Even 1 kW, if reproducible on demand, would be spectacular. So why do they want to wait for a one thousand times more powerful device?

Can the DOE and the DOD--said to be already aware of spectacular results--speed up the process, in the name of all inhabitants of our planet? ”

f) If all researchers were Americans then the DOE would probably be able to initiate a process by which researchers would be “drafted” to lead a Manhattan-like project. Should I use the term “subpoenaed” instead of “drafted?” But they are Italians, with a laboratory in the US. Why did Rossi decide to set up the shop in the US and not in Italy? Does the Italian government, or the UN, have the power of forcing him to do what seems to be desirable?

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G) TO BE ADDED to HTML (3/13/2010)

a) Reflecting on the safety aspect of a MW device, **X wrote**: "Yes I can understand the safety concerns. They apparently already have a nuclear device which produces GJ [giga-joules of energy] in a confined space and they have no certainty as to what the underlying reactions are. Without any guiding theory, control may be somewhat haphazard. If they have a device which amplifies energy by a factor of 100 or more, what will they do when the multi-MW "reactor" goes critical and requires no external power? The temperature keeps rising, and melts its way through any container....

I just hope there's a negative temperature coefficient of reaction rate. This is likely as at high temperatures where NiH is unstable. Even so, as these phenomena are capable of melting nickel, Rossi is taking a very risky step."

This made me think about what Peter Gluck calls omnipresent "poisons" of NAE (Nuclear Active Environment). In fact, X just reminded us that hydrogen, in Piantelli's experiments had to be at least 99% pure. A very small amount of

deuterium stops the generation of excess heat. Deuterium might be used to play the same role as control rods play in our existing nuclear reactors.

b) **Ludwik** (posted at CMNS list on 3/11/2010):
. . . My second guess is that several people on this list are able and willing to verify the validity of the claim. I am not one of them. But I would very much like them to form a team.

*) The first task of the team would be to agree on the goal, for example, a device reproducibly generating excess heat of the average rate higher than 200 W, for at least two days. This is three orders of magnitude less than what has been claimed (heating a small factory) by Rossi et al.

*) Once the "confirming result", has been operationally defined, the team would develop a protocol. That is a difficult, but not impossible, task. The protocol, based on what is known, should be as simple as possible. The estimated costs should be listed. .

*) The protocol should then be posted on our list. That would be the end of the preliminary work.

*) Ideally at least three or four "able and willing" people would then start using the protocol "online." By this I mean performing experiments and sharing all results with us. Feedback from non-participants would probably be useful to those who perform experiments. Working in unison people can be more productive than working separately.

*) Final conclusions would eventually be published somewhere, for example, in JCMNS, at one of our conferences, or at the www.lenr-canr.org website.

c) Y1 responded:
I strongly support your suggestion to organize a group to replicate the Rossi patent and I offer myself to assist. I have a modest laboratory and am well equipped to undertake gas absorption experiments.

A private message from Y2 has me concerned about the veracity of this patent and I suggest we closely monitor this situation which may be fraudulent (in the USA). Perhaps you should contact Y2 directly. I sincerely hope the patent can lead to success.....

d) Y3 wrote:
If I remember correctly one of the experimenters commented that surface preparation was the key to establishing a repeatable result in their experiment. That included preparation of the materials in particular gas environment. I don't remember if it was O or H free. He talked about years of experimenting to get that part understood.

If you are not privy to these details then it would seem to me that the odds of successful replication are quite low. Rather like the initial replication attempts of the F&P experiment where experimenters did something that was similar and considered close enough but as we know know destined to fail.

Perhaps I am being overly critical here, but assuming that all of the relevant information is in the patent is assuming quite a lot.

Ludwik responded:
*) No, you are not being over critical. Your kind of observations could convince the team of tentative researchers to give up, after preliminary discussion. I was thinking about those who participated in earlier discussions of Piantelli's work (mentioned by Bill). I was also thinking about a possibility that Rossi might be willing to help. A person who wants to be secretive would not publish a paper.

*) I am not competent for this kind of research. The only thing I can do, after reliable 0.2 kW excess heat is produced, is to send some CR-39 and to look for tracks of nuclear particles. But this would be at the ending phase of the project--just a shot in the dark.

Y4 wrote (providing a link to R. M. Santilli's paper--see below):
The experiment should be easier to reproduce than many - but there are strong warnings about radiation levels. [See

http://www.i-b-r.org/NeutronSynthesisNCA-I.pdf

The author of this paper, Rugerro Maria Santili, informs us that "this his paper was rejected by Physics Letters, Il Nuovo Cimento and Physical Review Letters with "reviews" solely based on theoretical theologies that can only be qualified as being scientifically pathetic."]

Ludwik responded:
The abstract of the Santelli's article

http://www.i-b-r.org/NeutronSynthesisNCA-I.pdf

is worth thinking about. (The above link was posted by Andrew, this morning.)

Abstract

We report measurements, necessarily preliminary due to their novelty, toward the laboratory synthesis of the neutron from protons and electrons, in the hope that they are not judged via theoretical conjectures, but subjected instead to independent re-runs for their verification or denial, said process being requested by possible new clean energies so much needed by our increasing environmental problems.

NOTE:
This paper was rejected by Physics Letters, Il Nuovo Cimento and Physical Review Letters with "reviews" solely based on theoretical theologies that can only be qualified as being scientifically pathetic.

Information about very impressive Santelli's scientific background is at:

http://www.i-b-r.org/Ruggero-Maria-Santilli.htm

Most of us probably agree with the author that experimental results should be verified in laboratories; they should not be rejected on the basis of conflicts with accepted theories. He claims that his papers were rejected on that basis. This probably becomes obvious after reading the letters of rejections.

Here is a private message I just sent to Dr. Santilli:

Dear Dr. Santilli,

This message is prompted by your three-times-rejected 2008 article. In the abstract you wrote: ". . . " Rejecting experimental results conflicting with accepted theories is a disease that might kill science. I anticipate writing a short note on that subject; your case can be used as illustration. That is why I would very much like to know how the editors of the three mainstream journals explain rejections of your manuscript.

Ideally, I would like to quote their rejections. But that is a delicate matter; I would probably need permissions to quote etc. Equally useful would be your descriptions of the contents of these letters, provided I can quote your "personal e-mail information."

Ludwik Kowalski
Professor Emeritus, Ph.D.

This website contains other cold fusion items.
Click to see the list of links

389) Joke or Ignorence?

Ludwik Kowalski

Montclair State University, New Jersey, USA
March 14, 2010

A) A reference to Santilli's article was made at the end of Unit 388. Then the following was posted by one CMNS researcher: "I attache another extraordinary claim. The article containing it is circulating around. The title is "Ecological Nuclear Energy." I downloaded the file, and appended it it to this unit (see below). After reading the article, I posted the following message on our CMNS list.

The article, downloadable from

www.nuenergy.org/pdf/electronically-activated-radioisotopic-carbon-generator.pdf

is probably a joke. If I were a referee I would ask the following questions:

(a) What experimental evidence does Perreault have that electrons fuse with atomic nuclei, as in process (1)?

(b) Yes, the process (2) is exothermic (Q=+13.4 MeV). But the reverse process (1) would be endothermic (Q=-13.4 MeV). In other words, no net energy can be generated by two processes, unless something else is involved. What can this "something else" be?

(c) An experimentalist can be forgiven for not being able to speculate about (b). But s/he cannot be forgiven for not providing convincing evidence for the excess energy. How much energy is released in each round trip--process (1) followed by process (2)?

(d) An experimentalist claiming that most of the released energy appears in the form of beta and gamma rays is expected to provide evidence for such radiation.

No one responded so far. Interesting comments and observations, if any, will be appended at the of this unit (after the end of the article). Some sentences in the article are too long for my taste. But I am not going to correct anything.

Can the Electron Capture process, competing with the beta+ decay, be called fussion? I do not think so. In fact, I am not aware of nuclear reactions induced by electrons. Scattering of high energy electrons, on the other hand, was studied to measure sizes of atomic nuclei.

ECOLOGICAL NUCLEAR ENERGY FOR THE HOME

Translated and edited by Bruce A. Perreault on 11-20-2006

I have forced myself to write this article to shape my practical experiences that I have made about nuclear energy to the order of many people whom have requested for me to make references of the case.

Nuclear energy is a subject that has been badly focused and badly used from the beginning of the times, from the discovery of the radiations and the experiments performed by the Curies, the Uranium and all that derived from it in a technology at the moment is very complex, of high cost, dangerous and generates polluting by-products. What makes this single energy possible is that it is available only to the countries that can pay for such expenses. Furthermore, never can this form of energy be used to take to the home or to use it to impel automobiles or to feed a generator for our houses for the same reason before mentioned, but we watch around us the enormous amount of processes of disintegration and nuclear reactions that surround us everywhere on the Earth we see here that IF a system can be made nuclear simple, ecological and very cheap.

Common occurrences such as electrical discharges, lightning, etc…, produce nuclear reactions, which also includes processes that simply happen in electrical circuits. If we can design an electrical circuit that produces a nuclear reaction within itself in stable form and controlled, everything is certain.

We do not need to look for difficult things and dangerous things like Uranium, Plutonium, etc… if we watch the spontaneous nuclear reactions that happen in our planet we will see that the radioactive elements participate more in the same way as the one that is perhaps one of most abundant of the planet and that is CARBON, and pure coal is something very easy to obtain, the more typical nuclear reaction we see in nature involves carbon. When carbon is bombarded by electrons in a process of nuclear fusion electrons are attracted to the nuclei of carbon atoms to form Boron in the following reaction:

$$^{12}_{6}C + ^{0}_{-1}e = ^{12}_{5}B \quad \textbf{(1)}$$

When carbon transforms into boron the electron must appear with energy within the rank of thousands of electron-volts, once the Boron is formed, the boron atom is seen that it is an unstable isotope and therefore is disintegrated again to transform itself back into the stable Carbon atom of the principle of this reaction:

$$^{12}_{5}B = ^{12}_{6}C + ^{0}_{-1}\beta \quad \textbf{(2)}$$

The electron is emitted or given back. Now, I illustrate through the symbol of the beta particle because the energy whereupon leaves with the electron from the nucleus which is initially much greater, no longer is it thousands of electron-volts, the exit energy is extreme, now in the 13 million electron-volt range, that is to say, an energy thousands of times to initiate the reaction, it is feasible to capture these beta rays of great energy, for example, in a toroid and thus to obtain immediate electrical energy of the nuclear process with no need of an intermediate process.

Reactions 1 and 2 are reversible reactions because the carbon once transformed into Boron returns to be reconstituted to initiate indefinite cycles, the later giving off more energy than the former but does not break any law of power conservation because the excess of energy corresponds to the stored internal energy in the atoms resulting in the conversion of matter into energy, this can be illustrated.

For the person who does not understand; as if we went to a zoo and we threw a peanut to an elephant, then the elephant takes the peanut and returns it to the thrower with the power of his trunk with many times greater energy, this is the

same. In addition to this fact, one knows that of each 100,000 carbon atoms that are bombarded with electrons only 1 enters the reversible reaction that it gives us. I calculate that 8KW of power is available per gram of transformed carbon. It is not necessary to resupply the system with coal because the system regenerates, but after a certain time it is advisable to change the coal source which is subject to wearing down. Not only is light and heat generated from all nuclear reactions but electrons are also freed.

Practical implementation:

How then we can accelerate electrons and shoot them into coal so that it will catch electrons in the form of beta radiation? Very simply, we obtain a pure coal bar and discharge a condenser into it, depends on the capacity of the condenser and of the voltage of the energy of discharge required for the reaction in equation 1 will be reached. All we know that when we discharge a condenser, especially in a short circuit through a coal bar, very high currents will take place that guarantee a high electron flow to favor greater collisions, and it is that simple, nothing of nuclear reactors, systems of cooling, pumps of high vacuum, only a simple electrical circuit will transform the nuclear energy released directly into electricity since the same emitted beta radiation is captured and stopped to feed an electrical load, which makes it more efficient than a nuclear system of a power station. For example; in which the reactor warms up the water for in the end with the pressure of that steam moving a turbine and generating electricity, we did not need any intermediate process here, because the nuclear energy becomes directly processed into electricity. The following figure shows the system and the process in general form that I have actually used:

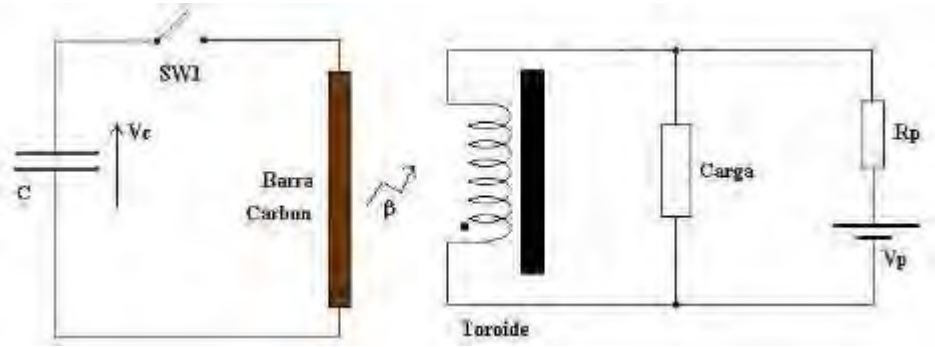

Fig 1. Practical system of direct transformation of nuclear energy to electrical

In the figure a condenser *C* is loaded initially to a Voltage *Vc* and is discharged when closing the switch SW1, said switch can actually be a FET of high current, in conjunction with a pure coal bar the awaited nuclear reaction will take place and it will free to the desired beta rays which are captured by the illustrated toroidal coil, as a simple coil catches the radiation, said toroid acts as a transformer of current for the current circulates through the coal bar but in addition to that the toroid is polarized initially with a continuous source composed by the Vp battery and the Rp resistance makes a low current circulate around the toroid, in order to guarantee beta rays they do not go through the toroid but are turned aside onto itself and thus to capture most of the released possible energy.

The practical results I have obtained are remarkable, to begin with it is appreciated that the effect of the polarization of current around the toroid hugs the coal bar, when not polarized the collected energy varies until it is at a factor of 3, and with a small system of a bar-toroid, with a maximum dimension of 15 cm, I easily managed to obtain 6KW with the load by all means placed in the output of the toroid, and as the power output of the system is much greater than the input simply refed in the tests and the initial starting battery was removed, this is justified by the enormous energy available because it is like the calculations before referred to which we have 8 KW of useful power per each gram of transformed carbon.

It is possible to finally emphasize that although the beta radiation is the practical fuel of the system here it is used completely being transformed into useful energy, unlike the conventional technology where the radiations are a dangerous by-product, in any case in the process of the safety of this system we are due to follow strict safety measures oriented to prevent damages to the health to the builder of this system by radiation exposure either through burns or repeated exposure to the medium or long term exposure, that is to say, during the tests to use suitable sealing,

suitable shields that are of lead, concrete or another material and a radioactivity indicator, leaving until the emission level during the tests is at a tolerable level, and by all means for the end assembly of the generating unit sealed, isolated to guarantee during its life utility that there is never is going to exist a radiation leak, even if in case of faults or accidents since this it is a passive system, if the bar is not excited its radioactivity will no longer be present, it is enough to disconnect the feeding without the problem of an explosion, critical mass or a chain reaction, only to take our own precautions to work with radiations with people who activate the circuit, the users only use these calibrated activated systems and they don't mention it and do not have to worry.

Conclusion:

The fact that conventional nuclear technology is so complex, expensive, dangerous and damages ecological systems seems to be something more than an accident of history, that is to say, all we know that from a sample of history that such technology wanted to be used to make a nuclear pump and all the pursuit oriented to that without fully accounting for the expenses involved and it moved forward as rapidly as possible, it is very probable the scientists who worked in those projects knew of all this but given the circumstances to make something possible or to pressures they received and the idea that anyone could simultaneously develop the technology that we today now call nuclear energy, I do not want to propose that people who use this technology are mistaken or making expenses at the cost of others, I only want to show the light of this evidence that anyone can verify on their own that nuclear technology could be today in our houses, impelling our automobiles, airplanes and spaceships without the contamination fear or damage to the ecology when anyone can have a system of these for their own use, coal abounds in our planet and is an inexhaustible and limitless source of energy.

If you have serious doubts or desire consultations about this brief and a clearer explanation send your e-mail to:

gigawattgratis@123mail.cl
jarayam@latinmail.com

Original Article: http://econuclear.tk/

B) Responding to my message (see the top of this message) Z1 reminded us about intersting Japanese experiments (Kohji KAMADA, Hiroshi KINOSHITA and Heishitiro TAKAHASHI) in which excess heat was produced by exposing the aluminum loaded with deuterons to beams of electrons of several hundreds keV. My reply was:

1) To avoid confusion I would make a distinction between a nuclear process and a nuclear reaction. Any nuclear reaction is a nuclear process but some nuclear processes are not nuclear reactions. Consider a situation in which the released nuclear energy is received by a crystal lattice, rather than by a nucleon or a small cluster of nucleons, such as an alpha particle. I would not call this a nuclear reaction. Radioactive decays are usually not called nuclear reactions; they are not triggered by collisions of two nuclear objects. (A nuclear object is a nucleon or a group of nucleons bound by strong nuclear forces).

A nuclear reaction is often defined as a process in which two nuclei or nuclear particles collide to produce products different from the initial particles. Electrons are not nuclear objects. I never heard of processes in which nucleons, or clusters of nucleons, are emitted from atoms bombarded by electrons.

2) Thank you for summarizing spectacular results reported by Kamada, Kinoshita and Takahashi. Why should we argue about how to call their experiments? Are they nuclear reactions or are they nuclear processes? They are interesting experimental facts. That name is sufficient, at this stage.

3) My attitude toward the Perreault's paper at

www.nuenergy.org/pdf/electronically-activated-radioisotopic-carbon-generator.pdf

is the same as described several hours earlier. I still suspect a joke. Naturally, I can add more questions to those four

that were already asked. For example,

e) What were the numerical values of Vc, C, Rp and L?

f) How was the excess energy measured and how large it was? Answers to (e) and (f) would be essential for researchers trying to verify your results.

g) How many experiments were performed and how reproducible your results were?

h) Why do you say that each gram of carbon produces "8 kW of useful power?" One gram would correspond to a certain amount of energy (kWh, not kW). For a given amount of energy, the power depends on the rate at which the fuel is consumed.

Do you people agree that the article is very weak? Is my reaction (pretending I am a referee) justified? If not then what should it be?

Ludwik
= = = = = =

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390) Conspiracy or Defence?

Ludwik Kowalski

Montclair State University, New Jersey, USA
March 15, 2010

1) This unit is a continuation of what was recorded at the end of Unit #388 My private message to Dr. R. M. Santilli was shown in that unit. This morning I received the reply with the link to the newer version of his paper. Also provided is the link to his cosmological paper. My background in cosmology (general relativity, gravity, etc.) is too limited and will probably focus on his first paper. Santilli informed me that this paper has just been "accepted by a reputable refereed journal. The new title is: "Experimental Confirmation of the Novel \ Intermediate Controlled Nuclear Fusion Without Harmful Radiations." the title of the rejected paper was "Apparent confirmation of Don Borghi's experiment on the laboratory synthesis of neutrons from protons and electrons."

2) Responding to Santilli I wrote: "My intention, as I wrote to you yesterday is to focus on what you call politics in science. Quoting rejection letters from editors of three mainstream journals (or quoting your descriptions of these letters) would be an essential part of the note I plan to publish. That is why I would like to have this input before starting to compose anything."

3) Two versions of the paper do not seem to be very different, as far as the basic claim is concerned. The described setups are said to synthesize neutrons from protons and electrons. What a coincidence, the same claim was made in Perreault's paper. The author thinks that 12B is produced when 12C is bombarded by electrons whose energy is several hundred keV. This implies that an electron and a proton fuse to produce a neutron, inside the 12C nucleus. That 2006 paper is in my unit #389. If I were a referee I would suggest the rejection of the paper, unless it were totally rewritten. Eight critical comments were made in my fictitious referee report.

My overall impression was that the article was a joke written by an incompetent author. Santilli, on the other hand, is a Ph.D.-level nuclear physicists and the author of many publications. I am ready to start reading his paper carefully, first the rejected 2008 version and then the accepted 2009 version. This time I will pretend being a reviewer, rather than a referee. This will also allow me to make comments. Will they be the same as those I made addressing Perreault? This remains to be seen.

4) On 3/20/2010 Ludwik posted the following blue message (3/20/2010) on the pivate forum for CMNS researchers. That was about 24 hours ago. No one responded. Others are probably also not satisfied with absence of clarity combined with the unusual format of Santilli's paper.
"Dear all,

A) Three recent CMNS claims were:

(a) Rossi's patent (converting nickel into copper in order to heat a factory)
(b) Parreault's article (converting carbon into boron in order to heat homes)
(c) Santilli paper (turning protons into neutrons and getting energy from decaying neutrons)

As recorded in my unit 388

http://csam.montclair.edu/~kowalski/cf/388amplifier.html

The claim (a) does not belong to the realm of science; it belongs to the realm of patents where claims are validated on the basis of honest commercial successes, i.e. on large numbers of satisfied customers.

B) All who commented on claim (b) think that the article is not credible. It contains several indicators of poor familiarity with science. I suspect the article is a joke, as described in my unit 389. And what about the claim (c); it has also been mentioned at the end of my unit 288. The author's credentials are very impressive and I contacted him. I wrote:

"This message is prompted by your three-times-rejected 2008 article. In the abstract it you wrote: 'We report measurements, necessarily preliminary due to their novelty, toward the laboratory synthesis of the neutron from protons and electrons, in the hope that they are not judged via theoretical conjectures, but subjected instead to independent re-runs for their verification or denial, said process being requested by possible new clean energies so much needed by our increasing environmental problems. (NOTE:This paper was rejected by Physics Letters, Il Nuovo Cimento and Physical Review Letters with "reviews" solely based on theoretical theologies that can only be qualified as being scientifically pathetic.) '

Rejecting experimental results conflicting with accepted theories is a disease that might kill science. I anticipate writing a short note on that subject; your case can be used as illustration. That is why I would very much like to know how the editors of the three mainstream journals explain rejections of your manuscript.

Ideally, I would like to quote their rejections. But that is a delicate matter; I would probably need permissions to quote etc. Equally useful would be your descriptions of the contents of these letters, provided I can quote your "personal e-mail information."

C) Dr. Sallini sent me a new version of his paper; it has just been accepted for publication in a reputable referee journal. But he did not send me what I asked for. In the next message he gave me permission to share the accepted version of the manuscript with others. But neither rejection letters nor paraphrasing were sent to me. I asked again but my third message was not answered. That was the end of our correspondence.

D) The manuscript that was rejected three times can be downloaded from:

http://www.i-b-r.org/NeutronSynthesisNCA-I.pdf

the recently accepted manuscript can be downloaded from

http://www.santilli-foundation.org/docs/ICNF.pdf

The contents are very different. Let me comment on what I read. One thing became clear to me at once; these two manuscripts are poorly written; I would certainly sent them back to the author, asking for more clarity. That is what the editors probably did. The most annoying is constant mixing of experimental facts with theoretical conclusions. Presenting experimental facts in the form of reports from analytical laboratories is not consistent with common practice. I am not going to list numerous shortcomings of each article; the bottom line is that the articles are very difficult to read with understanding.

E) The apparatus used by Scallini consisted of an electric discharge chamber filled with hydrogen, at a pressure of several atmospheres. Two tungsten rods, facing each other, were introduced into the chamber, in order to create the arc. In one case the gauge pressure was about 2 atm, the distance between the tungsten tips was 0.125," and the current was 30 A. The arc duration, in that case, was 3 seconds. Presence of neutrons and gamma rays was demonstrated by using several kinds of commercial detectors.

The author reminds us that "the neutron synthesis requires 0.78 MeV over the sum of the rest energies of the proton and of the electron. Where does this energy come from? At Point 3, in Section 5, Santelli speculates that the 0.78 MeV

of energy, needed to turn a proton into a neutron, “originates from the aether as a universal substratum with very high energy density.” That speculation is not repeated in the new version of Santelli's article. Both articles, however, refer to the so-called “Standard Hydronic Theory.” Unfortunately, I am not familiar with that theory; it was developed by Santilli.

F) The second version of Santilli article is not limited to production of neutrons from protons. Section 4, for example, describes experiments in which nitrogen was produced from deuterium and carbon.”

Added on 3/23/2010
Responding to the above, one CMNS researcher, Dr. Dean Sinclair, wrote to me in private:

*) About Rossi, My work would say that his idea is not beyond the realm of possibility" however, I suspect that his results are actually H:H to D, Probably through an HHH+ unit.

*) As to Perrault's Carbon to Boron, the only Carbon to Boron transform that I can see off the top of my head is C-11 to B-11, and I can't see where the C-11 source would come from.

To me, the most interesting is Santilli's work.

*) I suspect that Santilli is correct that he is producing neutrons as he is creating a SHOCK WAVE.
I agree with his comments about "Aether" involvement as I don't think that he is synthesizing them from electrons and protons: By my work, he is literally creating them from the usually overlooked Null Set that is part of any unit. I shall explain.

Possibly the most controversial aspect of my work is my opinion that "annihilation" and "pair production" are symmetrically reversed processes and indicate the presence of a ubiquitous combination unit which I call the "zerotron," a unit which is SHOCK WAVE DEFORMABLE TO A NEUTRON.!

In terms of my theorizing, I'd say Santini's work is probably valid; but, not exactly as he anticipated.

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392) Unexpected rejection of our important paper

Ludwik Kowalski

Montclair State University, New Jersey, USA
June 12, 2010

Introduction

The Curie Project was described in unit 375. Participants were Jeff Driscoll, Mike Horton, Ludwik Kowalski, and Pete Lohstreter. Our independent attempts to verify Oriani's claim of reproducibility turned out to be negative. On most experimental surfaces (31 out of 34) the mean track densities were essentially the same as on control surfaces. On the remaining three experimental surfaces the densities were much higher than those reported by Oriani. They were tentatively attributed to contamination. The purpose of this unit is to describe our attempt to publish that paper.

Submitting the paper

On October 16, 2009, I received the following message from Dr. Jan Marwan, who was certainly familiar with our project. "Dear Dr Kowalski, The American Institute of Physics (AIP) offered me to edit a book titled **"Low Energy Nuclear Reactions Sourcebook**" based on the New Energy Technology Symposium that I am organizing.

I work as the only editor together with the AIP on this book, and we want to give many research groups working in the LENR field the opportunity to publish their results here. This covers on the one hand brand new results recently discovered, but may also be a brief review of the work you performed over the last 20 years. The book we are planning is going to be a general sourcebook, covering most of the topics (experimental and theory) of LENR, with which the scientific audience worldwide, reading this book in the future, may get the general idea of what LENR is but also may go into very detail. We think that with this work, based on your article among many others, we are going to publish a book of high demand to which scientists and students at Universities, Research Institutes, Industries etc worldwide will have access for a very long time.

This AIP book is officially a proceeding based on the New Energy Technology Symposium held in San Francisco, CA, March 2010 at the American Chemical Society (ACS). However, the AIP agrees that people who, due to the difficult funding situation at this time, may not be able to attend the meeting, are given equal chances to publish in AIP, same as, of course, those who attend the meeting in San Francisco. This book, again, will cover new results but may also contain results already presented and published elsewhere (permission by the first publisher, of course, needed, and if no verbatim used) and so to give LENR scientists the opportunity to briefly review their long term work in this field. Those who already published in ACS LENR Sourcebook volume 1 & 2 are given equal chances to publish again (preferably new discoveries), this time in AIP. I think we all feel the same that this offer by the AIP does not come up very often in our life..... "

Naturally, we wrote the paper and submitted it before the deadline. It was hard to believe that our establishment (American Institute of Physics) was going publish a book with papers describing cold fusion. I am not inserting our paper here because it is too long (eight pages) and, more importantly, because its future remains undetermined. It was first accepted in May 2010, and then rejected in June.

The unexpected rejection

On June 11, 2010, I received the following message from Dr. Marwan

Ludwik,
I am sorry to let you know that the other reviewer surprisingly rejected your AIP paper.
I therefore decided not to include your paper into this AIP book. I truly apologise for the inconvenience.
Jan

Inconvenience? It is more that this. A report from the "other reviewer," Y, was attached to the message. The report of the first reviewer, X, was sent to me on May 8, when the book was accepted. Responding to this June 11 message I wrote:

"a) I am also very sorry. On what basis was our paper rejected?

b) How many reviewers did you send the paper to?

c) What options do we have in this unexpected situation? "

That was more than 24 hours ago. It is Saturday; I will probably not receive the answer before Monday. Meanwhile let me summarize the X and Y reports.

X ==> "Publish as received, or with minor corrections as indicated." All these corrections were minor and easy to make.

Y ==> "I strongly recommend the editor to reject this paper for publication."

Another big difference was in ranking of our paper. Five categories of ranking were:

a) Technical Quality
b) Significance
c) Comprehensiveness
d) Originality (when applicable)
e) Clarity.

Three choices for each category were: "good," "average," and "poor."

*** Ranking by X ==> a, c and e are " good," d is "N/A," and b is "average."

***Ranking by Y ==> all five categories, even d, which is not applicable, are "poor."

The rejection of our paper by Dr. Marwan, on June 11, was a sudden reversal; a previous message on June 10 asked for minor formatting corrections (elimination of blank lines between paragraphs).

What would I do if I were in Marwan's place?

It is not easy to be an editor; I am glad I am not in Jan's place. AIP ethical standards (Google found them for me) describe what is acceptable and what is not acceptable, as far as reviewing papers is concerned.

Suppose the manuscript is clear and comprehensive to me (in Marwan's place) while the referee's ratings for these categories are "poor." That would be an indication of a possible bias. The "poor" for all categories would also be a warning that something is not right.

I would ask Ludwik to comment about what referee Y wrote. Then I would send the manuscript to at least one more referee, and would inform Ludwik about this. In choosing referees I would eliminate those who might have vested

interests in defending the teams criticized in the submitted article. And I would be open-minded about what is recommended by referees. Their role is to suggest; decisions about rejections belong to editors.

Please note that we made no attempts to replicate SPAWAR experiments. Our comments (about that field) were based on results published by respected researchers (more specifically, authors of references 10 and 11). This was clearly stated in our manuscript. Ignoring this, reviewer Y wrote:

"This paper specifically addresses the reproducibility issue of the SPAWAR experiments. Kowalski and his co workers, in attempting to replicate this experiment, failed, and from that they questioned the results obtained by the SPAWAR and the Oriani group and concluded that the broad range of the SPAWAR research using CR 39 tracks to detect nuclear particle emission is wrong and totally misleading. Moreover, in his final conclusion Kowalski interpreted this issue as a failure of the whole LENR/cold fusion subject and convincingly predicted this research topic to become out of interest.

However, does the fact that Kowalski and his co workers not able to replicate the SPAWAR results means that SPAWAR, and a few other research groups who successfully replicated these experiments, have been misinterpreting their results, or does it rather mean that the Kowalski group failed because of lacking scientific background and experimental skill? On the one hand, the SPAWAR researchers working in this field are highly recognized and published their work in more than 20 peer reviewed papers. If Kowalski were right, it would mean that the reviewers had been wrong. On the other hand, it is not very much convincing to claim irreproducibility of the SPAWAR results on the basis of the work carried out by high school teachers. Although everyone wishes to replicate other researchers' experiments it still requires experimental expertise, and now one could argue that a high school teacher may not have the skill compared to a highly qualified research scientist.

To evidence the truth of LENR/cold fusion requires detailed experimental data that the SPAWAR group has provided, to prove this research wrong requires experimental data as well that I was missing reading this paper. Therefore, I strongly recommend the editor to reject this paper for publication."

I do not think that we, as a group, were unqualified to replicate Oriani's experiments. In fact, we complemented each other. I am a nuclear physicist (Ph.D. in 1963), J.D. is an engineer, M.H. and P.L. are experienced chemistry and physics teachers. We were an ideal team for the selected task. Aware of our limitations, we did not attempt to perform SPAWAR-type experiments. What else can I say? I also participated in numerous research projects; my list of publications, in peer reviewed journals, click to see it is also impressive.

The paper we want to publish will eventually be appended to this unit. I am not doing this now because this might conflict with our attempts to publish it. Those who read it will see that no attempts were made "to claim irreproducibility of the SPAWAR results **on the basis of the work carried out by high school teachers."** We were comparing results published by distinguished CMNS researchers, more specifically, by authors of references (10) and (11). The readers will also see that over 90% of our manuscript is devoted to our own investigations of the Oriani-type effect. Why were recent SPAWAR-type experiments marginally mentioned? Because they have something in common with Oriani-type experiments. In both cases thin Mylar films are used to protect CR-39 detectors from the electrolyte.

Conclusions and interpretations

Referring to our experimental results we wrote: "The overall conclusion is that experimental data reported in [8], and summarized in Figure 2, are not reproducible. Track densities measured on our thirty-four CR-39 surfaces were either much lower or much higher than those reported by Oriani. The origin of high track densities, in Experiments #10, #11 and #12, is not clear."

And here is our final observation: "Experimental results supporting two recent claims of emission of charged nuclear projectiles due to electrolysis are not reproducible. Focusing on reproducibility is probably more important, at this stage, than focusing on interpretation of experimental results. The future of the so-called "Cold Fusion" field--now called "Condensed Matter Nuclear Science" (CMNS)--remains uncertain. The attitude of those who control scientific

research (editors of mainstream journals, directors of granting agencies, etc.), toward the CMNS field remains highly negative. The field is in real danger of disappearing without producing clear yes-or-no answers about its extraordinary claims. It will probably be rediscovered later in this century."

Refering to this Y wrote: "Moreover, in his final conclusion Kowalski interpreted this issue as a failure of the whole LENR/cold fusion subject and convincingly predicted this research topic to become out of interest."

Where does the term "out of interest" come from? And we did not say "failure of the whole LENR/cold fusion subject;" we were referring to "the uncertainty" about the field future. Furthermore, we are not "convincingly predicting" anything. Is it not true that the field is in real danger of disappearing? In any case, our last observation could easily be modified to avoid possible misrepresentations. In Marwan's place I would ask Ludwik to rewrite the final observation. That can easily be done. Rejecting the entire paper (with valuable experimental results) because of one questionable formulation (at the end of the last paragraph) is not reasonable. Each of us put a lot of work into this piece of scientific research; why should it be wasted? We are pleased that no technical mistakes were found in our paper.

A constructive suggestion

Suppose the potentially-controversial ending sentences are removed. This would reduce our final observation to: "Experimental results supporting two recent claims of emission of charged nuclear projectiles due to electrolysis are not reproducible. Focusing on reproducibility is probably more important, at this stage, than focusing on interpretation of experimental results." Would such personal observation, or something similar, be acceptable? I hope so. If not then the entire last paragraph could be eliminated. This seems to be more reasonable than throwing away the data. Do you agree?

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393) My note (see below) was rejected

Ludwik Kowalski

Montclair State University, New Jersey, USA

August 23, 2010

This unit is the follow-up of what was described in unit 395. I am being prompted to compose it because of the e-mail message received today. I was informed that my short note has been rejected by European Physics Journal, Applied Physics.

1) My rejected note is shown first:

Four questions and a comment

Ludwik Kowalski
Montclair State University
kowalskiL@mail.montclair.edu

I read the paper by P. A. Mosier-Boss et al, [1] with great interest. Can a chemical effect, such as electrolysis, trigger a nuclear effect? This question has been debated since 1989, when the discovery of the so-called “cold fusion” was announced by Fleischmann and Pons [2]. Great progress toward answering this question has been made, as summarized in [3]. But the world is still waiting for a reproducible-on-demand, and convincing, demonstration of a chemically-induced nuclear reaction. The name “cold fusion,” by the way, has recently been replaced by CMNS (Condense Matter Nuclear Science). According to [1], triple tracks discovered in CR-39 detectors are due to neutrons, with energies higher than 9.6 MeV emitted during electrolysis, rather than, for example, to cosmic rays.

I have four questions and a comment. The first question is about the phrase “neutron flux of 10_7 n/s" (in the last paragraph in Section 2). The phrase seems to be contradictory. If the number 10^7 refer to “flux” then the unit should be (n/(cm^2 *s); if the unit is correct then the term “neutron flux” is not appropriate. I suspect that 10^7refers to the neutron source intensity (strength). If this is true then to estimate the flux one would have to divide the 10^7 by 4*PI*r^2, where r is the distance between the center of the source and the CR-39 detector.” My second question is about r; how large was it?

My first comment has to do with the number of tracks. As stated in Subsection 3.3, codeposition experiments “typically last two weeks.” The number of triple tracks per detector was “at most 5 to 10, including both the front and back surfaces.” That is indeed a very small number. It is reassuring that no triple tracks “have been observed in background monitoring detectors.” That rules out the “cosmic rays” interpretation. This argument would be stronger if the total numbers of experiments were reported.

Suppose that ten detectors are used in all codeposition experiments, and that the mean number of tracks per detector is 5. Then the total would be about 50 triple tracks. Suppose that only five triple tracks were found on ten background monitoring detectors. The difference between 50 and 5 would certainly be more significant than the difference between 10 and 3.

My third question has to do with the neutron source exposure time of 4.5 hours. Was this time chosen to produce about as many triple tracks as during a typical codeposition experiment (lasting two weeks)? What was a typical number of triple tracks found after 4.5 hours of DT irradiation?

My last set of related questions has to do with the overall strategy. What to do next? The world is waiting for a reproducible-on-demand, and convincing, demonstration of a chemically-induced nuclear reaction. What kind of codepositionon experiment is more likely to lead us to that end? The last SPAWAR paper [1], focusing on rare events, can be contrasted with earlier codeposition papers [4,5], where the reported racks were much more abundant.

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2) The rejection note:

Your manuscript, “Four questions and a comment,” has been carefully considered by the referees of The European Physical Journal-AP. As you can see on the enclosed reports, the referees have raised serious concerns regarding its suitability for publication.

I therefore regret to inform you that your manuscript has not been accepted for publication. Thank you very much for having submitted your article to our journal and I hope that you will nevertheless consider EPJ-AP for the publication of your future articles.

3) Comments made by three referees (sent to me by the editor):

Referee 1:

In 2009, we reported on the observation of triple tracks in CR-39 detectors used in Pd-D co-deposition experiments [1]. A search of the literature indicated that triple tracks are diagnostic of the 12C(n,n')3alphas carbon breakup reaction in the detector with an energy threshold of 9.6 MeV [2-5]. At the time, it was only possible to compare images of triple tracks observed in CR-39 detectors used in the Pd/D co-deposition experiments with those reported in the literature and it was found that the Pd/D co-deposition generated triple tracks were similar to nuclear-generated triple tracks. The intent of this effort was to expose CR-39 detectors to a DT neutron source and to compare the resultant triple tracks with those we have observed in our Pd/D co-deposition experiments. Figures 3 and 4 of our paper [6] show side-by-side comparisons of symmetric and asymmetric triple tracks in CR-39 detectors used in Pd/D co-deposition experiments and exposed to DT neutrons. The two sets of tracks are indistinguishable.

Since the on-line publication of our paper [6] comparing DT and Pd/D co-deposition generated triple tracks in CR-39, Kowalski has asked why the CR-39 detectors were exposed to the DT neutron source for 4.5 hours. There is no significance in the exposure time used to irradiate the CR-39 detectors. Our intent was to obtain a large sampling of DT-generated triple tracks to compare with those triple tracks observed in the Pd/D co-deposition experiments. This is particularly important given that the carbon break-up reaction can proceed to the four-body final state through one or more of the following reaction mechanisms [7] .....The observed shape of the triple track in the CR-39 detector is, consequently, dependent upon the reaction mechanism that occurred.

Kowalski has also asked why the neutron flux was reported in units of n s^-1 as opposed to n cm^-2 s^-1. That was because nine CR-39 detectors were placed on the neutron tube. The position of those detectors relative to the target was not known. Therefore, the distance between the detectors and the target, r, was not known. Consequently it was not possible to report the flux in n cm^-2 s^-1. Furthermore, the purpose of the experiment was to see what DT neutron tracks look like and to compare them with the tracks observed in our Pd/D co-deposition experiments. Those goals were met. As this was a qualitative comparison between DT-generated triple tracks and Pd/D co-deposition triple tracks, Kowalski's comments on flux units and exposure time are not relevant. We also note that Kowalski does not dispute the fact that one cannot differentiate DT-generated triple tracks and Pd/D co-deposition triple tracks.

Kowalski has asked ?What was a typical number of triple tracks found after 4.5 hours of DT irradiation of CR-39.? That was not the goal of this effort and again his question is not relevant to the results presented in our paper [6]. However, Kowalski's question was addressed by both Abdel-Moneim and Abdel-Naby [3] and Phillips et al. [8].

Kowalski has indicated that the observation of "at most 5 to 10 triple tracks" in CR-39 detectors used in Pd/D co-deposition experiments is a very small number and suggests that it would be more significant to list the total number of triple tracks found in all co-deposition experiments. Again Kowalski's statement is not relevant. We would like to point out that we only look for triple tracks in areas where the track density is low. Consequently, the number of triple tracks is, in all likelihood, under-reported. Also we have conducted over a hundred experiments using CR-39 detectors. This includes Pd/D co-deposition experiments as well as control experiments. As we noted in our papers [1,6], triple tracks have only been observed in experiments involving the use of palladium and deuterated water. They have not been observed in copper electroplating experiments nor in Ni-screen electrolysis experiments in either D_2O or H_2O. Nor have triple tracks been observed in blank detectors.

Kowalski states that "the world is waiting for a reproducible-on-demand and convincing demonstration of a chemically-induced nuclear reaction.? He then asks ?what kind of co-deposition experiment is more likely to lead us to that end" The intent of our research is not to provide the world with such an experiment. Our intent is to conduct experiments to gain a better understanding of the phenomenon. CR-39 is simply one tool we are using to gain that understanding.

Kowalski's last sentence "The last SPAWAR paper focusing on rare events, can be contrasted with earlier co-deposition papers were the reported tracks were much more abundant" is puzzling. Presumably this statement is meant to discourage us from reporting on the existence of triple tracks by disparagingly saying that they are too few to warrant further attention. It should be noted that the discovery of new subatomic particles and CP symmetry violations in physics have been the result of "rare" events. The rarity of such events does not invalidate them. There are other such examples in nature. Clearly Kowalski does not appreciate the significance of triple tracks in CR-39 detectors used in Pd/D co-deposition experiments. Nor does he appreciate the fact that the Pd/D co-deposition triple tracks are indistinguishable from DT neutron generated triple tracks. For us to not report on the existence of triple tracks in our Pd/D co-deposition experiments would have been scientifically negligent.

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Referee 2:

The SPAWAR group has been researching particle emission from Pd codeposition experiments for several years, and they have published a variety of remarkable results. Kowalski has now for years played the role of the critic, skeptic, or questioner. In the present manuscript, Kowalski is continuing this role.

Two of his comments/questions refer to the exposure of CR-39 plates by a neutron source; he wants to know why a 4.5 hour exposure was used; he points out the 10 million neutrons/sec is not a flux, and he wants to know the flux.

One of his comments has to do with the statistical significance of the triple tracks.

The final comment seems to have to do with strategy, but is in actuality a criticism.

It is the case that the SPAWAR group should probably have given an integrated flux number as neutrons per unit area. We presume that they picked the exposure time in order to get a useful signal. This issue of how many such tracks have been seen is important, and readers would benefit if SPAWAR could shed light on this. The final comment/criticism is not as helpful or interesting.

There is no physics contribution in this manuscript. It would be appropriate for a simpler version of this comment to be published, along with a short response from SPAWAR immediately following.

However, given the negative history of the interaction between Kowlaski and the SPAWAR group, we need to be mindful to minimize the nontechnical content of the comment. It should be revised to a few sentences that request

(1) clarification of the integrated neutron flux in units of neutron per unit area; and

(2) additional information about how many triple tracks have been seen total in how many experiments, and perhaps additionally how many hours of run time.

Referee 3:

This paper does not contain any scientific advance, nor any improvement on earlier work.

3) Quick comment (I will probably write more later).

a) I agree with the Referee 3; the purpose of my short note was to ask for clarifications, not to publish new information. I expected my note to be published in the same issue in which the three questions would be answered by one of the SPAWAR authors.

b) Referee 2 wrote “given the negative history of the interaction between Kowlaski and the SPAWAR group ...” That hurts; I am trying my best to be useful. Do not forget that I was among the first CMNS researchers who published results that were identical to those published by the SPAWAR team. I started using solid state track detectors in 1961 (using mica, not CR-39). A year later I wrote a review article on the subject (in a Polish journal “Postempy Fizyki,” volume 13, page 463). I summarized what I learned from an American, R. Walker, in France. He brought this new method of detection to Europe; I was probably the first European to observe fission fragments with them.

Offering constructive criticism should not be called “negative interactions.” I would be very happy to have friendly interactions with SPAWAR people; I admire their work. Their results are reproducible on demand and that is very important.

c) Referee 3 wrote “Kowalski has indicated that the observation of “at most 5 to 10 triple tracks” in CR-39 detectors used in Pd/D co-deposition experiments is a very small number and suggests that it would be more significant to list the total number of triple tracks found in all co-deposition experiments. Again Kowalski’s statement is not relevant.”

I disagree, it is very relevant, considering the context in which this observation was made. Here is that context again :

My first comment has to do with the number of tracks. As stated in Subsection 3.3, codeposition experiments “typically last two weeks.” The number of triple tracks per detector was “at most 5 to 10, including both the front and back surfaces.” That is indeed a very small number. It is reassuring that no triple tracks “have been observed in background monitoring detectors.” That rules out the “cosmic rays” interpretation. This argument would be stronger if the total numbers of experiments were reported.

Suppose that ten detectors are used in all codeposition experiments, and that the mean number of tracks per detector is 5. Then the total would be about 50 triple tracks. Suppose that only five triple tracks were found on ten background monitoring detectors. The difference between 50 and 5 would certainly be more significant than the difference between 10 and 3.

I was a little surprised that not a single triple track was observed in background detectors. My expectation would by about 3 or 5, in ten such detectors. That is why seeing 50 would be more reassuring than seeing only 10. But I do understand practical difficulties mentioned by Referee 3. (By the way, my guess about ten chips exposed to neutrons was actually very close to reality. The actual number was nine, as written by the Referee 3 above.)

Yes, the final comment was not necessary. It was not an attempt to “discourage” anything; it was a statement about the strategy I would prefer. I would focus on what has already been shown to be reproducible on demand and would start “playing with parameters.” But, as they say in Russia, “na wkus i na cviet towarishchej niet.” Or “in matters of taste there are no friends.”

4) Additional comments and observations (to be added)

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394) Philosophical Aspects of Cold Fusion Controversy

Ludwik Kowalski (see Wikipedia)
Professor Emeritus
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March 7, 2012

Abstract
The field of Cold Fusion (CF), now called Condensed Matter Nuclear Science (CMNS), remains controversial. The original 1989 claim made by M. Fleischmann and S. Pons (F&P) was that a chemical process in an electrolytic cell could initiate a nuclear reaction-fusion of two deuterium nuclei. This conflicted with known behavior of such nuclei. In the US, CMNS claims were evaluated by the Department of Energy (DOE), in 1989 and 2004, as summarized in this article. The controversial F&P claim, and the DOE investigation of it, are examined in the context of scientific methodology of validation of claims, both empirical and theoretical. Neither F&P nor the DOE followed the expected methodology.

1. Introduction

The philosophy of science is concerned with the assumptions and methods of scientific explorations. One of these assumptions is that activities of scientists are properly described by Merton's **CUDOS** norms: **C**ommunalism (discoveries are not private); **U**niversalism (of the scientific methodology of validation of claims); **D**isinterestedness (of scientists motivated by love of truth); **O**riginality (search for new data and explanations); and **S**crutiny (checking and double-checking of claims). The purpose of this essay is to address these norms in the context of the still ongoing Cold Fusion controversy. That controvesy--started in 1989 (1,2,3,4,5,6)--divided physical scientists into two feuding camps (7,8,9,10).

Scientific methodology refers to the set of norms developed to deal with difficulties, especially with mistakes and controversies. Most scientific mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other researchers. Scientific results, if valid, wrote Huizenga (5), must be reproducible on demand. “When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press.” The scientific process, in other words, is expected to be self-corrective.

The purpose of this presentation is to analyze the ongoing CF controversy, an example of a situation in which self-correction has not worked for 23 years. Why is it so? The author of this article, and three other researchers, tried to verify one recent CF claim-emission of alpha particles during electrolysis. The results were negative, as described in (11). The most recent CF claim was made by an Italian engineer, Andrea Rossi (12), the inventor of a new kind of nuclear reactor. That claim cannot be independently verified because of imposed secrecy. Rossi's appeal to believe in the efficiency of his "secret catalyst" is not consistent with the methodology of validation practiced by scientists and engineers. His claim may or may not be valid. But there is not a good reason for accepting it as truthful. Secrecy is certainly not consistent with the "scrutiny norm" of CUDOS.

2. In Science Theories Guide But Data Decide.

Why is the CMNS controversy started in 1989 unresolved? Because CF claims are still not reproducible on demand, and because they conflict with accepted theories. A theory, in this context, is a logical/mathematical structure that agrees with a wide range of already verified experimental data. Empirical scientists know the rule-theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on

additional verifications of new experimental results. A recent claim that neutrinos travel faster than light, for example, was received with great skepticism; it has already been shown to be due to an experimental error.

Referring to such situations, Huizenga wrote: “There are occasionally surprises in science and one must be prepared for them.” Theories are not carved in stone; scientists do not hesitate to modify or reject theories when necessary. Rejecting a highly reproducible experimental result “on theoretical grounds” would not be consistent with scientific methodology. Unlike mathematics (and other formal sciences), empirical science is based, in the final analysis, on experimental data, not on logical proofs. In that sense methods of validation of claims in physical and social sciences are similar. Scientific theories are models of objective reality; they are changed, or modified, when new facts are discovered.

Mathematics, on the other hand, is more like theology than like empirical science. A mathematical truth, called a theorem, is based on initial undeniable assumptions (axioms), and on logical reasoning based on them. The only way to refute a theorem is to find a logical error in its derivation. In that sense mathematical or theological truth is said to be eternal; it is not refutable by experimental data. One similarity between theology and mathematics, however, should not prevent us from seeing important differences; one of them has to do with disagreements about axioms. Such disagreements among theologians are frequent; disagreements among mathematicans are rare.

3. Scientific Claims: Data And Explanations.

Basic principles of scientific methodology of validation of claims are usually known. Less widely known are differences between validation of facts and validation of scientific theories. A typical process of acceptance of a fact is schematically illustrated in Figure 1 below. A preliminary experimental result is presented to a scientific community. This often leads to attempts to either confirm or refute what is presented. Results are accepted when they become reproducible on demand. Absence of such reproducibility puts data into the domain of protoscience, which may or may not become science.

More important claims call for greater scrutiny, against possible errors and possible fraud. Fraudulent data have recently been discovered in applied sciences, for example about efficiency of certain drugs or medical treatments. Such episodes are not consistent with CUDOS rules. How can they be explained? Complexity of research and its fragmentation--large teams working on small parts of projects and not communicating with each other--is probably an important factor. Such situations are more common today than when CUDOS norms were formulated.

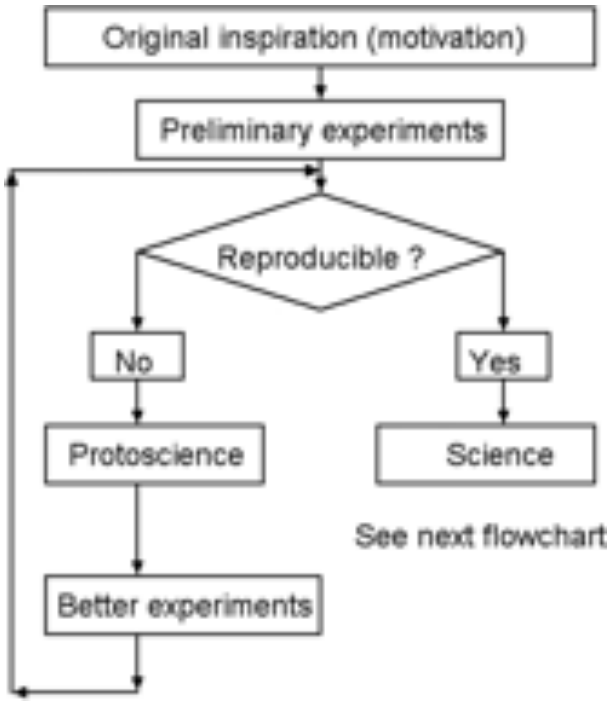

Figure 1

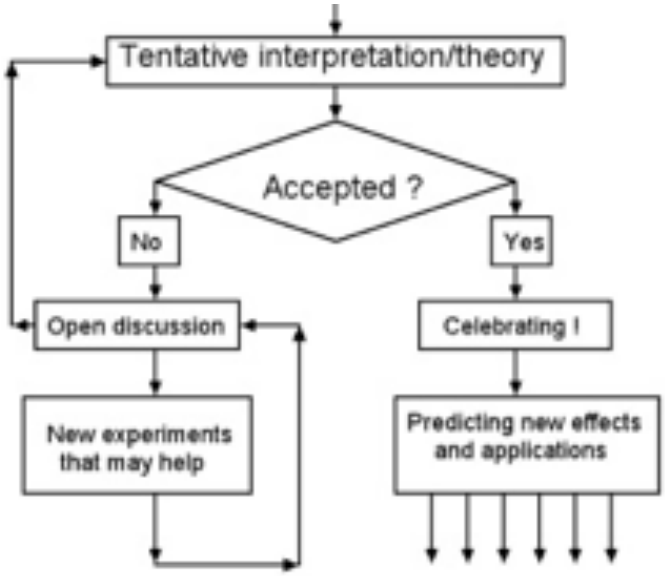

Figure 2

The "love of truth," a component of CUDOS, refers to the pleasure scientists derive from discovering facts and explanations. To explain something usually means to identify causes, and to construct logically satisfying models of reality. The process of acceptance of an explanation, schematically illustrated in Figure 2, is often the continuation of the process diagramed in Figure 1. An attempt to explain one fact, or to resolve an apparent logical conflict, often leads to discoveries of other facts.

A classical example of this was the "theoretical prediction" of the existence of planet Neptune, in 1846. A more recent, and less widely known example, was the discovery of a subatomic particle named neutrino. Experimental data, collected in 1920's showed that most beta rays (electrons emitted in beta decay) had energy lower than expected on the basis of the $E=mc^2$ formula. Austrian theoretical physicist W. Pauli solved this "logical inconsistency" by suggesting that tiny neutral particles, later named neutrinos, are responsible for the missing energy. His hypothesis was formulated in 1933. Experiments designed to confirm reality of neutrinos turned out to be successful, 23 years later. They were performed by American experimental physicists, C. Cowan and F. Reines.

4. The original mistake of Fleischmann and Pons

The strategic mistake made by F&P was an attempt to interpret experimental data before their results were recognized as reproducible (see Figure 1). Trying to establish priority, under pressure from Utah University, the scientists announced their results at a sensational press conference (March 23, 1989). At the same time they claimed that the reported amount of excessive heat was due to fusion of deuterium nuclei -ionized hydrogen atoms introduced into palladium.

. . . . .  . . . . Martin Fleischmann, on the right, with the author of this article (2003)

It is well known that two hydrogen nuclei can fuse, releasing energy. But this happens only at extremely high temperatures. At ordinary temperatures the probability of the reaction is practically zero, due to the well-known coulomb repulsion of positive nuclei. F&P had no evidence that the excessive heat was due to a nuclear reaction. They wanted to study the CF phenomenon for another year or so but were forced to announce the discovery by the university administrators (13). The only thing they knew was that the excessive heat could not be attributed to a

chemical reaction.

Suppose their experimental results had been described without any interpretation, and the phenomenon had been named “anomalous electrolysis.” Such a report would not have led to a sensational press conference; it would have been made in the form of an ordinary peer review publication. Only electrochemists would have been aware of the claim; they would have tried to either confirm or refute it. The issue of “how to explain the heat” would have been addressed later, if the reported phenomenon were independently confirmed.

But that is not what happened. Instead of focusing on experimental data (in the area in which F&P were recognized authorities) most critics focused on the disagreements with the reliable hot fusion theory. Interpretational mistakes were quickly recognized and this contributed to the skepticism toward their experimental data. Jumping from the process described in Figure 1 to the process described in Figure 2, was premature.

5. First Investigation Of CF By The US Government

The significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by “vested interests” of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation. A panel of scientists, named ERAB (Energy Research Advisory Board), and headed by John Huizenga, was formed to investigate CF in 1989. The final report, submitted to the DOE several months later, interfered with the normal development of the field.

It should be noted that ERAB scientists investigating the CF claims were not personally involved in replications of experiments. Conclusions and recommendations from their report (14), based on visits to several laboratories rather than participation in experiments, are summarized in the Appendix. Only one of their conclusions (item 2 in the Appendix) refers to CF experiments. Conclusion 4 was about anticipated practical uses of CF while the remaining four conclusions (1, 3, 5, and 6) were about various aspects of the suggested interpretation of experimental results. Instead of focusing on reality of excess heat critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion.

The same observation can be made about the ERAB recommendations. Only one of them (item 9) refers to possible errors in experiments. Items 7 and 8 refer to future funding while items 10, 11, and 12 refer to what was expected on the basis of the suggested hot-fusion interpretation. It is clear that the ERAB observations were based mostly on ”theoretical grounds,” and not on identified errors in experimental data. Recommendations about future financial support for CF were very important. But the DOE ignored them. Support for CF research practically stopped in 1989.

Another result of the first DOE investigation was that editors of some scientific journals started rejecting manuscripts writen by CF scientists, bypassing the peer review (25). They also started to impede communication between nuclear scientists. Sixty letters to the editor of Physics Today--related to the second DOE investigation--were received, according to (26), but not a single one was published. Editorial discrimination directed against a targeted group of nuclear science experts is certainly not consistent with CUDOS norms. How can it be explained? Why was the scientific methodology of validation of claims-theories guide but experiments decide - not followed by the DOE-appointed scientists? Why did “rejections on theoretical grounds” prevail?

6) Second Investigation Of CF By The US Government

The second DOE investigation of CF was announced in March 2004, nearly 15 years after the first one. Links to three online documents related to that investigation - Conference Agenda, Meeting Agenda, and DOE CF Report - can be found in (15). The six most important scientific questions, based on new experimental claims, were:

(a) Is it true that unexpected protons, tritons, and alpha particles are emitted (9, 16) in some CF experiments?

(b) Is it true that generation of heat, in some CF experiments is linearly correlated with the accumulation of ^{4}He

. . . and that the rate of generation of excess heat is close to the expected value of the 24 MeV per atom of ^{4}He (9, 17)?

(c) Is it true that highly unusual isotopic ratios (9, 18) have been observed among the reaction products?

(d) Is it true that radioactive isotopes (9, 19) have been found among reaction products?

(e) Is it true that transmutation of elements (20, 21) has occurred?

(f) Are the ways of validating of claims made by CF researchers [see conference reports presented at (16, 17,18)]
. . . consistent with accepted methodologies in other areas of science?

A positive answer to even one of these questions would be sufficient to justify an official declaration that cold fusion, in light of recent data, should be treated as a legitimate area of research. But only the (b) question was addressed by the selected referees. They were asked to review the available evidence of correlation between the reported excess heat and production of fusion products. One third of these referees stated that the evidence for such correlation was conclusive. That was not sufficient; the attitude of the scientific establishment toward cold fusion research did not change.

7) Conclusion

Long-lasting controversies about scientific discoveries are widely known. Let me mention "Alfred Wegener, whose hypothesis was rejected by the 'cognoscenti' for half a century, but which now represents the basis, not simply for a theory, but also for a true paradigm shift in science - plate tectonics." I am quoting from a comment made by a university colleague, Dr. J.C. Delany. The CF controversy seems to be different in terms of its intensity and caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. Most CMNS researchers are also Ph.D. level scientists. The same is true for those scientists who believe that the announced discovery of CF was a “scientific fiasco”.

Why was so little accomplished to solve the CF controversy during more than two decades? Because CUDOS, and other rules of scientific methodology, were not followed. Scientists are only human; competition among them, as among other groups of people, tends to produce not only positive but also negative influences. Cold fusion will be viewed as an interesting episode in the history of science, regardless of verdicts about validity of numerous CMNS claims. More specifically, the long-lasting CF episode will be remembered as a social situation in which the self-correcting process of scientific development was not allowed to flourish. To what extent was this due to extreme difficulties in making progress in the new area (without financial support from the DOE, NSF, etc.), rather than to negative effects of competition, greed, jealousy, and other "human nature" factors? Such unanswered questions are worth addressing in the context of philosophical debates about science and society.

8) Appendix: The 1989 US Government Investigation of CF

As stated in Section 4, the first investigation of the CMNS field took place in 1989. The final report, submitted to the DOE several months later, interfered with the normal development of the field. It should be noted that ERAB scientists investigating the CF claims were not personally involved in replications of experiments. Their report (14), based on visits to several laboratories rather than participation in experiments, can be summarized by the following statements:

Conclusions:

1. There is no evidence that a nuclear process is responsible for excess heat.
2. Lack of experimental reproducibility remains a serious concern.
3. Theoretically predicted fusion products were not found in expected quantities.
4. There is no evidence that CF can be used to produce useful energy.
5. The CF interpretation is not consistent with what is known about hydrogen in metals.
6. The CF interpretation is not consistent with what is known about nuclear phenomena.

Recommendations:

7. We recommend against any extraordinary funding.
8. We recommend modest support for more experiments.
9. We recommend focusing on excess heat and possible errors.
10. We recommend focusing on correlations between fusion products and excess heat.
11. We recommend focusing on the theoretically predicted tritium in electrolytic cells.
12. We recommend focusing on theoretically predicted neutrons.

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395) A strange story worth recording

Ludwik Kowalski

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July 23, 2010

(1) On July 21, 2010 I learned about a new SPAWAR paper. It was published in EPJ AP, on July 7, 2010. The title is "Comparison of Pd/D co-deposition and DT neutron generated triple tracks observed in CR-39 detectors;" the authors are: P. A. Mosier-Boss, J. Y. Dea, L. P.G. Forsley, M. S. Morey, J. R. Tinsley, J. P. Hurley and F. E. Gordon. I downloaded that interesting article and informed the CMNS list about its existence. After sharing the link I wrote: I will read their article more carefully later. The first quick reading convinced me that a big step froward has been made by SPAWAR team. I hope their article will be discussed on this forum."

After reading the article I posted this message on the CMNS list (also on July 21): "I suspect that in order to estimate the neutron "flux" (at the end of Section 2) the number 10^7 should be divided by $4*PI*r^2$, where r is the distance between center of the source and the irradiated CR-39. Is this correct? If so then how large was r? "
(2) Another list subscriber, Abd ul-Rahman Lomax, responded (also on July 21): "I think there is a misunderstanding or incomplete text here. The 'flux' has been stated, there, for the exposure from a "Thermo Fisher Model A290 neutron generator," as 10^7 sec^{-1}. Either there is a missing unit, i.e., "cm^{-2}," or the figure is total neutron emission from the source, which then would require a distance (and specified intermediate material) in order to be meaningful, quite as you note. My suspicion is that the "cm^{-2}" was simply omitted, since we really only care, in understanding what's happening at the detector, about the actual flux passing through a particular area, not total neutron emission, and the flux would also be dependent upon beam pattern, etc. If I'm correct, then, the figure is 10^7 neutrons/cm^2/sec, for 2.5 hours, which specifies the figure we need to know. Otherwise, without the other data, the figure is almost meaningless.
(3) Responding to the above (on July 22) I wrote: "Yes, indeed. The effect produced by neutrons in a given location is directly proportional to the flux [n/(s*cm^2)] while the source is usually described in terms of its intensity [n/s]. The pending question is about 10^7. Is this the flux or is it source intensity?" Why did no one from SPAWAR answer our questions ?

(4) On July 22 I received a message from X. Actually, it was not sent to me; it was sent to edjap (typing error in this address), and to Pamela Boss. The author has a 1964 BS degree in history and physics and a 1967 MA degree in psychology). Prompted by a detail from that above message I responded (sending the CC to Pamela Boss, but not to epjap): "Suppose the electrodes (anode and cathode) are removed from a SPAWAR cell. Suppose the 6kV DC is applied to copper plates in contact with external parallel surfaces of the plastic cell. My expectation is that the field (due to 6 kV) inside the electrolyte would be very small but not zero.
- - - - - - - - - - - - - - -
Let me elaborate. All materials, even plastics have finite resistance, often many giga-ohms. Suppose each wall of the SPAWAR cell has the resistance 30 giga-ohms. The resistance of the electrolyte is much smaller. Suppose it is only R2=100 ohms.

a) How large is the electric current flowing from one copper plate to another? V=6000 V, R=60 giga-ohms=$6*10^{10}$ ohms

I=V/R=6000/6*10 =10 A or 0.1 micro-ampere.

b) What is the potential difference between the walls (inside the electrolyte)?

U=I*R2=10^{-7}*100=10^{-5} volts=10 micro-volts

c) How large is the electric field inside the electrolyte (assume the distance between the walls is L=2 cm)?

E=U/L=10/2=5 micro-volts per centimeter. This is a very low fields, in comparison with 6000V / 2 cm = 3000 V/cm

For an ideal dielectric R would be infinity, I would be zero, and E would be zero. But ideal dielectrics do not exist."

(5) The above was sent to X last night. Several hours later (on July 23) X responded. The reply was addressed to epjap@edpsciences.org; with my name in the CC field. I was not aware that this was a formal submission of a comment to European Physics Journal Applied Physics (EPJ AP).

(6) Then I received the message from EPJ AP. They wrote: "We are pleased to tell you that we have received [your] manuscript and we would like to thank you for choosing EPJ AP. As you are aware, we can only consider articles that have not been published or are currently submitted for publication in any other journals or another EPJ section. We will inform you of the editorial decision as soon as possible."

The EPJ Inquiry System at : https://articlestatus.edpsciences.org/is/epjap/ is at your service to follow up the status of your article. Your author ID for access is: ..... and article reference number: ....

(7) Replying immediately, I wrote: "I did not submit any manuscript to epjap yesterday. Please ignore anything submitted last night under my name. It was probably a joke. Also, please send me that manuscript. Thank you in advance. "

(8) They responded: "The message has been sent by Rich Murray. It seems to be a comment on an article which has been accepted for publication (please see the attached file)." That file was nothing else but what I wrote to X. I am the first author, he is the second author.

(9) Here is my second message sent to epjap: "Thank you for the quick reply. Rich Murray, who I do not know, did ask me a question yesterday. It was about the electric field. (The cell has flat plastic walls. The outside surfaces of these walls are coated with copper and 6000 V dc is applied to copper. How strong is the electric field inside the electrolyte?)

I answered Murray in a private message. Without asking for permission, he took my reply and submitted it as a comment. I stand for what I wrote, and I have nothing against his comment. But I do not want to be the co-author of it. Please remove my name. I did read the last SPAWAR paper (about triple tracks) with great interest and would like to comment on it. Please describe the rules governing your forum."

(10) Their response was: "We have removed your name on the comment sent by Murray Rich. Unfortunately, we don't have any forum on our web site but if you want to make a comment on the article, you could send us the comment and we will send it to the authors in order to know whether they want to make a reply or not. (As you have already done for the article "The Use of CR-39 in Pd/D Co-Deposition Experiments")

(11) Why did no one from SPAWAR answer questions about the flux? They were posted on the CMNS list two days ago. Frustrated by this I quickly composed the same questions in the form of a short note, to be submitted to EPJ AP. Then I read the latest SPAWAR paper again and formulated several more questions. A two-sentence note became a one-page manuscript (with five references). I submitted it. The title is "Four questions and one comment." Will they publish it? It depends on the referees. I would very much prefer to discuss the paper on our private CMNS list.

(12) I am glad that my name was removed; most experiments described in the last SPAWAR paper were performed without using the electric field. In the last paragraph of my submitted manuscript I wrote: "The world is waiting for a reproducible-on-demand, and convincing, demonstration of a chemically-induced nuclear reaction. What kind of codeposition experiment is more likely to lead us to that end?" In other words, should we be focusing on extremely rare triple tracks (at most 5 to 10 per 4 cm^2 during two weeks of electrolysis) or on much more abundant tracks discovered in earlier codeposition experiments? My advice, to those who want to contribute, would be to return to the first SPAWAR protocol. I was one of several people who used it independently, and who reported the same results as Pamela et al. That was in 2007. I would prefer to study more abundant tracks, trying to show that at least some of them are due to nuclear reactions induced by electrolysis. Neutrons are not more convincing signature than charged particles. What is wrong with using the same protocol over and over till a convincing yes or no answer is obtained?

(13) Today (July 24) the question was finally answered, by one of the authors of the paper. As I suspected, the 10^7 is not the flux. Responding to the clarification I wrote: " My recollection is that neutrons are generated in a metallic foil loaded with tritium. The foil is bombarded with a focused beam of D ions, accelerated to a chosen energy, for example, 1 MeV. The foil is located in a vacuum chamber. Suppose the distance between the source and the CR-39 detector is 10 cm, as guessed by Akito. In that case $4*PI*r^2=1256$ and the flux is $10^7/1256$, or nearly 10^4 n/(s*cm^2). But the "neutron pipe" might be longer than 10 cm. The flux would be only ~ 100 n/(s*cm^2) if r were 100 cm. I am assuming that the source is not pulsed. For a pulsed source the 10^7 n/s might stand for the source intensity during each pulse, and the mean intensity (over long time) could be several orders of magnitude smaller.

Why am I digging into this? Because I want to understand why the irradiation time had to be as long as 4.5 hours."

(14) Responding to the above Akito Takahashi wrote: " On Jul 22, 2010, at 4:19 PM, Ludwik Kowalski wrote: *Yes, indeed. The effect produced by neutrons in a given location is directly proportional to the flux [n/(s*cm^2)] while the source is usually described in terms of its intensity [n/s]. The pending question is about 10^7. Is this the flux or is it the source intensity?*

Dear Ludwik, Pam kindly replied that the operator of A290 NG told 10^7 n/s yield. Accordingly, I revised my previous speculation, as follows:

**) " Basic data for $^{12}C(n,n')3$ alphas for 14MeV neutron incidence : 0.206 b for reaction total. Branching ratio to the ^{12}C excited state 9..64MeV: 33% (See K. Kondo, et al.: JNST, 45(2008)103-115). Sub-branching ratio to the four body direct break-up; the channel (4) of Pam; n' + 3Alphas; not exactly known, but let's assume 20%.

**) Cross section to the channel (4): 0.2x0.33x0.2=0.013b
**) Thermo Fisher Model A290 NG: http://www.thermoscientific.com/wps/portal/ts/ I did not find A290. I however guess it would be like an old KAMAN type pulsed DT neutron generator (NG) in shielded tube (several microsec pulse) to produce 10^{10} n/s on the order. Actual time-averaged yield was 10^7 n/s due to the pulse repetition.

**) Pam set her CR39 at about 10cm from NG (on the surface of NG tube), 14MeV neutron flux is 10^4 n/s/cm^2.

**) She irradiated CR39 for 4.5 hrs, so that 14MeV neutron fluence (flux times irradiation time) is: $4.5x60x60x10^4 = 1.6*10^8$ n/cm^2

**) Reaction (event) rate of the channel (4) for producing triple tracks is: Assuming ^{12}C density in CR39 as $6*10^{22}$ C/cm^3 and 0.1 cm thickness of CR39, Triple track events: $(6*10^{22})*(0.013*10^{-24})*(1.6*10^8) = 1.2*10^5$ [tracks/cc] Triple track-events by $1cm^2$ CR39: $1.2*10^4$ tracks/cm^2

**) From Fig.1 of Pam's paper (EPJ AP 51 (2010) 20901), we assume one triple track in $0.025*0.025$ $cm^2 = 6.25*10^{-4}$ cm^2 area of an optical microscope observation for comparison with Phillips. Pam would find $(1.2*10^4)*6.25x10^{-4}= 7.5$

by NG. This is a total number of stars (triple tracks) in a CR39 with one cm^2*0.1cm size. We need to consider the actual etching condition of CR39 after irradiation.

**) In Pam's co-deposition experiments, she found 5-10 tracks per a CR39 detector. This result would correspond to $[(1.6*10^8)/(1.2*10^4)]*(5-10)= (1.3-0.65)*10^5$ fluence of around 14MeV neutrons. It corresponds: 0.16*(1.3-0.65)= 0.1-0.05 n/s/cm^2 flux of DT neutrons, if we assume two weeks run of her co-deposition experiment. CR39 was set very close to the cathode of co-deposition, so that its solid angle to see the source (cathode) is about 2*pi. Neutron (maybe DT) production rate in the co-deposition experiment is estimated to be 0.2-0.1 n/s.

**) If about 100mW excess heat observed were nuclear origin (suppose 10^{11} events per a joule), the observed level of "DT" neutrons was very low, on the order of 10^{-11} of the nuclear heat level.

BTW: Neutron energy Pam estimated by Eq.(5) would be En – En'. This might mean that the incident neutron energy were more than 13.46 MeV. As we do not know En', inelastic scatted neutron energy, of the corresponding event of the star (triple track), we may merely say so.

My speculation may be too much at the moment. We shall wait for a quantitative study by the SPAWAR group

(15) Next morning I wrote: "Thank you Akito. Several years ago I exposed CR-39 chips to neutrons of several MeV (from a Pu-Be source). At least 99% tracks seen after etching were due to protons, that is to hydrogen nuclei on which neutrons are scattered inside the CR-39 plastic. Occasional larger pits were most likely due to single alpha particles. I do not recall seeing triple tracks. This is not surprising; less then 1% of neutrons, from a Pu-Be source, have energies higher than 10 MeV. But I am surprised that SPAWAR CR-39 surfaces were not totally covered with highly overlapping pits due to recoiling protons, after 4.5 hours of exposure. That is why I suspect the flux might have been much lower than 10^4 n/(s*cm^2). We still do not know r, the distance between the neutron source (where deuterons are focused), and the irradiated CR-39. I suspect that your guess (r=10 cm) was an underestimation. But why should we be guessing? Even a very approximate answer would be sufficient."

(16) How important is the issue of tracks produced by neutrons from a DT source? The central issue is emission of neutrons due to electrolysis, presumably from a sequence of two fusion events. First two deuterons fuse producing ^3H. This is the CF reaction (7). Then the product of the reaction, ^3H fuses with another deuteron. This is reaction (9). Another sequence of two fusion reactions (8) and (10), is responsible for production of high energy protons.

Thermonuclear reactions 7, 8, 9, and 10 are well known. According to most textbooks, however, they are practically impossible at low temperatures. That is the essence of the cold fusion controversy. But in the context of SPAWAR hypothesis an attempt to compare tracks produced during electrolysis with track due to ~14 MeV neutrons is highly justified. I was very excited when I read, in the abstract, that "In this communication, triple tracks in CR-39 detectors observed in Pd/D codeposiotion experiments are compared with those generated upon exposure to a DT neutron source. It was found that both sets of tracks were indistinguishable." That is what prompted me to post the message quoted in (1) above. Yes, a step forward was made. But will it lead to a demonstration the world is waiting for? That remains to be seen.

(17) If I were to asked to verify the hypothesis advanced by SPAWAR--occurrence of reactions 7, 8, 9, and 10--I would focus on high energy protons, rather than on high energy neutrons. Ranges of high energy protons from reaction 10, in all materials, are much longer than ranges of alpha particles of several MeV. For protons with energies between 12 and 18 MeV the ranges in CR-39 are ~1.5 mm and ~3.0 mm. respectively. Some of these protons (incidence angles close to zero degrees) would travers the CR-39 deterctor (1 mm); others (larger angles of incidence) would be stopped in it. Many tracks, on both sides of the detector, would probably be strongly elliptical. This is only a guess; I have never exposed CR-39 to high energy protons. The local ionization density, due to protons with energies up to 30 MeV is still sufficiently high to produce etcheable tracks. (P.S. Googling the Internet I found a paper describing CR-39 tracks due to protons of 9.6 MeV and 30 MeV. I suspect that even 100 MeV protons might be detectable in CR-39.)

What energy would a 15 MeV proton have after traversing 1 mm (132 mg/cm^2) of CR-39? The answer is 10.5 MeV. Protons of that energy are cerainly detectable in CR-39. In other words, a 15 MeV proton, traversing a CR-39 chip perpendicularly, would produce a circular pit at the exit side of the detector. The diameter of that pit would be about 1/3 of the diameter of a pit due to an alpha particle from Am-241. A similar (but slightly smaller) pit would be produced at the point incidence. Some protons entering the detector at non-zero angles will exit the chip (producing a pair of pits), while others will be stopped in the CR-39 material (producing only one elliptical pit).

Recognizing strongly elliptical pits would probably be easier than recognizing triple tracks produced by neutrons from reaction 9. Furthermore, protons are charged particles and their detection efficiency is close to 100%. In other words, nearly every high energy proton crossing a CR-39 surface would probably create a visible track. The efficiency of detection of Mhigh energy neutrons, by contrast, is usually smaller than 0.0001%.

It is interesting that presence of strongly elliptical tracks is not mentioned in the SPAWAR paper. That probably means that no such tracks are produced in codeposition experiments. How can this be interpreted? Does it mean that the probability (cross section) of the CF reaction 8 is many orders of magnitude lower than the probability of the CF reaction 7? Note that thermonuclear reactions 7 and 8 have practically identical cross sections, in a wide range of energies. . . .

(18) Trying to get people involved I just posted another short message (on July 26). I wrote: "SPAWAR paper is very interesting. It is certainly worth discussing on this list. Please visit my just updated unit 395."

(19) It is 7/28/2010. I am surprised. My observation about strongly elliptical tracks due to protons from the Reaction 10 was posted two days ago. But no one commented on it. Even SPAWAR people remain silent. Why is it so? Their paper was published in a refereed mainstream journal. In their place I would be pleased to address reader's concerns immediately. Something is not normal. Am I asking nonsensical questions? Is my form of asking them not appropriate?

Another comment about still unspecified neurton flux was posted today by Abd ul-Rahman Lomax. He reminded the list that the source strength (approximately 10^7 n/s) was given to the SPAWAR team by the operator. But we cannot use this number unless the value of r is also know. He wrote: " I'd have assumed that the neutron source has a known intensity and known characteristics, such that the placement of the CR-39 in a particular position would produce a known flux per unit area per second. While this is not essential for seeing the characteristic triple tracks, for which it could be asked "Just give me a pile of neutrons," it is not only triple tracks that are of interest, it is also the far more numerous apparent proton recoil tracks. It should be possible to determine from the CR-39, from comparison of the track characteristics and density, two estimates of neutron flux integrated over the experimental period; the estimates from triple tracks and from proton tracks should be roughly equal.

But to do this we would have to know the neutron flux from the DT source, the total neutron emission of 10^7 /sec is almost meaningless, except to establish an upper bound. Surely this would be a known value to the operatiors of the equipment. I'm a little bit troubled by the appearance in a peer-reviewed journal of a number, presented as a fact, that turns out to be simply something 'told by the operators'. But perhaps I'm naive as hell. "

(20) Responding to the above, a member of the SPAWAR team provided information about circumstance under which the irradiation with fast neutrons took place. But nothing was said about the flux. This prompted me to post the following short message:

"What they [the SPAWAR team] did is reasonable. But what prevents them from calling the operators and asking about the distance between the irradiated CR-39 detector and the center of the source? The operators know what the distance was, at least approximately. That would be enough for us to estimate the flux. This additional information is likely to enrich the already-very-impressive results published by the SPAWAR team. Their work is admirable. Let us hope it will lead to what the world is waiting for. Best wishes to Pamela, and to her coworkers."

From the reply posted by Pamela I lerned that several CR-39 detectors were placed around the tube. My question

about the distance r was not answered again. My reply consisted of one sentence: “what was the diameter of the tube? ”

It is August 2. Responding to my question about the diameter, another member of the SPAWAR team posted a message which I prefer not to quote. It amounted to something like 'mind your own business and stop bothering us.' Once again, my question was not answered. I am disappointed. Why knowing the flux is so important? Because the reader of the paper must be able to compare what is reported with what is expected. The expected results cannot be calculated unless the flux, and the exposure time, are known. I have no idea why only the exposure time was specified (4.5 hours). How can this information be used when the flux is not given?

Yes, it is their experiment, not mine, as stated by the author of the last message. It is time to stop asking the same question. My disappointment is not with SPAWAR science, which is admirable. It is with human relations. Will my unit 396, announved the day before yesterday, bring some contributions? I hope so.

PS
R. Murray sent another message to the editor of EPJ AP. The CC was again sent to me and to Pamela Boss. Murray's submision was refused and he was commenting on the rejection. It was mostly about the electric field inside the cell. I do not know why Murray speculates about the "possibility [that the] solar system dark matter" may be involved. But his concern about the possible of "microleaks" makes sense to me. SPAWAR people are experienced chemists and their plastic cells were probably very clean. My calculation was based on this assumption. But suppose the opposite walls are coated with a conducting layer of dry electrolyte. In that case the electtic field inside of the the would be much higher than I calculated.

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Rossi again? I have nothing positive to report.

Ludwik Kowalski; 3/12/2012

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1) FeedBlitz often sends me articles about scientific-technological developments in the world. The title I received this morning is "Steven Krivit and the troubling case of Andrea Rossi." It is an interview with a science journalist Krivit about Rossi. I also followed Rossi's claims, as documented in unit 399 at this Cold Fusion website (click the link below). I think that Rossi's claims are not consistent with what is known and understood by authors of basic nuclear physics textbooks. That is why I am also not optimistic about Rossi's E-cats. But "not being consistent with what is known and understood" does not exclude the possibility that something new and important has been discovered by Rossi. It only lowers the probability of Rossi's claims being valid. That probaility is likely to be below 1%, in my opinion.

2) Most of you already saw the draft of my article **"Rossi's Reactors--Reality or Fiction?"** The final version has been printed in the January 2012 issue of Progress in Physics (pages 33-35). The online version is available at their website (Google will take you there).

3) Below is the abstarct of my "letter to the editor" of Progress of Physics. The letter, entitled **"Social Aspects of Cold Fusion: 23 Years Later"** will be printed in the April 2012 issue. The online version might be shown in March. Why am I quoting only the abstract? Because journals usually want to be the first to publish.

ABSTRACT: "The field of Cold Fusion, now called Condensed Matter Nuclear Science (CMNS), remains controversial. The original 1989 claim made by M. Fleischmann and S. Pons was that a chemical process in an electrolytic cell could initiate a nuclear reaction--fusion of two deuterium nuclei. More recent CMNS claims, made by experimental scientists, are: emission of charged nuclear projectiles during electrolysis; accumulation of 4He; production of radioactive isotopes; and transmutation of elements. In the US, CMNS claims have been evaluated in two Department of Energy (DOE) investigations, in 1989 and 2004, as summarized in this article. These investigations did not lead to any resolution of the controversy. Scientists and adminstrators are not ideal; competition among them, as among other groups of people, tends to have both positive and negative influences."

Comments, as always, will be appreciated.

Ludwik (see Wikipedia)

P.S. 3/6/2012

4) The entrire paper has just been published.
click to download it

5) The article entitled "Rossi's Reactors--Reality or Fiction," mentioned above, can also be seen:

click to download it

The paper shows why I think that Rossi's claims conflict with what is written in our nuclear physics textbooks.

6) Here is what a colleague X from Montclair State University posted, on a discussion list, after reding my "Social Aspects of Cold Fusion: 23 Years Later" article:

I believe that there is little controversy at this point. There are a handful of investigators who continue to pursue this hypothesis on a vain hope that it will prove to be correct in spite of the large body of evidence and existing knowledge to the contrary. It is not the responsibility of the scientific community to refute every hypothesis that is forwarded on the basis of irreproducible results. If those who continue to pursue CF can provide a working prototype and instructions that a competent scientist can follow to build it, then this "controversy" will instantly disappear. It is impossible to prove that CF cannot be induced by electrochemical means involving heretofore unknown physics. The onus of proof is on, and should remain on, those who make such extraordinary claims.

I believe that the scientific community has been unusually kind to the CF proponents. To propose that one has developed an energy source that violates much of currently accepted physics takes a certain type of individual. To continue to claim that it works without showing an independently verifiable prototype also takes a certain type of individual. Most of the time the term "charlatan' is applied.

On a side note, I do not know that Fleischmann was an indisputable leader in electrochemistry in 1989. I don't currently have access to the citation index to check on his pre-1989 contributions.

And here is my reply:

I agree that the burden of proof is on those who make claims. We cannot check all claims, especially those made by authors without scientific credentials. But I also think that the DOE, NSF, etc. have some obligation toward claims made by Ph.D. level scientists who worked in National Laboratories for decades, conducted first-class research and published widely in refereed journals. They should have access to referees as all other scientists. But editors of many scientific papers often reject CF papers by themselves. That is not normal. I am also skeptical about many cold fusion claims. But that does not mean that I should support topic-based discrimination.

You have seen how I reacted to a recent claim made by an inventor, Andrea Rossi (who is not a scientist). His claims conflict with what is in our nuclear physics textbooks. Does this mean that he cannot possibly be right that something totally unexpected has been discovered? No it does not mean this. But the probability that he is right is very low, most likely less than 1%, in my opinion. Neither the DOE nor the NSF should support his research. Let us wait and see; perhaps he is right in believing that his invention will be validated by a large number of satisfied customers. I am certainly not going to invest in his "great technology."

Several hours later X responded:

Hi Ludwik, I think we all *hope* that the CF side is right. I remember the excitement and skepticism in 1989... "Probably not, but what if?" We'd all like to see a cheap convenient way to exploit the fusion mass defect to produce energy. But, honestly, 23 years is more than enough time to make the Palladium-Deuteroxide reactor reproducible. That other approaches have been forwarded in the mean time seems a bit desperate to find some excuse to keep looking. So, I disagree about topic-based discrimination. We should not take papers on the Phlogiston Theory or on perpetual motion machines or on poly-water. If they want to publish in the top journals, let them produce a working reactor that others can replicate and test independently. If they are correct they will all be multi-billionaires and can laugh at us from their mansions.

A colleague of mine pointed out that Fleischmann was one of the first to report the surface enhanced Raman effect, though he erroneously attributed it to a concentrative effect. So I will take back any objections about him

having been a leading figure in electrochemistry. I wonder if his "miss" on surface enhanced Raman by being too conservative influenced his thinking 15 years later when he saw another unusual phenomenon in his lab.

My next-day reply was:

Those interested in Cold Fusion (CF), might enjoy reading McKubre's 2009 paper at the 15th International CF conference in ROME. The link is:

http://www.enea.it/it/produzione-scientifica/pdf-volumi/introduction-iccf15-proceedings-2.pdf

Mike is an electrochemist. I know him personally and I have no doubt that he is honest. In fact, he is a hero, in my view. How many people would be willing to continue probing for more that two decades under showers of spits? I think that he is motivated by the noble desire to help society.

Unfortunately, I did not go to the ICCF15 in Rome in 2009. But I did participate in, and contributed to, three earlier conferences (USA-2003, France-2004, and Japan-2005). My impression was, and still is, that most CF researchers (not all) are like Mike, and that their methodology of validation is scientific. Great scientists I met, including Frederic Joliot Curie, who introduced me to research, would agree. The difficulties described by Mike are real.

Subject-based discrimination (denying peer review process to Ph.D. level scientists) is harmful and totally unjustified. The DOE should support CF projects. A clear yes-or-no answer, about how to make excess heat experiments (described by Mike) reproducible on demand, could be obtained at a cost that is negligible (in comparison with how much is spent on supporting hot fusion studies). Yes, I am thinking about 0.1%, such as several million dollars versus tens of billions. Download Mike's paper and read it carefully; you will probably agree that there is nothing unscientific in it.

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Click to see the list of links to my other units

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More on spectacular claims of Andrea Rossi

Ludwik Kowalski; 3/12/2011

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1) The reactor invented by Andrea Rossi, about which I wrote a year ago (see unit 388 at this website), is again in the center of international attention, as one can see by going to

http://pesn.com/2011/01/19/9501747_cold-fusion-journals_warming_to_Rossi_breakthrough/

The recent public demonstration, at the Bologna University, could have been more effective than it was, without revealing the nature of the secret catalyst. Rossi could have provided the blueprint of the apparatus to a trusted authority, for example, Italian government laboratory, asking them to manufacture his simple device. They would bring it to the University of Bologna and allowed Rossi to place the secret fuel (nickel powder mixed with something else) into the cylinder. He would not be allowed to do anything else to the apparatus. That would eliminate a possible suspicion of a hidden energy source, somewhere within the apparatus.

This, however, would not eliminate another possible suspicion--that some chemical fuel was mixed with nickel. But suppose the powder supplied by Rossi is weighted, both before and after the experiment. Suppose the change in weight is negligible, in comparison with what it would be expected for the most effective chemical fuel. That would be a sufficient indicator that a non-chemical process was responsible for the released thermal energy.

2) Unit 388 reminded me that in the patent application Rossi claimed that "a practical embodiment of the inventive apparatus, installed on October 16, 2007, is at present perfectly operating 24 hours per day, and provides an amount of heat sufficient to heat [his small] factory." That was nearly four years ago. Suppose the rate of heating was 50 kW; suppose the heating time was 4 months per year (totaling 4*3=12 months, or 8640 hours). How much thermal energy was generated? The answer is

50*8640=4.32 * 10^5 kWh, or 4.32 * 10^5 * 3.6*10^6 = 1.555* 10^12 J

This is the same as from burning 5.29 tons of coal.

3) Was the thermal energy, reported by Rossi, released from a nuclear reaction? This would produce a measurable amount of reaction products. Addressing this issue, I posted the following message on the website for CMNS researchers, on January 24, 2011.

Andrea Rossi and Sergion Focardi, have a reactor capable of producing 12,400 watts of heat power from an electrical input of just 400 watts." In other words, the net heat generation rate is 12,400-400=12,000 W =12,000 J/sec = 7.7*10^16 MeV/sec

Suppose a typical fusion reaction contributes only 2 MeV of heat (escaping neutrinos, neutrons, and gamma take away the rest of the released energy). That would mean that nearly (7.7/2)*10^16, or about 4*10^16 fusion reactions is taking place each second.

Suppose that the atomic mass of a detectable combustion product is about 60 amu (1 amu=1.66*10^-27 kg).

What is the total mass of all products created during one hour of operation? The mass production rate is: 60*4*10^16=2.4*10^18 amu/sec=4*10^-9 kg/sec=4*10^-3 mg/sec

The mass produced in 36,000 second (10 hours) would be 36,000* 4*10^-3=144 mg. That would be detectable with a standard analytical balance.

The mass would be considerably larger for non-radioactive products (because no energy is removed from the reactor by neutrinos, neutrons and gammas).

The bottom line is obvious; detection of heat, at the rate of 12.4 kW, correlated with detection of nuclear combustion products, should be trivial, for qualified chemists.

The reactor described by Rossi (see above) operated for about 12 months (not only 10 hrs, as in my illustration). That is 864 times longer. The accumulated mass of reaction products (and their daughters, if any) would thus be 0.144*864=124 grams. For a reactor operating at the rate of ~40 kW (rather than 10 kW) the mass of the accumulated reaction products would be four times larger, or about 500 grams. In other words a sizable fraction of the nickel fuel would be consumed, producing 500 grams of new products. The isotopic composition of the spent fuel would be drastically changed. What prevents Rossi from announcing existence of such change? This can easily be done without revealing the nature of the catalyst. Both scientists and potential investors would be convinced that something totally new has indeed been invented by Rossi.

He is certainly aware of this. Here is what Rossi wrote in 'Journal of Nuclear Physics:'

http://www.journal-of-nuclear-physics.com/?p=62&cpage=2

Actually, this is a blog, not an on-line journal. But that is not my point.

(A) the Ni powder I utilized were pure Ni, no copper . At the end of the operations in the reactor the percentage of copper was integrally bound to the amount of energy produced. A charge which has worked for 6 months, 24 hours per day, at the end had a percentage of Cu superior to 30%

(B) About the Ni isotopes: the isotopes after the operations were substantially changed in percentage. We are preparing a campaign of analysys with a Secondary Ions Mass Spectrometer at the University of Padua (Italy), at the end of which the data will be published on the Journal Of Nuclear Physics.

Why were such spectacular results--conversion of at least 30% of nickel into copper and drastic changes in isotopic composition--were not published at a scientific conference, or in a real journal? That puzzles me. How can this be explained?

4) Here is how the most recent demonstration of the device was summarized in

http://pesn.com/2011/01/19/9501747_cold-fusion-journals_warming_to_Rossi_breakthrough/

'Focardi and Rossi demonstrated a novel boiler on 14 January 2011, which converted water at about 13 degr C to steam at 101 degr C. The device reportedly produced energy from nuclear reactions between nickel and hydrogen. An electrical heater in the device drew about 1000 W at startup. Later, once the reactions started and provided heat, the input power was reduced to about 400 W. Consumption of hydrogen gas was essentially negligible. The input water flow was about 150 grams each half minute. Given the measured input and output temperatures, that flow rate, and a measurement that the steam was dry, it is easy to compute that [in the steady state] the device delivered over 10 kW of thermal [power] to the water. '

The reactor, shown on available illustrations, was a small cylinder; the amount of nickel powder is probably less than two or three kilograms. Accepting this estimation, I conclude that the 30% of produced copper, reported by Rossi,

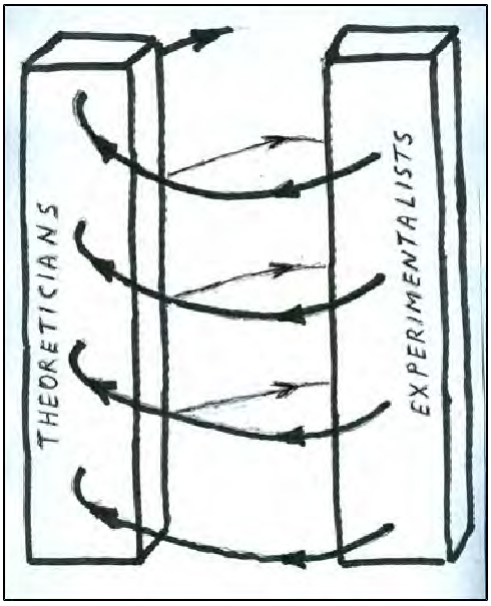
translates into about 1 kg, after a prolonged operation. My very conservative estimate, 144 mg after 10 hours of operation (see point 3 above) is probably consistent with what he reported.

According to Rossi (see the link to The Journal of Nuclear Physics in point 3 above) production of copper takes place when hydrogen nuclei, which are protons, fuse with nickel nuclei. Fusion of protons with nickel nuclei has been studied by many physicists, including myself (as reported in The Physical Review, Vol .163, Nr. 4, 1074-1077, November 1967). But our protons had the energy of 14.3 MeV, Rossi's protons, by contrast, had low temperature energies, close to 0.04 eV.

The probability of nuclear fusion, expressed in terms of measurable cross sections, is known to decrease rapidly with energy. How can 0.04 eV protons fuse with nickel, whose atomic number is 28? Rossi does not have to answer this question; his role is to provide experimental evidence that such fusion does occur, under the influence of a catalyst. I am sure that a theoretical explanation will be found, soon after his experimental results are independently confirmed, and after information about the catalyst is published.

One of the well known facts is that the cross sections of a nuclear reaction does not depend on the chemical composition of the target bombarded by protons. The target can be a crystalline nickel, a compound containing nickel, a melted nickel, or vaporized nickel. No exception from this rule has been found. But the rule has not been tested for all conceivable solid compounds. As far as I know, it has never been tested for protons of very low energy. Rossi, like several other scientists, probably believes that the rule is not universal and that exceptions are possible, especially at very low energies. His experimental results, if confirmed, would validate such expectations.

The 'all swans are white' rule, I remember reading somewhere, was valid up to the time at which black swans were discovered. In science, unlike in mathematics, validations, in final analysis, are based on experimental results, not on logical proofs. On the other hand, predictions based on logic, have often been successful; in other words, they were later validated by experimental data. Disagreements between new experimental results and existing theories should not be ignored. Yes, the 'theoreticians often guide while experimentalists decide.' But theories are based on verified results from experiments and observations. The chicken and the egg dilemma? Not really. Why not? Because the process of accumulation of scientific knowledge is not circular; it is spiral, as illustrated below.



Concecutive spiral loops, representing accumulation of knowledge about something, do not coincide. They wind throught two supporting blocks, labeled experimentalists and theoreticians. At any given elevation scientists in a given block exchange contributions with scientist in another block. What is being exchanged is never the same; in fact, it is usually very different at each level. Note that each block is both a receiver and a giver.

Added on May 11, 2011

The above was submitted to four mainstream journals, during the last two months. Unfortunately, it was rejected by editors (not by peer reviewers). The rejected manuscript can be seen at:

http://pages.csam.montclair.edu/~kowalski/cf/rossi.html

How can this be explained? This is not what editors are expected to do.

Added on May 13, 2011

The nature of the catalyst remains secret; independent replications are impossible. That is why most messages posted on the private list for CMNS researchars, have been theoretical. Here is a good summary, posted two days ago by Abd ul Rohman Lamax.

*Many theoretical physicists worked on CF theory, including more than one Nobel laureate. It is obviously a very tough theoretical problem. The first problem was that far too many, who might be otherwise competent, took a shallow look, said "impossible," and then dismissed the growing body of experimental evidence as *necessarily artifact, error,* since, *impossible.**

That was obviously a systemic failure, or, as Huizenga called it, perhaps not realizing the irony, The Scientific Fiasco of the Century.

*In normal problems with artifact, the research was done to demonstrate the artifact, cf. Polywater and N-rays. With both polywater and N-rays, there were *confirmations,* then demonstrations that the observations were artifact, not merely argument from impossibility. With cold fusion, there was only replication failure, and, by definition, replication failure cannot demonstrate artifact in the original work! It can only demonstrate one of two things: failure to replicate, conditions not the same or a chaotic effect, or, secondly, total error or fraud on the part of the original experimenter, or possibly some unidentified artifact that isn't reproducible. The latter possibility declines and rapidly vanishes with successful replication.*

If it's replicable -- and cold fusion was -- then any artifact should be identifiable, by controlling it in and out.

Once it was known that He-4 was being generated at the right heat ratio, and that was confirmed, as it was -- it's a reproducible and reproduced experiment, properly described -- it should have been all over. Before the close of the century.

But what is happening to cause this effect to appear? As Dr. Storms has pointed out, there are "plausible explanations," but no theory has been validated in any strong sense. Normally, what we want to see is predictive value, for theory is needed for prediction, as well as understanding. (Non-predictive theories can have a value for organizing thought, but not for engineering, and these are of some interest, but it's not really "scientific." Some non-predictive theories can usefully suggest avenues of approach, a good example might be the general wave theory of Dr. Irving Dardik.)

From my sense of the situation, any cold fusion theory is going to involve one or two "leaps," possibly more, that is, assumptions that lead to some possible understanding of a mechanism, but, because more than one are probably necessary, not to a complete understanding, adequate to make accurate predictions and answer objections. What is needed in this situation, my sense, is far more experimental discovery of the effect of controlled conditions.

My sense is that this is precisely what Rossi did. My own idea of approach would be to move in almost the opposite direction from much of the research, to design experiments that are smaller and cheaper and simpler, for thus it would become easier to vary the conditions, and to run the same experiment many times, to determine how much variation is still uncontrolled. Thus the effort to make it "bigger" and thus "better," worked against what is needed for understanding, particularly when funding was limited.

Rossi also apparently had a lot of his own money to put into this.

(In all this, I'm assuming that Rossi's heat is really being generated and is not fraudulently simulated. "Artifact," i.e., simple error in measurement or interpretation, seems highly unlikely at this time. Because of how much is at stake, "fraud" should not be casually discarded as a possibility, but I hasten to add that this is not an accusation of any kind, it is simple prudence. If there are hundreds of millions of Euros at stake, or more, what would be preposterous in a small scientific matter becomes not-so-inconceivable. Only when we have truly and fully independent replication -- which should happen this year if the effect is real and they are not blowing smoke, will we be able to leave "fraud" completely behind. And fraud may still take place in other ways! I predict there will be people who will claim other "discovery" who won't have actually done it, and further attempts to fleece unwary investors, even if Rossi and Defkalion are completely honest.)

So, right now, if anyone wants to pursue the Rossi track, and I know that there are people doing it, I suggest Smaller is Better. Make the tests as small as you can, consistent with showing some clearly significant effect. Depending on your calorimetry, 105% might be enough, it certainly was enough to show cold fusion with skilled calorimetry. Then keep varying it, a series of experiments that change only one variable, the more the better within one series. All the data is valuable, there are no "failures" in an approach like this.

As data like this becomes available, from those who choose to publish it, finding adequate theories will become easier. It will still be one of the most difficult puzzles in theoretical physics to come along for a long time, I predict. There are probably two miracles involved: how fusion or other LENR happens in the first place, and then how is the energy converted into heat, what happens to the expected gammas, etc.? Maybe someone will get lucky and propose a mechanism that, all by itself, causes the great forehead slap, and numbers will fall out. But I'm not counting on it.

Added on May 14, 2011

Intense theoretical discussion, involving several people, has been going on at the private Internet website for CMNS researchers. Let me show two interesting generalizations formulated by Ed Storms and John Fisher. They are addressed to those who speculate about mechanisms of nuclear processes responsible for the reported experimental data.

In one of his messages, **Ed Storms wrote:** *We have two entirely different kinds of reactions to explain; fusion and transmutation. The questions is, "Is one or more than one mechanism in play"? Regardless of how many mechanisms are operating, the same consequence is produced, i.e. the barrier is overcome by a process other than brute force and the energy is released in small increments, generally too small to detect.*

I think we have good evidence that fusion between two D takes place and produces the expected ^{4}He. Tritium is a problem because it can not result simply from d-d fusion and produce the observed consequences. Tritium has been produced when deuterium is present but also in H_2O. The evidence suggests both d and p must be present. In both cases, radiation is seldom detected. So, we need a mechanism that addresses these conditions and consequences.

Transmutation has been shown to involve clusters as well as single d or p. In both cases, radioactive isotopes are very seldom produced. In addition, radiation is seldom produced. The rate of the reaction does not seem to be sensitive to the charge on the target nuclei. This means the mechanism hides the charge on the p or d, even to the extent when a cluster contains 6d, but does not affect the charge on the target. When transmutation occurs in Pd+D, the Pd atoms forming the lattice are not the main target. The situation in the hydrogen system, according to Rossi, is different. Does this mean that several mechanisms are operating, which are sensitive to whether d or p is used?

I suggest the first approach is to see if a mechanism can be found that can produce all the observations without making ad hoc assumptions. None of the propose mechanisms can do this although some make that false claim. The erzion and polyneutron ideas do not fit the bill on many levels even though they are clever and address limited observations very well. The deflated electron idea of Horace, although possible as a basic concept, is much too complicated and filled with arbitrary assumptions and vocabulary to be plausible. The BEC ideas have no observational support and have

limited application. If a BEC were found to form in a lattice at RT, this observation alone would warrant a serious prize, regardless of CF. I have addressed the W/L theory before and shown that it is not worth discussing. The idea suggested by Andrew is getting close to what is required, but it also needs to be explained better. So, I hope people who have made theoretical suggestions will take these comments seriously and try to fix the problems so that we can get something to guide experiment.

In one of his messages, **John Fisher wrote:** *The comment by Ed Storms regarding X-rays associated with LENR reminded me again of the beauty and utility of the experiments done by Iwamura and his associates. Their reactor is conceptually simple: a specially prepared palladium foil with deuterium gas on one side and vacuum on the other. The Pd foil is special by having very thin layers of CaO interleaved with Pd layers near the surface facing the gas. With this reactor they have performed a number of critical experiments. They observed excess heat and X-ray emissions when using Pd foils containing CaO, but not when using plain Pd foils. They observed transmutation of target elements deposited on the deuterium-facing surfaces of foils, but there was no transmutation when H was substituted for D or when MgO was substituted for CaO. These experiments raise clear-cut questions for theory:*

(1) How does theory account for the X-rays?

(2) Why is there excess heat with D gas but not with H gas?

(3) What is the role of CaO in this reactor?

(4) Why doesn't MgO support nuclear reaction like CaO?

(5) How does theory explain the observed transmutations?

We theoreticians owe a substantial debt of gratitude to the Iwamura team for raising these challenging questions against which to test our theories.

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400) An Abandoned Line of Research

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I have just reread a ten years old article (1), signed by nine authors. The main authors were probably Cecil and Jones. They loaded deuterium into titanium foils--by placing them into the hot and compressed deuterium gas--and detected energetic charged particles, emitted form foils. Presence of such particles has been reported by other researchers, as summarized in (2). But detectors used by Keeney et al. were more advanced. The authors were able to distinguish different kinds of charged particles, and to measure their energies.

Here is the abstract of their article: *"We present evidence for energetic charged particles emanating from partiallydeuterided titanium foils (TiDx) subjected to non-equilibrium conditions. To scrutinize emerging evidence for low-temperature nuclear reactions, we investigated particle yields employing three independent types of highly-sensitive, segmented particle detectors over a six-year period. One experiment measuring neutron emission from TiDx foils showed a background-subtracted yield of 57 ± 13 counts per hour. (The neutron experiments are discussed in a separate paper in this proceedings.) A second experiment, using a photo-multiplier tube with plastic and glass scintillators and TiDx registered charged particle emissions at 2,171 ± 93 counts/hour, over 400 times the background rate. Moreover, these particles were identified as protons having 2.6 MeV after exiting the TiDx foil array. In a third experiment, coincident charged particles consistent with protons and tritons were observed with high reproducibility in two energy-dispersive ion-implanted detectors located on either side of 25-micron thick Ti foils loaded with deuterium.*

Our overall data therefore strongly suggest low-level nuclear fusion in deuterided metals under these conditions according to the fusion reactions d + d _ n(2.45 MeV) + 3He(0.82 MeV) and d + d _ p(3.02 MeV) + t(1.01 MeV), with possibly other nuclear reactions occurring. Important advances were particle identifications, and repeatability approaching 80% for coincident charged particle emissions. Metal processing and establishing non-equilibrium conditions appear to be important keys to achieving significant nuclear-particle yields and repeatability."

Their detector was a plastic scintillator, mounted of a glass and on the photomultiplier tube (PMT), as shown in Figure 1. That tube was connected to a multichannel analyzer. I am familiar with this method of detection. In fact, my very first original scientific contribution (3) was a thin layer of CsI, evaporated on glass. I wanted to use it in my doctoral dissertation studies. But silicon detectors became commercially available. They offered a better solution to a problem I solved, and I used them instead. My setup, for simultaneous detection of two fission fragments, with two solid state detectors, was also the same as shown in Figure 10. The fragments were emitted from a very thin uranium target and the whole setup was inside a vacuum chamber. What I did not know, however, was that the shape of the electric pulse, produced by the PMT, provides information about the origin of light (plastic scintillator versus glass, as shown in Figure 8).

Note that the labels "Light output" along the horizontal axis (Figure 3 etc.) is most likely the channel number of the analyzer; it is particle energy in arbitrary units. The background was 400 times lower than the signal, according to the abstract. The lines in Figure 2, on the other hand, are not experimental data, except the Am-241 calibration point. They show where the data points were expected. The vertical axis numbers, in Figure 2, should probably be multiplied by the factor of 10, in order to agree with the energy spectrum in Figure 3. (The amplifier gain was probably not the same in each experiment.) It would be more useful if all Light Outputs were expressed in terms of MeV, not in arbitrary units.

The emission of charged particles is apparently influenced by the "non-equilibrium conditions," as stated in the abstract. That refers to the 3 A current flowing through the foils, as illustrated on the right side of Figure 1. Figure 9

shows that the dependence of the rate of emission on time, after the end of loading, was not exponential. The maximum counting rate, at t=100 minutes, was close to one charged particle (mostly a proton with the energy close to 3 MeV) every 1.6 seconds. This amounts to 2200 cnts/hr. The counting rate at t=2 weeks was only 30 cnts/hour.

How reproducible is the shape of the distribution shown in Figure 9? How can such bell-shaped distribution be explained? Unfortunately, no answers to these questions can be found in the article. Even more surprising is the total absence of the follow-up publications; this article was published in 2002, at the 10th International Cold Fusion Conference, Cambridge, MA. How can absence of the follow-up publications be explained?

References

1) Keeney et al. "Charged Particle Emission from Deuterided Metals," in Condensed Matter Nuclear Science: Proceedings of the 10th International Conference on Cold Fusion; World Scientific, 2006, pages 509-523.

2) Edmund Storms, "The Science of Low Energy Nuclear Reaction: a Comprehensive Compilation of Evidence and Explanations bout Cold Fusion."; 2008 (see Table 11, on pages 101-104)

3) Ludwik Kowalski "Thin Layers of Csl(Tl), Obtained by Evaporation Under a Vacuum, as a Detector of Fission Fragments."

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401) A Refutation and a Replication

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Introduction
In closing sections of Chapter 4 (1), Ed Storms focuses on various aspects of replication experiments. Let me address some of these issues, in the context of two projects: our Curie Project and SPAWAR project.

Curie Project
Our purpose (2) was not to either accept or reject Oriani's claim; it was to either confirm or not confirm it. The so called "burden of proof," as always, is on the author of a claim. The burden of rejection, on the other hand, is on all of us, not on authors of individual reports. I agree with Storms that "deciding whether sufficient replication has been achieved to justify accepting the effect as real phenomena is a complex problem." That is why a decision about accepting or not accepting belongs to the entire scientific community, in any given discipline. Unfortunately, a large fraction of nuclear scientists have already decided that all CMNS claims are pseudoscientific. That conclusion, accepted in the early 1990's, prevents constructive evaluation of new experimental data.

As stated in the Introduction, our goal was to either confirm or refute the claimed reproducibility of the Oriani effect. According to Storms, "repetitive failure [to replicate results] generally means the parameter space in which critical conditions fall is too small to be entered very often by accident using the available methods." This is certainly a possibility; our individual conditions were not exactly the same as in Oriani's lab. What exactly was responsible for our failure to replicate the originally reported results? That question can be answered, at least in principle, via a very complex investigation. I am thinking about changing places, for example, Oriani performing experiments in our laboratories using distilled water and us performing experiments in his laboratory, using tap water. I am also thinking about the use of teflon by Richard, about the use of spot-welded contacts by each of us, etc. A systematic investigation of this type would be practically impossible, under our working conditions.

SPAWAR project
The idea of organizing a collective attempt to replicate Oriani's results, The Curie Project, was a logical consequence of my participation in The Galileo Project, an attempt to confirm the effect discovered by SPAWAR team. Like other participants I observed the same copious pits as those observed by scientists who discovered them. After confirming their experimental results I addressed the issue of interpretation. Are the observed pits due to alpha particles, as claimed by SPAWAR scientists, or are they due to something else, as suspected by Richard Oriani? My answer to this question, based on measured sizes of copious pits, was that they cannot be due to common alpha particles, as described in (3). This conclusion was rebutted in (4). The issue the interpretation of reproducible results remains unresolved (5).

Unfortunately, this line of research--investigation of copious pits, on both CR-39 surfaces--was apparently abandoned in favor of the investigation of rare triple tracks (6), presumably due to fast neutrons. Why unfortunately? Because spectacular results whose reproducibility has been independently confirmed are rare. Such results should be studied, not abandoned. The first task should be to identify parameters, or combinations of parameters on which the effect depends or does not depend, as described in (1). For example, what happens when the magnetic field is either increased or decreased, when different cathodes, codeposition rates and laser illuminations are used, when ... etc. etc. Changing one parameter at a time, and publishing the results, preferably in different journals, would be highly desirable.

Conclusion

As stated in (7), each of us "is a citizen of the community of science. Each shares responsibility for the welfare of this community," and for the society at large. Attempting to objectively confirm or refute various scientific claims is our professional obligation.

References:

1) Edmund Storms, "The Science of Low Energy Nuclear Reaction: a Comprehensive Compilation of Evidence and Explanations about Cold Fusion.," 2008 (see page 117)

2) Jeff Driscoll et al. "Issues Related to Reproducibility in a CMNS Experiment," "Journal of Condensed Matter Nuclear Science," vol. 5, June 2011, page 34-41. (it can be downloaded from http://www.iscmns.org/CMNS/JCMNS-Vol5.pdf)

3) Ludwik Kowalski, Comment on "The use of CR-39 in Pd/D co-deposition experiments," Eur. Phys. J. Appl. Phys. 44, 287–290 (2008). The article can be downloaded from the library at http://lenr-canr.org

4) Mosier-Boss, P.A., et al., "Reply to Comment on 'The Use of CR-39 in Pd/D Co-deposition Experiments': A Response to Kowalski." Eur. Phys. J. Appl. Phys., 2008. 44: p. 291-295. The article can be downloaded from the library at http://lenr-canr.org

5) Matt McConnell et al. at: http://lenr-canr.org/acrobat/CantwellRsearchforc.pdf

6) Mosier-Boss, P.A., et al., "Triple tracks in CR-39 as the result of Pd/D Co-deposition: evidence of energetic neutrons," Naturwiss., 2008. doi:10.1007/s00114-008-0449-x(96): p. 135-142.

7) APS Guidelines For Professional Conduct at: http://www.aps.org/policy/statements/02_2.cfm

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Nuclear transmutation of radioactive isotopes and reactor waste deactivation in biological systems

Prof. Vladimir Vysotskii
Kiev National Shevchenko University,
Kiev, Ukraine

Dr. Alla Kornilova
Moscow State University,
Moscow, Russia

28 August 2011

Experimental investigation of fusion of iron-region stable isotopes in "one-line" growing microbiological cultures (1994-2000)

A typical series of experiments concerning nuclear transmutation of elements consisted in growing of microbiological culture in 3 disks

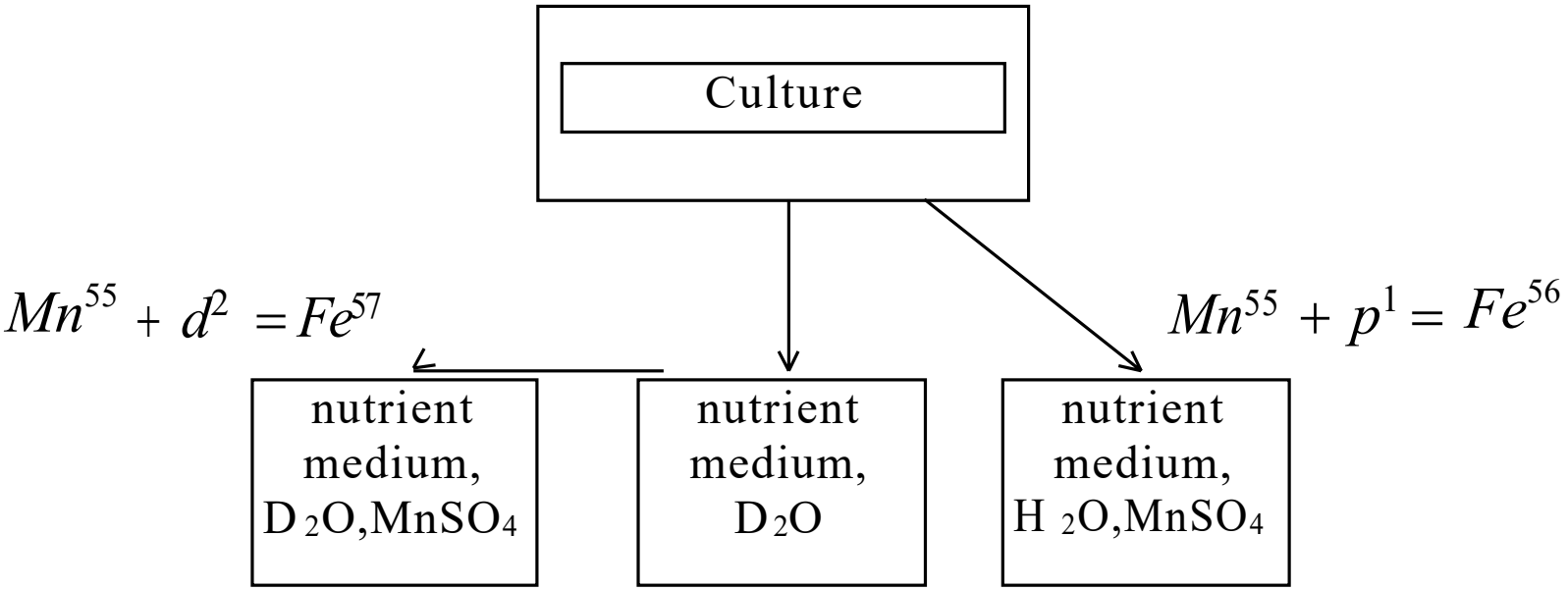
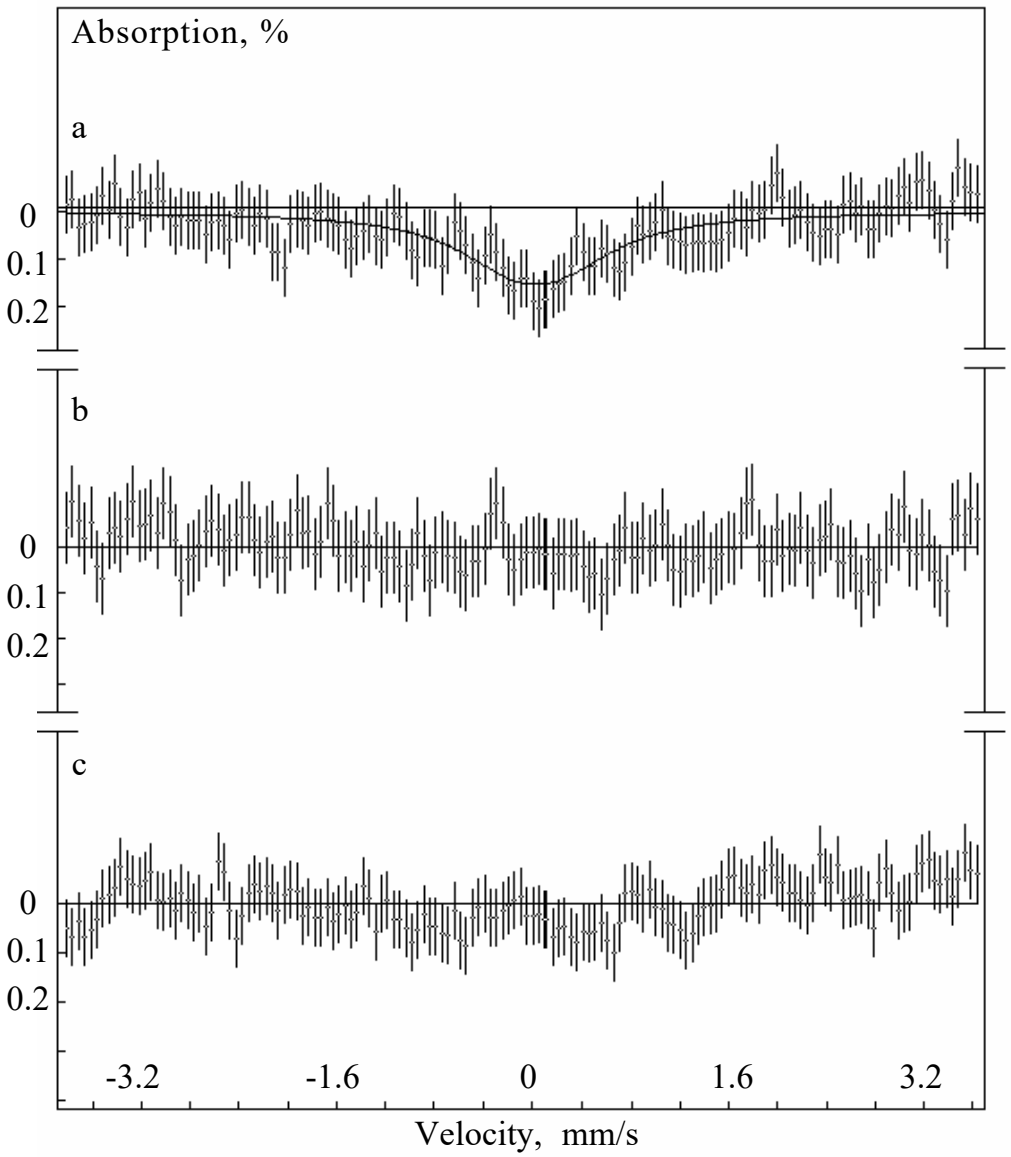


Fig.1 The scheme of experiment.

Such series of experiments was held for different cultures, different time of growth Δt (24, 48 and 72 hours) and different growth modes (in still disks and media and in suspension stirring mode using magnet stirring device).

Bacteria and yeast were grown in a thermostat at optimal temperature 32 C.



Mossbauer investigation of isotope transmutation

It was shown that the transmutation process during the growth of such microbiological cultures had taken place, but its effectiveness had been low:

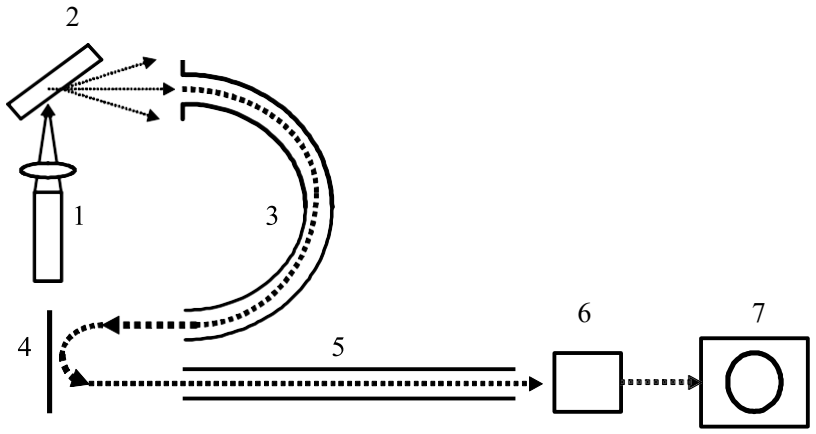
$$t = \frac{\Delta N(Fe^{57})}{N(Mn^{55})\Delta t} \approx 10^{-8}$$

synthesized Fe^{57} nuclei per s and per single Mn^{55} isotope

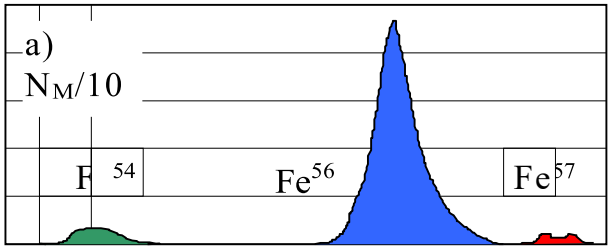
The Mossbauer specter for the grown culture S*accharomyces cerevisiae T-8:* *a*) in D_2O with Mn^{55}; *b*) in H_2O with Mn^{55}; *c*) in D_2O without Mn^{55}

Studying of a transmutation of light and intermediate isotopes in growing microbiological culture by laser time-of-flight mass spectrometer

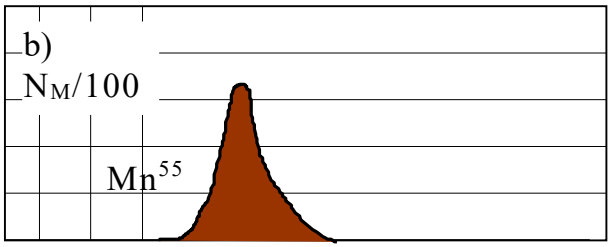
$$Mn^{55} + d^{2} = Fe^{57}$$



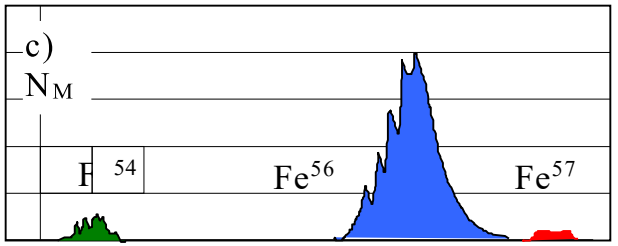
Laser time-of-flight mass-spectrometer



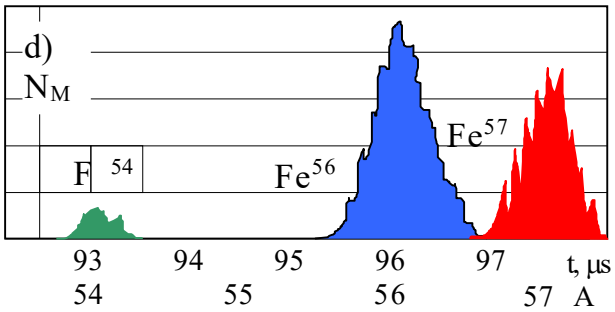
Nature Fe



Nature Mn



Culture grown in D_2O without Mn



Culture grown in D_2O with Mn

Fusion of iron-region stable isotopes in optimal growing microbiological associations (2000-2006)

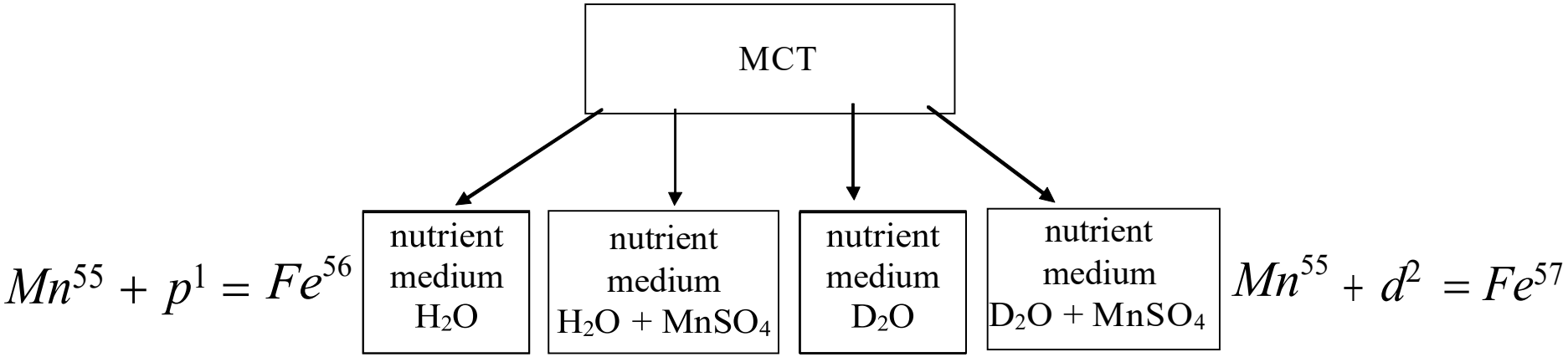
The relatively low efficiency of these reactions is the result of the relative narrow interval of optimal functional individual characteristics for supporting of nuclear activity in any "one-line" type of culture. During the growth of a "one-line" culture, we hypothesize that processes involving forms of auto-intoxication of nutrient media by metabolic products take place.

In a contrast to these "one-line" cultures, we have investigated microbe syntrophin associations that include great numbers of types of different cultures that are in the state of complete symbiosis.

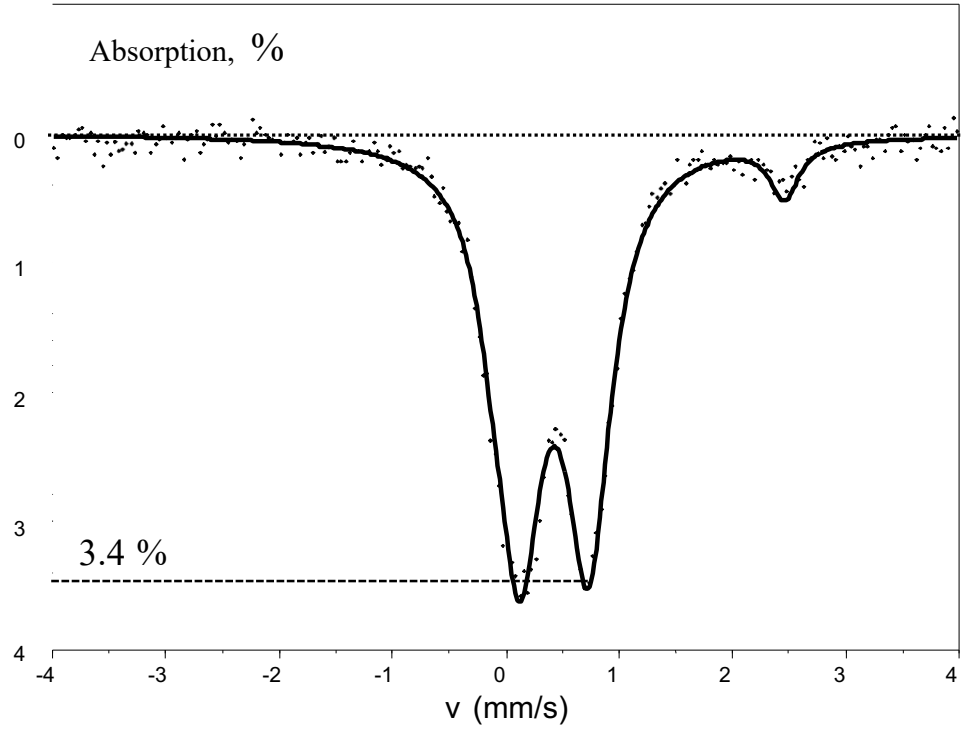
The ***MCT*** (microbial catalyst-transmutator) compound is the special granules that include:

1. concentrated biomass of metabolically active microorganisms (microbe syntrophin associations);
2. sources of carbon and energy, phosphorus, nitrogen, etc.;
3. gluing substances.

Investigation of nuclear reaction $Mn^{55} + d^{2} = Fe^{57}$ with MCT

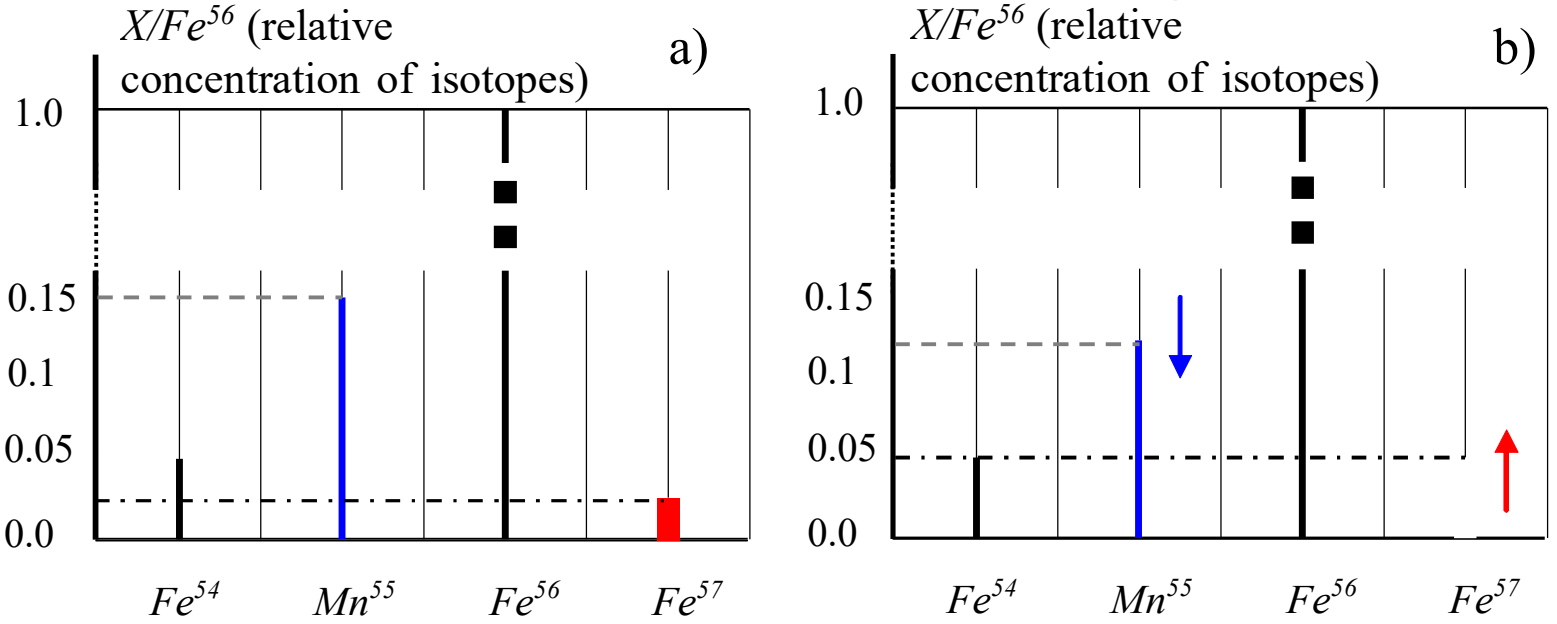


In these experiments the very large increasing of transmutation efficiency (by 100 times) was observed!



The Mossbauer specter for **growing microbiological associations** in D_2O with Mn55

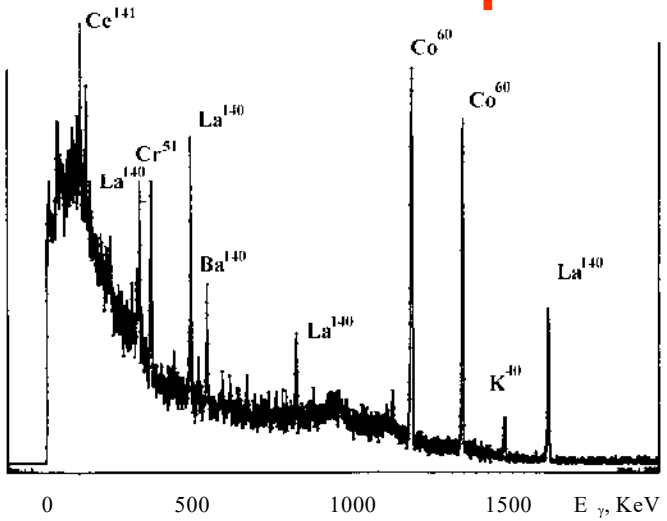
For verification of these results, additional examinations of the isotopic ratio of the same dried biological substances (both control and transmutated) were conducted by TIMS (**Thermal Ion Mass Spectroscopy**, «Finnigan» MAT-262.
The results of TIMS measurements presented in Figure



$$Mn^{55} + d^2 = Fe^{57}$$

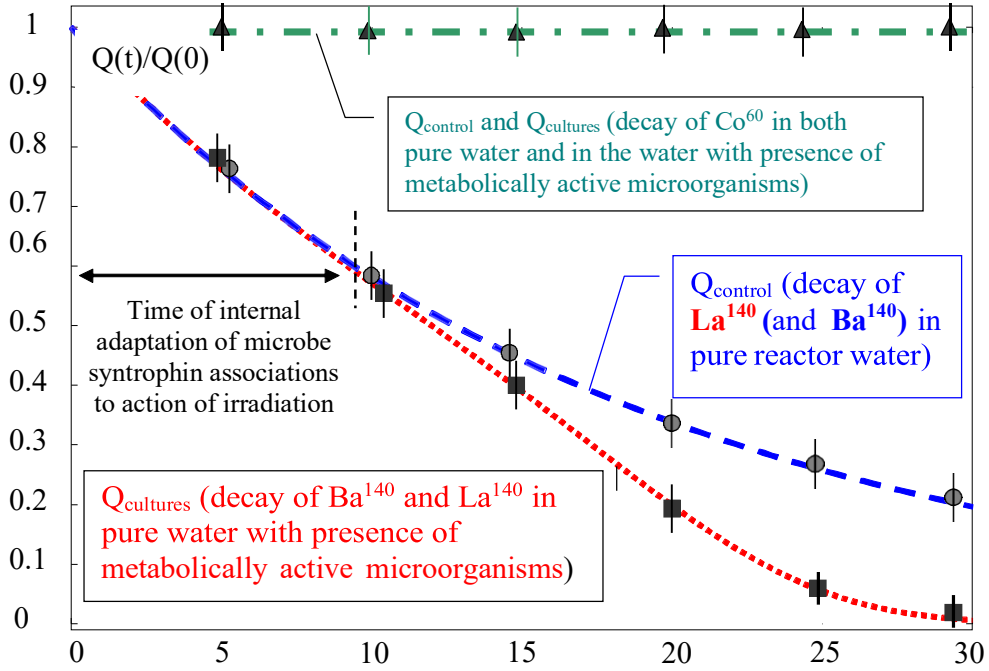
Mass-spectrum of iron-region of microbiological associations (dried biological substances) that were grown in control nutrient medium with H_2O and Mn^{55} (case a)) and in experimental nutrient medium with D_2O and the same quantity of Mn^{55} isotope (case b)) .Here $X=Fe^{54};Mn^{55};Fe^{57}$ ***The process of increasing (−) of concentration of Fe^{57} isotope is accompanied by decreasing (−) of concentration of Mn^{55} isotope***

Experiments on controlled decontamination of active isotopes in biological cells (2003-2010)



a) Deactivation of reactor water in biological cells

Spectrum of gamma-radiation of distilled water from first contour of water-water atomic reactor of Kiev Institute for Nuclear Research (10th day after extraction from the active zone).



t, days after extraction of water from the active zone of nuclear reactor

Change of activity Q(t) of the same reactor Ba^{140}, La^{140} and Co^{60} isotopes in the experiment on transmutation (activity $Q_{cultures}$ in pure reactor water with presence of metabolically active microorganisms) and in the control one (activity $Q_{control}$ in the same pure reactor water without microorganisms)

Studied La-140 isotope has short life-time 40.3 hours and is nonstable daughter isotope of Ba-140 radioactive isotope that has life-time about τ_{Ba140}= 12.7 days: Ba-140 $\rightarrow$ La-140 + β- + ν^* $\rightarrow$ Ce-140 (stable) + β- + $\tilde{\nu}$

Initial activities of the Ba-140 and La-140 isotopes (on the 10th day after extraction of water from the active zone of the nuclear reactor) were

$$Q_{Ba-140} = 1.46.10^{-6} Curie / l$$

$$Q_{La-140} = 2.31.10^{-7} Curie / l$$

The possible way of radioactive Ba^{140} isotope transmutation to the stable state is

$Ba^{140} + C^{12} = Sm^{152} + \Delta E$

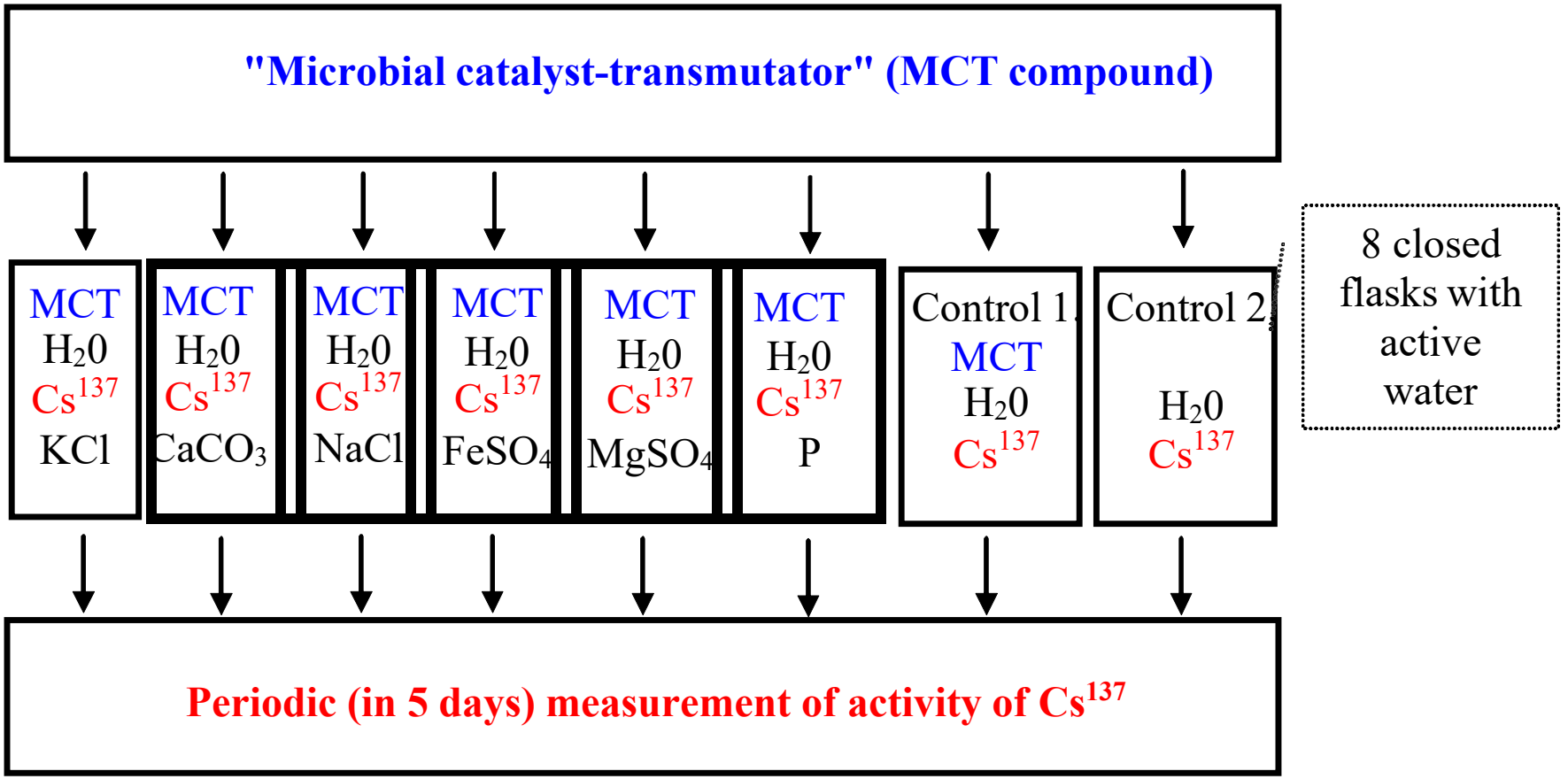
These reactions are energy favourable and

$\Delta E = E(A_{Ba}, Z_{Ba}) + E(A_C, Z_C) - E(A_{Sm}, Z_{Sm}) = 8.5$ MeV is positive.

The Sm(2+) and Ca(2+) ions are chemically alike and have the approximately same ionic radiuses of divalent state (R_{Sm} » 1.2 A, R_{Ca} » 1.06 A). Substituted element Ca is among several vitally necessary elements. Ions of created Sm(2+) elements can substitute Ca(2+) ions while microbiological cultures are growing.

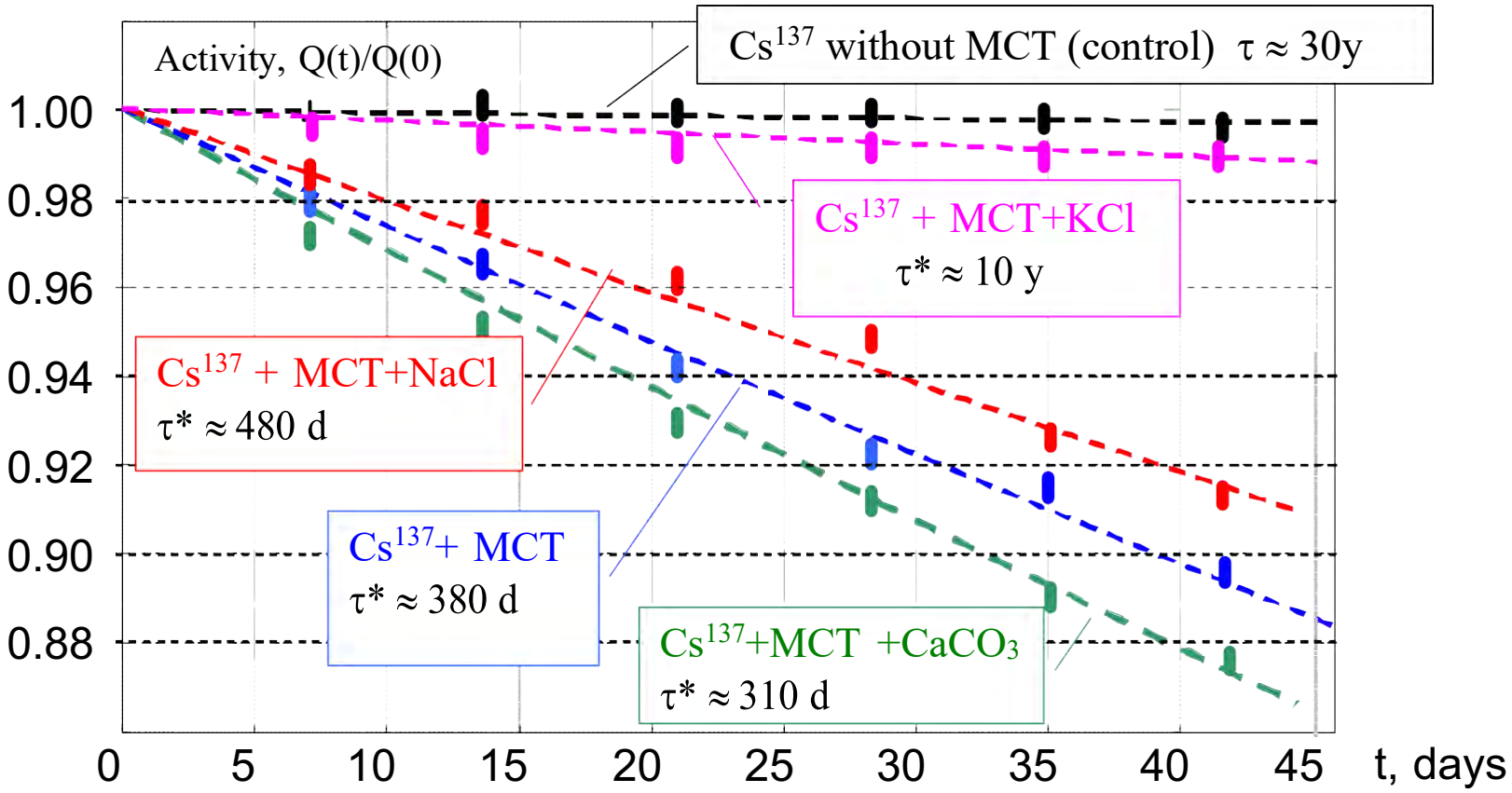
b) Deactivation of reactor Cs^{137} isotope in biological cells

The research has been carried out on the basis of the same distilled water that contained long-lived reactor isotope Cs^{137} (activity $\approx 2.10^4$ bq), In our experiments 8 identical closed glass flasks with very thin walls and with 10 ml of the same active water in each were used. The MCT was placed in 7 glass flasks.



Study of utilization of active isotopes at different conditions

Results of experiments on accelerated deactivation (decontamination) of Cs137 isotope in "hot" water



During 100 days we have observed speeded up decay of Cs^{137} isotope (by transmutation to different stable isotope) in all experiments with MCT and with the presence of different additional salts.

The most speeded up decay with t* » 310 days (accelerated by 35 times) was observed at the presence of Ca salt - Cs^{137}+MCT +$CaCO_3$.

Deactivation of different active isotopes in most optimal experiment

(MCT + active water with presence of Cs^{137}+ $CaCO_3$ salt)

		Start of experiments	Intermediate finish (100 d)			
Isotope	Energy, keV	N_1, registered events per 10^3 s	N_2, registered events per 10^3 s	Error (absolute/ relative)	Natural decay per 100 d	Change $(N_2-N_1)/N_2$
Cs^{137}	661.7	266900	216800	±478(±0.2%)	-0.6 %	-24 %

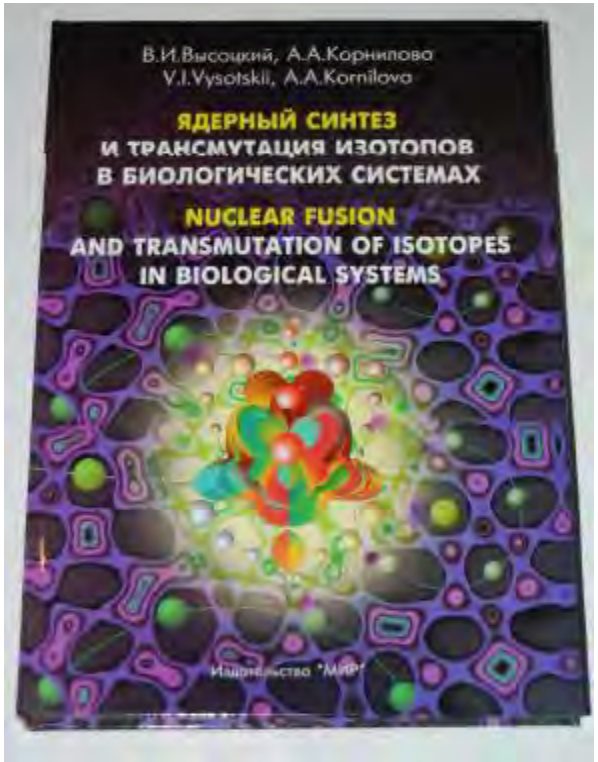
The possible reaction of Cs^{137} isotope utilization is $Cs^{137} + p^1 = Ba^{138}$ (stable) + ΔE. The result of this reaction is the creation of stable Ba^{138} isotope. This reaction is energy favourable (ΔE = 5.58 MeV is positive).

The Ba^{2+} and K^+ ions are chemically alike and have the approximately same ionic radiuses of divalent state (R_{Ba} » 1.4 A, R_K » 1.33 A). Substituted element K is among several vitally necessary elements. Ions of created Ba^{2+} elements can substitute K^+ ions in metabolic process while microbiological cultures are growing.

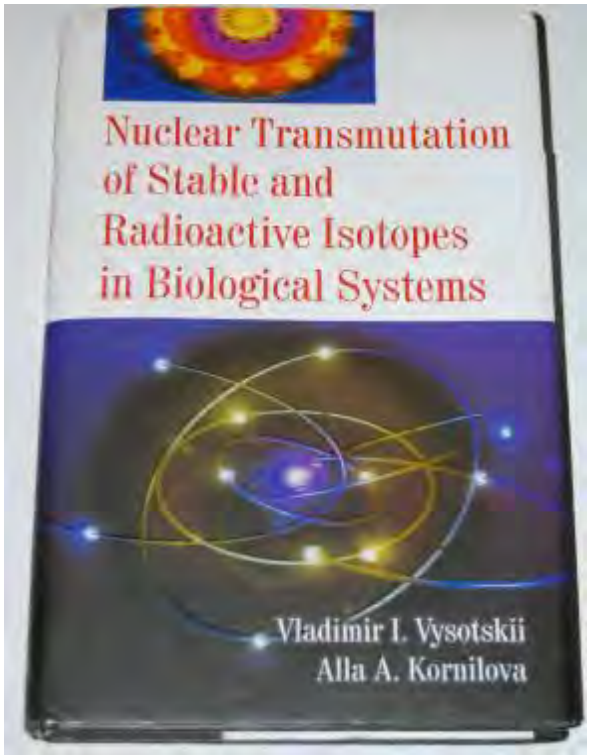
The presented results show perspectives of use of the effect of stable and radioactive isotopes transmutation in biological systems for natural and industrial applications.

These results can give the answer to the question of the reasons of abnormal accelerated decrease of environmental radioactivity in some isolated areas inside Chernobyl NPS accident zone and after bombing of Hiroshima and Nagasaki with initial high level of radiation pollution.

Biophysical reasons and possible physical mechanisms of isotope transmutation in biological systems are described in our books:



Vysotskii V.I., Kornilova A.A. Nuclear fusion and transmutation of isotopes in biological systems, Moscow, "MIR" Publishing House, 2003



Vysotskii V.I., Kornilova A.A. Nuclear transmutation of stable and radioactive isotopes in biological systems, Pentagon Press, India, 2009.

COLD FUSION IS NOT VOODOO SCIENCE

Ludwik Kowalski (about the author)

Montclair State University, USA, March 2012

Table of Contents (for this item only)

This website contains **other cold fusion items.**
Click to see the list of links to other items

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1) Introduction

As a retiree I participate in a memoir-writing workshop for senior citizens, directed by Lucile Lichtblau. About eight of us meet each month to read and discuss our compositions. My first two memoirs were based on WWII events; the third was about Cold Fusion (CF). What is CF? I am not ready to answer this question at this point; the answer will emerge from subsequent chapters. For the time being let me say that CF is a highly controversial field of physical science research. It is also known as Condensed Matter Nuclear Science (CMNS), and Low Energy Nuclear Reactions (LENR). These acronyms might be useful to those who are impatient and want to start Googling for answers immediately. These three names will be used interchangeably below.

The controversy started in March 1989, when two university professors in Salt Lake City announced a totally unexpected discovery. Some people think that this was the greatest fiasco of the last century; others believe that this discovery was an important step toward future technology of pollution-free nuclear energy. My short memoir was read at one of our monthly meetings. But it was not well received. Most participants were confused by technical terms. I was advised to focus the essay on personal experience, rather than on science. That is what I did. But what started as a short essay turned into a book.

Seeking a model of clear writing about the topic, I consulted the book "Cold Fusion And The Future." The author Jed Rothwell is a friend. He wrote: *"many nightmare problems that seem beyond any present solution, such as global warming, invasive species, and providing clean drinking water and sanitation to billions of poor people, may be remedied with cold fusion combined with other technologies. The future might be better than you think."* Jed is not a professional scientist. But he knows enough science to describe it clearly to lay people. His book is freely available online.

Yes, abundant and pollution-free energy would make life on earth better for billions of people, especially in underdeveloped countries. Unfortunately, the world is still waiting for a reproducible-on-demand demonstration of a CMNS effect. The essence of the CF controversy is whether or not a chemical process can trigger a nuclear reaction. Most scientists think that this is impossible. But a small fraction of them, perhaps 100 people worldwide, including myself, continue to conduct experiments whose purpose is to investigate LENR effects.

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2) What Is Cold Fusion?

The best way to start explaining CMNS is to refer to so-called "hot fusion," a process in which two atomic nuclei of hydrogen fuse at temperatures exceeding several million degrees. This process generates thermal energy (heat) in hydrogen bombs, and in stars. In the last five decades numerous attempts have been made to turn a hydrogen bomb explosion into a "slowly burning" controllable process. This line of technological research, costing tens of billions of dollars, has not yet produced anything of practical use.

Fusion of atomic nuclei has been studied by physicists since 1930s. We know that such fusion is only possible at extremely high temperatures. Its probability at ordinary temperatures-- that is below ten thousand degrees or so--is practically zero, due to mutual electric repulsion of atomic nuclei.

That is why physicists were so surprised, in 1989, when two chemists, Fleischmann and Pons (F&P), announced the discovery of cold fusion, presumably similar to hot fusion but taking place at room temperature. The announcement made at the University of Utah press conference created a lot of excitement. The cover page of the Business Week magazine was "Miracle or Mistake: Fusion in a Bottle." Similarly, Times magazine's front page question was "Fusion or Illusion?" Newsweek's front page was also devoted to cold fusion; the title was "The Race for Fusion." F&P had no evidence that measured heat was due to a nuclear process; that was only their assumption.

At that time I participated in a research project at Brookhaven National Laboratory. My work had nothing to do with nuclear fusion. But we debated the F&P's discovery constantly. A chart on a wall was updated each morning, showing how many teams of scientists, worldwide, confirmed the announced discovery and how many reported negative results. Similar debates were taking place in other labs, as I learned later. Most of them were focused on the "theoretical impossibility" of CF, rather than on possible experimental errors. Needless to say, I followed the debates with great interest. But, like most scientists, I came to the conclusion (in 1992) that the F&P claim was not justified.

My renewed interest in CMNS was rather coincidental. In the summer of 2002, I went to a scientific conference in Albuquerque, NM, to hear what nuclear scientists had to say about new ways of dealing with radioactive waste produced in existing nuclear reactors. My wife joined me after the conference. We rented a car and had a wonderful week in New Mexico. Naturally, we stayed in Santa Fe--how could one skip this wonderful place. We also visited the WWII museum in Los Alamos, and Alamogordo, site of the first nuclear explosion, several months before Hiroshima

and Nagasaki. But the conference had a most remarkable and totally unexpected consequence for me--the renewal of interest in cold fusion (CF).

Why unexpected? Because the conference was not about CF. Several reports, however, were devoted to CF topics. I listened to them very carefully and talked with scientists who described new results. I was very impressed by their credentials, and by the fact that they were debating experiments, not interpretations. That is why I decided to get re-acquainted with developments in the field, 13 years after the controversy started. The purpose of this book is to describe my CMNS-related activities since the Albuquerque conference. I have met many interesting scientists, attended four international CF conferences, participated in several research projects and published several papers in that field. This has been chronologically recorded, more or less regularly, at a dedicated website:

http://pages.csam.montclair.edu/~kowalski/cf/

Some items at this website are more technical than others. But most of them can be comprehended by people who studied physics and chemistry in high school.The readers of this book are also expected to be familiar with elementary science.

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3) Meeting a Russian Scientist, Alexander Karabut

The scientist who impressed me the most in Albuquerque was Alexander Borisovich Karabut, from Russia. F&P, as mentioned in Chapter 2, had no evidence that their excess heat was due to a nuclear process. They suspected that excess heat was nuclear because it was too large to be due to a known chemical reaction. Karabut, and his team, by contrast, reported not only excess heat, generated at the rate of about 9 watts, for 120 hours, but also the presence of several nuclear effects. This was a revelation to me.

The team spent ten years studying nuclear processes associated with generation of excess heat at ordinary temperarures. His talk at Albuquerque was the summary of findings; some of them had been reported as early as 1990. As a Russian speaker I was able to help the author improve his presentation in English, a language in which he is far from fluent. We talked about his paper before it was formally presented, and we discussed it afterwards. What I heard in Russian was much clearer than in his English text. The link to my translation is:

http://csam.montclair.edu/~kowalski/cf/13karabut.html

One of the effects, reportedly observed by Russians, was emission of high-energy alpha particles from the palladium foil saturated with heavy hydrogen. This effect alone, if independently confirmed by other scientists, would be sufficient to validate the idea that a nuclear process can be triggered by a chemical process at low temperature. Why had no one tried to replicate experiments described by Karabut? That question still puzzles me. Whose moral obligation was to it to verify such extraordinary results? The most obvious people were other CMNS experimentalists. But each of them worked on his or her own project, usually without any financial support. Furthermore, confirming a discovery made by someone else is not as rewarding as being recognized as the discoverer of something unknown and important.

I can only imagine how Karabut's discoveries would have been treated in Stalin's USSR. The Academy of Sciences would at once have organized several replications. Confirmation of results would turn Karabut into a famous scientist. Rebuttal of the results, on the other hand, would have led to immediate disqualification, or much worse. But that is not what happed in post-USSR Russia. The scientific establishment, associated with the Academy of Sciences, declared Karabut a pseudo-scientist. This was not based on new experimental results; it was based on theoretical grounds--the reported facts conflicted with the already-accepted theory of hot fusion reactions.

The main accuser, according to Karabut, was the Academician E.P. Kruglakov, the author of "The Highwaymen of

Science." This book, published in 2001, is indeed very interesting; Karabut sent it to me, after returning to Moscow. The Russian scientific establishment, according to Dr. Karabut, considers cold fusion to be voodoo science. In fact, Dr. Kruglakov heads the "Commission to Oppose Pseudo-Science and Falsifications in Scientific Research." I had no idea that such a commission had been created by Russian Academy of Sciences. The book ranks cold fusion at the same level as N rays (a well known case of either fraud or self-delusion in France), astrology, extrasensory perception, and magic. Karabut hinted that the antagonism against CMNS in Russia has more to do with the competition for very limited financial support than with objectivity.

I can also imagine how our own government would have reacted to CMNS discoveries published by Russian scientists during the Cold War. The DOE (Department of Energy) would have quickly organized several replications, in order not to be left behind. Successful replications would probably have been classified and additional research would have been sponsored by the DOE, at various laboratories. Refutations, on the other hand, would provide evidence that results reported by Karabut should not be taken seriously. It is interesting that a book similar to Kruglakov's was published in the US: "Voodoo Science: The Road From Foolishness to Fraud" by a physicist, Dr. Robert Park.

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4) Meeting George Miley

Dr. George H. Miley, a chemical and nuclear engineering professor from the University of Illinois, was another scientist I met at Albuquerque. His conference paper made me aware of how much I had missed since I had stopped paying attention to the CMNS field. A very impressive summary of Miley's professional accomplishments can be found in Wikipedia. Before 1989 he was a hot fusion researcher; afterward he became a CF researcher as well. In 1990 he published a paper, in cooperation with another researcher, about production of chemical elements in thin layers of metallic films saturated with hydrogen. They wrote: " the Ni film [removed from our experimental cell] was found to contain Fe, Ag, Cu, Mg and Cr." Concentration of these elements, after the experiment, was found to be much higher than before. If confirmed this would be undeniable evidence for nuclear reactions taking place at low temperatures.

But the most interesting Miley paper, as far as I am concerned, was published in 2002, the year I met him. The title was "Some Personal Reflections on Scientific Ethics and the Cold Fusion 'Episode' ". Unfortunately, he did not tell me about this paper in Albuquerque. I read it several years later; it is now available online. The link is:

http://www.lenr-canr.org/acrobat/MileyGHsomeperson.pdf

Most papers written by CMNS researchers are devoted to scientific and technical topics, as one can verify by going to the online library at

www.lenr-canr.org

That library, by the way, was created by Jed Rothwell. George Miley's long paper is devoted to political aspects of the controversy. Here is a brief summary. My encounter with CF, he wrote, *"was at the initial congressional hearing in Washington D.C. on the topic. I was selected to provide input from a fusion researcher known for innovative research who might comment on CF from a 'neutral position'. Thus, I was 'squeezed' into the testimony order between the originators of the field, Pons and Fleischmann, and a strong opponent of CF, Harold Furth, the then director of the Princeton Plasma Physics* [hot fusion] *Laboratory."*

In his talk Miley speculated about possible future CF developments. After the hearing, he said that *"a CIA agent caught me in the hall and warned that someone like myself with a 'Q clearance' should not publicly air such sensitive speculation. As it turns out, my speculation had some validity*. Describing other 1989 meetings, Miley wrote that *"almost 'carnival atmosphere' was created by the combination of reporters, entrepreneurs, garage inventors, curious on-lookers, politicians, financial brokers, and scientists at the initial Los Alamos National Laboratory [...]*

Then there was the 'famous' NSF-EPRI meeting in Washington DC where the NSF ended up withdrawing 'official' sponsorship at the last moment due to the swing in opinion against CF. Despite this controversy, Edward Teller [known as the father of the first hydrogen bomb] attended this meeting in a wheel chair (due to a recent operation) and provided a guiding example of an open scientific mind by freely entering the discussion. Instead of ruling CF out due to lack of theoretical explanation, he suggested that a new particle, dubbed "meshuganon," would be needed (and might actually exist) to explain the observations reported by Pons and Fleischmann. [...].

Looking back, he continues, *"the CF field has caused grief for many key persons who became 'too' strongly involved. Pons and Fleischmann left the US for France [...]; the President of the University of Utah was forced to resign as a result of issues raised about CF funding procedures [...], John Bockris at Texas A&M, was bombarded with University-appointed investigating committees and, as a 'crowning blow' was forced off-campus with the second International Meeting on Low Energy Nuclear Reactions that he hosted. Gene Mallove found it necessary to step down from his scientific information post at MIT following publication of his book, Fire from Ice. Peter Hagelstein faced a hostile promotion committee at MIT after his early theoretical work on CF; [...] Why should such intense controversy and drastic personal repercussions develop over a scientific field?*

Certainly the unconventional manner in which Pons and Fleischmann introduced CF by announcing it to the press initiated the controversy which eventually polarized the field into camps of 'believers' and 'non-believers'. The fundamental reason behind this emotional approach to CF was, in my view, the tremendous impact that CF, if proven true, could have. Consequently, the vast amount of money and the prestige at stake brought out the 'best' and the 'worst' in people. [...]

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5) Beginning Of Censorship: Also From Miley's Paper

The paper from which I am quoting is worth reading in its entirety. It is a rather unique testimony from an open-minded researcher and editor, caught in the middle of the 1989 CF controversy. In one section he writes: " *Another criticism of my editorial policy on CF has been that since I have done research on the topic, I must be biased in favor of it. It's true that I have had papers in most ICCF meetings, starting from the original LANL meeting in Santa Fe. This criticism, in my view, amounts to a double standard. My initial selection as FT's* [FusionTechnology] *editor, and the other two journals, was based on my recognized research on fusion, lasers, and plasma physics.*

This track record was assumed to provide me with better insight into the technical content of the papers, and allow me to select top reviewers. In universities, teaching and research are well recognized as reinforcing each other. The same is certainly true for editing and research. Why wouldn't the same be true for CF? [...]

In conclusion, the issue of whether my FT position, as opposed to Nature's closed-door policy, is proper for a scientific journal must be left to the reader. The question to be answered, in my opinion, is which policy will advance science best in the long run? To rephrase the question, we might ask if the publications in FT have communicated new scientific information or have they mislead readers? [...]"

In March of 2012, George Miley was again at the center of the CMNS controversy. This time he appeared as a designer of something very practical--a CF nuclear battery. Chemical batteries do not last long enough to power electrical equipment in a spaceship. That is why nuclear batteries, containing highly radioactive plutonium, have been used in long-lasting NASA missions. Miley is developing a battery based on a CF effect. Such battery, using non-radioactiv and less expensive hydrogen and palladium instead of plutonium, would indeed be highly desirable. A brief announcement of his invention can be found at:

http://nextbigfuture.com/2012/03/george-miley-upcoming-presentation-game.htm

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6) Theories Guide But Experiments Decide

Scientific methodology of validation of claims, a set of rules developed to deal with difficulties, mistakes and controversies is well known. Most scientific mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other scientists. Our results, if valid, wrote one scientist, John Huizenga, must be reproducible on demand. *"When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press."* The scientific process, in other words, is self-corrective. The process might be slow but it works, more often than not.

Why is the CMNS controversy unresolved since its beginning in 1989? Because the claims are still not reproducible on demand, and because experimental results conflict with the accepted theory of nuclear fusion. A theory, in this context, is not just a hypothesis, it is a logical structure that is known to agree with a wide range of already verified experimental data. Scientists know the rule--theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on additional verifications of new experimental results.

Referring to such situations, Huizenga wrote: "There are occasionally surprises in science and one must be prepared for them." Theories are not carved in stone; scientists do not hesitate to modify or reject theories when necessary. Rejecting a highly reproducible experimental result "on theoretical grounds," which is quite common, is not consistent with scientific methodology. But that is exactly what often happens when CMNS claims are criticized.

John Huizenga, one of recognised leaders of the field known as Nuclear Chemistry, was a senior colleague, when I was a post-doctoral researcher at Columbia University. He often visited us and I had the priviledge of discussing topics of common interest with him. His book about CMNS, "Cold Fusion: The Scientific Fiasco of the Century," persuaded me that F&P claims were not valid. This was nearly ten years before the 2002 Albuquerque conference.

My comments on the process of scientific development in general, and on the CMNS field in particular, can be found in Chapter 23.

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7) First US Government_Investigation

The significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by “vested interests” of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation. A panel of scientists, named ERAB (Energy Research Advisory Board), and headed by John Huizenga, was formed to investigate CF in 1989. The final report, submitted to the DOE several months later, interfered with the normal development of the field.

I was later disappointed to learn that ERAB scientists investigating the CF claims were not personally involved in replications of experiments. Conclusions and recommendations from their report, based on visits to several laboratories rather than participation in experiments, are summarized in a paper I published recently

http://www.ptep-online.com/index_files/2012/PP-29-L2.PDF

Only one of their six conclusions refered to CF experiments; the remaining five conclusions were about anticipated practical uses of CF, and about various aspects of the suggested interpretation of results. Instead of focusing on reality of excess heat, critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion. The same observation can be made about the six ERAB recommendations. Only one of them referred

to possible experimental mistakes. It is clear that the ERAB observations were based mostly on ”theoretical grounds,” and not on identified laboratory mistakes. Support for CF research in the US practically stopped in 1989.

Another result of the first DOE investigation, as described by Miley, was that editors of some scientific journals started rejecting manuscripts writen by CF scientists, bypassing peer review. This kind of discrimination, directed against PhD-level scientists, is totally inconsistent with scientific norms. Illustrations of such descrimination are to be found in chapters 17 and 18.

Inserted on 5/24/2012

Here is message about CF that I posted last night at our university forum.

"As shown in < http://www.ptep-online.com/index_files/2012/PP-29-L2.PDF> , the first two DOE investigations of CF claims failed to focus on what was the most important--promotion of experiments designed to verify reality of excess heat. The DOE refuted F&P claims on the “theoretical basis,” that is on the conflict between the reported fact and the hot fusion theory.

What would I do if I were appointed to organize the first DOE investigation, in 1989? Familiar with Fleischmann’s undeniable reputation I would offer him a generous research grant, for two or three years, to continue CF research. I do not think that F&P’s motivation was anything else but desire to be recognized as discoverers of something new and important. They would probably welcome the grant and cooperated with the government. Unwillingness to cooperate (in order to protect secrets, pending patent applications, etc.) would show that the claim is commercial and does not justify the grant."

Knowing that the government grant was accepted, I would find two knowledgeable electrochemists and made them responsible for cooperation with F@P. The purpose of cooperation would be to help them to clearly describe the protocol, and to publish it in a peer reviewed journal. A year or two later (if the controversy were still not resolved via the normal self-correcting process) I would select ten other reputable electrochemists (Energy Research Advisory Board), working in government laboratories, and asked them to replicate the F&P experiment, using the published protocol. The formal report, prepared by ERAB chairman, would be mostly based on available experimental data, not on the fact that results conflict with our model of thermo-nuclear reactions.

P.S. Yes, I know that no one will ask me for something like this. It is only a speculation, based on past mistakes."

Responding on the forum a prominant CF researcher wrote:

"Of course Ludwik, any one who was interested in benefitting from LENR would have treated the situation the way you describe. The people at the DOE are not stupid or ignorant. They know how to do a proper review. The fact that the DOE did not act this way reveals that they were not interested in finding out the truth. They wanted the possibility of LENR being real to be eliminated. This phenomenon is a threat to all that the DOE values. When the DOE wants a program to be supported, they work very hard and ruthlessly to see that this happens. They did just the opposite with respect to LENR. This was a result of well considered policy. That is why an attempt to contact Sec. Chu or anyone in the DOE who has authority will not change their approach. They want LENR to fail for obvious reasons. They just don't want to appear to reject the idea, so they created these sham reviews. "

Some one could say that this is a conspiracy theory. Saying "a reasonable hypothesis," rather than "a theory" would probably be more appropriate. A formal theory should be based on specific assumptions--for example, already accepted theories or data--and a logical derivation of the final statement.

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8) Second US Government_Investigation

The second DOE investigation of CF was announced in March 2004, nearly 15 years after the first one. The six most important scientific questions, based on new experimental CMNS claims, were:

(a) Is it true that unexpected protons, tritons, and alpha particles are emitted in some CF experiments?

(b) Is it true that generation of heat in some CF experiments is linearly correlated with the accumulation of ^{4}He, and that the rate of generation of excess heat is close to the expected value of the 24 MeV per atom of 4He?

(c) Is it true that highly unusual isotopic ratios have been observed among the reaction products?

(d) Is it true that radioactive isotopes have been found among reaction products?

(e) Is it true that transmutation of elements has occurred?

(f) Is the research methodology of CF scientists the same as that used by other scientists? In other words, is it consistent with the generally accepted norms of the so-called "scientific method?

A positive answer to even one of these questions would be sufficient to justify an official declaration that cold fusion, in light of recent data, should be treated as a legitimate area of research. But only the (b) question was addressed by the selected referees. They were asked to review the available evidence of correlation between the reported excess heat and production of fusion products. One third of these referees stated that the evidence for such correlation was conclusive. That was not sufficient; the attitude of the scientific establishment toward cold fusion research did not change.

Scientific disagreements are not supposed to be resolved by voting. Why was the reconized methodology of validation of claims--theories guide but experiments decide--not followed by the DOE-appointed scientists? Why did “rejections on theoretical grounds” prevail? The only answer I have is that scientists are not ideal; competition among them, as among people in other social groups, has both positive and negative effects.

Cold fusion will certainly be viewed as an interesting episode in the history of science, regardless of verdicts about validity of numerous CMNS claims. More specifically, the long-lasting CF episode will be remembered as a social situation in which the self-correcting process of scientific development was not allowed to flourish. To what extent was this due to extreme difficulties in making progress in the new area (without financial support from the DOE, NSF, etc.), rather than to negative effects of competition, greed, jealousy, and other "human nature" factors?

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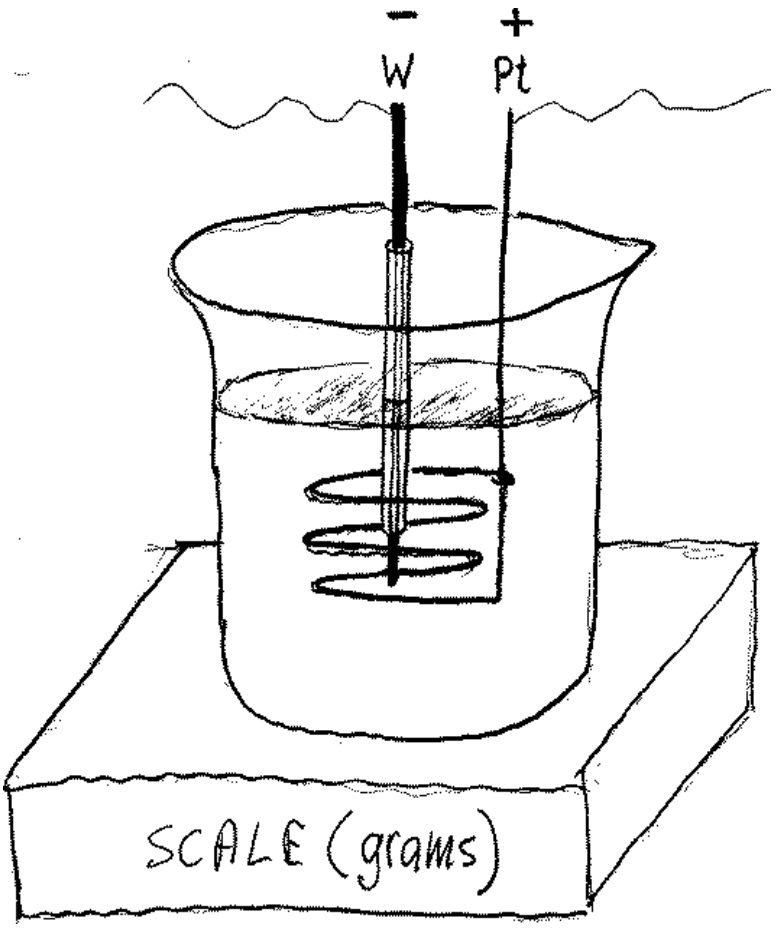
9) Excess Heat, Real Or Apparent?

The 2005 scientific conference in Japan (ICCF12), in which I participated, was devoted to CF. One French scientist, Pierre Clauson, described a high voltage electrolysis process in which excess heat was said to be generated at the rate of about 100 watts. After some hesitation I decided to verify this claim, with a colleague from Texas, Scott Little. The experiment turned out to be more difficult than I expected; we were not able to confirm or refute reality of excess heat.

But in a subsequent 2007 experiment, conducted with Richard Slaughter in Colorado, and Pierre who came to guide us, the excess heat was apparently detected. Why do I say "apparently"? Because the wattmeter we used, brought by Pierre from Paris, was later found to be inappropriate for our setup. The excess heat turned out to be zero when a more sophisticated wattmeter was used (in Paris, by Pierre and his partners.)



The diagram of our cell is shown above. Here is how the setup was described in my later conference presentation: *"[... Our] cell operating under such conditions is shown in Figure 1. The cathode was a tungsten rod while the anode was a large platinum wire spiral, or a platinized niobium cylinder. The electrolyte was potassium carbonate (K_2CO_3) dissolved in distilled water. The concentration was 20 grams per liter. Decomposition of water, at high current, was so intense that yellow glow discharge and arcing was taking place in the layer of gas-plasma surrounding the cathode."*

The scale supporting the cell was used to measure the amount of water evaporated in each experiment. That allowed us to calculate the amount of heat generated. Instruments used to measure electric energy are not shown in the figure.

Let me mention that Clauson's results presented at the conference were a confirmation of similar results reported by a Japanese scientist, Tadahiko Mizuno, two years earlier. In 1997 Mizuno wrote a book entitled "Nuclear Transmutations: The Reality Of Cold Fusion." It was translated into English by Jed Rothwell, our mutual CMNS friend. Detecting excess heat generated at the rate of 100 W was rather unprecedented; most often measured rates, in F&P type of cells, are 1 W and less. Before going to the conference in Japan--during a sabbatical leave-- I made an arrangement for working with Mizuno. But the plan had to be canceled, due to some administrative complications. Instead of working with Mizuno I was able to work with another top Japanese scientist, J. Kasagi. But this was not a CF project.

Many people reported generation of excess heat. But non of them claimed that excess heat experiments are reproducible on demand. The situation facing researchers in this area is well described by Mike McKubre, at a cold fusion conference-ICCF15 in Rome. The link to his report is

www.enea.it/it/produzione-scientifica/pdf-volumi/introduction-iccf15-proceedings-2.pdf

Mike is an electrochemist. I know him personally and I have no doubt that he is honest. In fact, he is a hero, in my view. How many people would be willing to continue studying CF for more that two decades under showers of insults.

I think that he is motivated by the noble desire to help society. Unfortunately, I did not attend ICCF15 in Rome in 2009. But I did participate in, and contributed to, four other conferences (USA-2003, France-2004, Japan-2005 and USA-2006). My impression was, and still is, that most CF researchers (not all) are like Mike, and that their methodology of validation is scientific.

Great scientists I have met, including Joliot-Curie, who introduced me to research, would agree. The difficulties described by Mike are real. What do I mean by showers of insults? I will explain this in a later chapter. Download Mike's paper and read it carefully; scientists among you will probably agree that there is nothing unscientific in it.

Speaking about irreproducibility I often refer to the following personal experience. I was heating milk in a microwave oven for two minutes. Then I removed the cup and inserted a cold spoon into it. At that moment I observed sudden "explosive boiling;" It was a clear indication that superheated milk was created. I tried to reproduce this next day, and several days later, using the same oven, the same cup, the same amount of milk, etc. but without success. Does it mean that my observation was not real? I do not think so. It only means that some important factors, perhaps the air temperature or pressure were different in subsequent experiments. Or perhaps the rate and angle at which the spoon was inserted into the cup were not exactly the same. Cold fusion phenomena seem to depend on factors which are hard to identify.

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10) Robert Park, A Scientist Writer

George Miley's observations can be contrasted with those of Robert Park, whose book "Voodo Science: The Road from Foolishness to Fraud," published in 2000, has already been mentioned. Referring to this book, one reviewer wrote: *, "Professor Park does more than debunk, he crucifies. .... You'll never again waste time or your money on astrologers, quantum healers, homeopaths, spoon benders, perpetual motion merchants, or alien-abduction fantasists."* But isn't CF different from the above? I don't exclude the possibility that some CMNS claims may have been fraudulent; con artists are naturally attracted to scientific controversies, as illustrated in:

http://csam.montclair.edu/~kowalski/cf/16voodoo.html

George Miley told me about the 9th International Connference on Cold Fusion, ICCF9. It took place in China, where many new discoveries were presented and discussed. The next conference, ICCF10, was to take place in Cambridge, Massachusetts and I decided to attend. One of the organizers of this conference told me that Robert Park was personally invited but decided not to come. That was an insult, in my opinion. What I would do in his place? I would welcome the chance to meet the authors of questionable claims, to discuss controversial topics and to ask for evidence. Dr. Park's refusal to participate disqualified him in my eyes. Those who accuse others of being pseudo-scientsts should have the courage to face their opponents.

In a review of Park's book at: http://home.netcom.com/~storms2/park.html

Edmund Stoirms wrote:

[...But to Park] *the explanation becomes more important than the observation. Because this particular explanation can not be believed, the observation must also be rejected. Thus, a major flaw in modern science is revealed - a Theory is more important than an Observation. The behavior of nature is not real unless it can be explained, especially using conventional concepts. This flaw in logic is at the heart of the book and provides an explanation for rejection of these and other subjects by many scientists.*

__New discoveries always conflict with some dearly held belief. This conflict when used to reject the claims, prevents new discoveries from being explored and properly explained. This is not to say that all 'strange' ideas are correct or

that all have a new and worthwhile explanation. Clearly, some should be rejected as being caused by obvious error, fraud, or simple insanity. The problem comes in deciding how much time and resource should be devoted to a search for an explanation and how the resulting facts should be evaluated..... If science is to clean up its act, this defect in the approach scientists use needs to be addressed. A clear and extensive discussion of this general problem can be found in the book "Revolution in Science" by J. Bernard Cohen (1985) or "Forbidden Science" by Richard Milton (1994)."

Robert Park is not alone in his bias against CF scientists on the basis of authoritarian pronouncements made by DOE investigators. He apparently does not need to hear the CF reports, or perform his own experiments. Fortunately, such an attitude does not prevail among mainstream scientists, but it is common. Aggressive discrimination against CMNS reminds me of something else. Long ago, when I was a communist student in Poland, I believed that genetics was pseudoscience. That was the official party line, supporting Lysenko's teaching. The same was true about cybernetics; it was defined for us as "bourgeois pseudo-science serving American imperialism." Naturally, no one is takes such statements seriousey today, even in Russia. But disagreeing with them could have been dangerous, when Stalin was alive.

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11) Three Professional Biographies

These short professional biographies of Fleischmann, Pons (chemists) and Jones (physicist) appear on pages 46-49 of E.F. Mallove's book : "Fire from Ice; Searching for Truth Behind the Cold Fusion Furror," John Wiley & Sons, New York, 1991. I strongly recommend this book.

***Martin Fleischmann**, now a naturalized British subject, was born March 29, 1927. [...] Since 1986, Fleischmann has been a Fellow of the Royal Society, an honor given only to the most distinguished of scientists. The author of over 200 scientific papers [...], Fleischmann won the Royal Society of Chemistry's medal for Electrochemistry and Thermodynamics in 1979. He was president of the International Society of Electrochemistry (1970-1972). In 1985 he was awarded the Palladium Medal by the U.S. Electrochemical Society.*

***Stanley Pons**, born in 1943, attended Wake Forest University in Winston-Salem, North Carolina, graduating in 1965, and began advanced studies at the University of Michigan at Ann Arbor. But with his doctorate almost in hand in 1967, he, the eldest of three brothers, left school to work in his father's prosperous textile mills and to manage a family restaurant in North Palm Beach, Florida. Eventually, his love for chemistry drew him back to active science.*

With the encouragement of faculty at University of Southampton in England, he entered its graduate program in chemistry and received his Ph.D. there in 1978. Martin Fleischmann was one of his professors. After being on the faculty at Oakland University in Rochester, Michigan, and the University of Alberta in Edmonton, Pons came to the University of Utah in 1983 as an associate professor, becoming a full professor in 1986, and Chairman of the Department in 1988. He has authored or coauthored over 150 scientific publications.

***Steven Jones** was well known to physicists and the hot fusion community, which gave him a credibility that Fleischmann and Pons could not match. That Jones came out with a dissimilar but closely related item of cold fusion news at about the same time, ironically, may have boosted the credibility of Fleischmann and Pons in their claims. But there was initial confusion about what Jones was asserting, because of his well-known earlier work on cold fusion of a different sort -- the concept called muon-catalyzed fusion.*

Much of the difficulty that ensued between Fleischmann and Pons on one side and Jones on the other -- a friction that has now lessened considerably -- can be understood in part from a chasm of personality differences. [...] Jones pursues his science with religious fervor, almost literally. His University stationery bears witness, inscribed as it is with the Brigham Young University motto, 'The Glory of God Is Intelligence.' " Yes, Jones' research was focused on nuclear reactions, not on excess heat. He was fully aware that excess heat associated with reactions he was studying

was too small to measure. The discovery of excess heat was announced by F&P only.

In my opinion that discovery, after being verified several times, should have been announced, more or less, in this way: 'we know that the excess heat is produced in our cells but we have no idea what process is responsible for it.' The approach would be --'let us agree on facts before discussing conceivable interpretations.'

Some think that discovering an experimental fact without giving some kind of explanation is not a scientific event. I cannot agree with this. Electric batteries, for example, invented in the early 1800s by Alexander Volta (Italy), became valuable long before their operation was theoretically explained. The same can be said about the accidental discovery of X rays, in 1895 by Wilhelm Roentgen (Germany). And who would believe that radioactivity of uranium, discovered in 1897 by Henry Bequerel (France), would lead to atomic bombs and to electric power plants, half a century later? Will CF lead mankind to new technologies in the next century? It's possible but not certain.

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12) The First CF Conference I Attended

One year after the Albuquerque conference I attended the conference devoted to CMNS topics only. Such conferences have been taking place each year, in different countries, since 1990. This was the tenth one (ICCF10); it took place in Cambridge, USA. I did not come to the conference empty handed. But my presentation, entitled "The Dilemma of a Physics Teacher," had nothing to do with specific CMNS claims; it was about how to deal with CF claims in classroom situations.

In the first half of my talk I said: *"This presentation is dedicated to a high school chemistry student who sent me an e-mail message last spring. She wrote: 'Help! My name is Maggie Johnson and I am a sophomore at Saratoga High School. In my chemistry class, I am doing a project on Cold Fusion. I was looking on the Internet for websites on Cold Fusion, and I came across links to your Cold Fusion items. I was wondering if you could give me some advice or information?'*

A year ago I would have replied that cold fusion is pseudoscience. But I am no longer comfortable with this kind of reply. Why am I not comfortable? My first opinion was based on Huizenga's famous ERAB report. I knew the author personally and I respected him. His criticism of cold fusion was convincing because it was based on the idea that cold fusion is a thermonuclear reaction between only two colliding hydrogen ions. Experimental data certainly do not support such an idea. [... They seem to indicate that a much larger number of atoms, in a crytal, participate in each CF event]

Two other factors helped to discredit the cold fusion field in many minds: (a) the claim that experiments in this area are extremely simple, and (b) that practical applications are going to be possible very soon. Again, I do not know who the authors of such claims were. Those who criticize cold fusion today, Park in the US and Kruglyakov in Russia, essentially repeat Huizenga's arguments. What was convincing in 1989 is no longer convincing today. Why do they ignore generation of helium? Why do they ignore more sophisticated calorimetry? Why do they ignore unnatural isotopic ratios? Why are they not at this conference listening to presentation of new data and defending their own ideas? That is another set of questions that I am not able to answer. Ignoring experimental data is not an acceptable method of addressing a scientific controversy. I am still not convinced that cold fusion is real. But I no longer say that it is voodoo science.

Why am I still puzzled? Because everything I know about nuclear science goes against the idea that nuclear reactions can be induced by chemical processes at ordinary temperatures. I wish I had a chance to personally participate in experiments generating extraordinary results. But, like most teachers, I have no access to a sophisticated laboratory which would be needed to verify accumulation of helium and heavier reaction products. I read about such phenomena and I am impressed. But I would be more comfortable if the reported results were examined and officially confirmed by an appointed panel of open-minded experts.

I am also puzzled by the fact that hundreds of sophisticated research scientists exploring cold fusion over the past 13 years have not yet developed a reliable demo for teachers; windows of opportunity did exist in several countries. Teachers need experiments that can be performed with simple instruments available in colleges and universities, such as Geiger counters and gamma ray spectrometers. Excess heat generated at a rate of about one watt is not convincing unless one is able to deal quantitatively with all possible chemical reactions taking place in the apparatus. I am not a chemist.

Reproducible generation of excess heat at the level of twenty watts, or higher, for a long period of time, would be much more convincing to a physics teacher, especially if it could be correlated with emission of nuclear particles or gamma rays. Even a 70% reproducible demo would be useful; teachers know that some experiments, for example in electrostatics, do not succeed when humidity is too high. Cold fusion seems to depend on factors which have not yet been identified. Abnormal isotopic ratios, reported by many independent researchers, are extremely convincing but a typical teacher can not verify such data. [...]

I am optimistic that the cold fusion controversy will be resolved, one way or another. The optimism is based on the following quotation from what John Huizenga, the author of the ERAB report, wrote in 1989. 'The scientific process is self-corrective. This unique attribute sets science apart from most other activities. The scientific process may on some occasions move slowly, sometimes even along a circuitous path. The significant characteristic of the scientific method, however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error.'

And here is another quote from the panel of appointed scientists responsible for the first national investigation of cold fusion. 'The Panel recommends against the establishment of special programs or research centers to develop cold fusion. However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system.' Sympathetic attitude toward unresolved issues is worth emphasizing.

What will the verdict of history be? Sooner or later, perhaps in 50 years, the cold fusion puzzle will be resolved (like the 'puzzle of cybernetics,' or the 'puzzle of genetics,' both in USSR). Only two outcomes are possible: (a) CF phenomena will finally be confirmed or (b) CF phenomena will not be confirmed. In each case one will have to deal with important social issues. Suppose that CF is confirmed. Then one would have to explain causes of a long-lasting conflict between scientists and administration. Suppose that CF is not confirmed. Then one would have to explain a phenomenon of massive self-deception involving hundreds of top scientists in many countries. In either case you will be recognized as participants of an important and unique event in the history of science.

Keep working to clarify the most intriguing scientific and social puzzle of the 20th century. I am certainly not the only physics teacher waiting for a consensus on cold fusion. Keep submitting good papers to traditional refereed journals, such as Physical Review, etc. Do not be discouraged by frequently unjustified rejections of your papers. Document such rejections and make them known to mainstream scientists. Deplorable confrontations with overly-bureaucratic editors should also be exposed. Take advantage of the new electronic journal devoted to cold fusion. Dissociate yourself from voodoo scientists and openly criticize them. Keep bringing cold fusion topics to scientific conferences devoted to areas overlapping with your activities. My own interest in cold fusion was reawakened at such a conference one year ago. Try to seek contacts with students, and with the general public. But focus on puzzling scientific results; it is too early to speculate about practical applications."

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13) The Editor Of Physics Today Rejected My Letter

Discrimination against CF manuscripts, by editors of major scientific journals, has been described by George Miley, in Chapter 5 above. Let me describe my own experience with this kind of unjustified bias. It is also a long quote from my

ICCF10 talk. In the second half of that presentation I said:

"About half a year ago I wrote a letter to the editor of Physics Today. In that letter I described my own dilemma, as a teacher, in dealing with cold fusion, and asked for help. Why was my short letter rejected? Why was I not allowed to see what the referees wrote about it? Ironically, that letter was triggered by the article entitled 'New American Physical Society's Ethics Guidelines.' That article by Jim Dawson was published in the January 2003 issue of Physics Today." Why "ironically? Because what they did was an example of behavior inconsistent with the article they published.

"I welcomed the new guidelines and asked how a physics teacher can make sense of 'cold fusion?' Was the research conducted in that area, in the last ten years, a 'departure from the expected norms of scientific conduct'? Did it 'lead other scientists along fruitless paths?' I see no evidence that the data were 'fabricated.' As a physics teacher I am confused by the situation. Some say CF was 'a fiasco' while others say it was an 'important discovery.' How should teachers address this topic in the context of 'public affairs between science and society,' or in the context of discussing 'institutional support for new ideas and innovations?'

After waiting several months I sent an e-mail message asking about the status of my letter. On Thursday, June 12, 2003, I received a reply from Letters Editor at Physics Today. He wrote: *"So far, I have a split decision on the possible publication of your letter. I expect soon to have a tie-breaking input from a third reviewer. I will let you know as soon as I have a firm decision. Thank you for your patience*." On Thursday, July 3 Dr. Hanna wrote "*We have completed our review of your letter commenting on the APS ethics guidelines story in our January 2003 issue. Our decision, after some valuable discussion, is not to publish your letter. Thank you for writing and for your interest in Physics Today."*

Unhappy about this I wrote *"I would very much appreciate if you could send me the reports of the referees evaluating my letter to the editor."* The immediate reply was: *"Please let me explain. I know that scientists who submit articles to peer-reviewed scientific journals expect reviewers to give them a critique of their letters. Physics Today is not, in the strictest sense, a peer-reviewed scientific journal; it is, instead, a special-interest magazine for physicists. Generally, my reviewers are staff writers and editors (all physicists) who may give me little more than 'thumbs up' or 'thumbs down' on a letter. As a rule, we do not give out the specific comments of the reviewers, because we consider them to be internal business. Thank you for your inquiry. I hope my explanation has helped."*

That was the end of our correspondence. Why was it not obvious to them that my letter to the editor was not about specific CMNS claims. I hoped that readers of the journal will comment on my 'special-interest" plea, advising me how to resolve the dilemma. That what Letters To The Editor are for. I often benefit from reading that section each month. Why did the editor decide to deprive me of this valuable resource? How can this censorship be justified in the context of 'New American Physical Society's Ethics Guidelines'? Something was not right. My experience was consistent with George Miley's observations.

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14) Meeting Fleischmann And Jones

I was lucky to personally meet Martin Fleischmann and Steven Jones, whose research triggered the CMNS controversy. Fleischmann's presentation at ICCF10 (the first CF conference I attended) was based on what happened in the past. I was surprised to hear that he was motivated by profound theoretical consideration, and by results of experiments performed half a century earlier. At the end of the talk he said: *"I believe that the work carried out thus far amply illustrates that there is a new richly varied field of research waiting to be explored."* Jones made three presentations based on work in progress. Fleischmann was kind enough to allow me to be photographed in with him. He is wearing glasses on the photo shown below.

Jones and his coworkers presented two papers devoted to detection of nuclear projectiles from titanium foils saturated with heavy hydrogen. The first presentation was devoted to energetic protons, the second to emission of energetic neutrons. Instruments they used were similar to those I used in my post-doctoral studies. That is why I approached Jones and asked for numerous details. At the end of this conversation Jones invited me to visit his laboratory at Brigham Young University. I visited him several months later and he gave me a sample of titanium to investigate at home. I hoped to discover delayed emission of protons and alpha particles.

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15) New CF Results Reported By Other Researchers

The most impressive ICCF10 presentations, from my point of view, were those of Yasuhiro Iwamura, from Japan, and Dennis Letts, from Texas. Iwamura and his coworkers diffused heavy hydrogen through a thin palladium foil. The surface where hydrogen entered was covered with a chemical element Sr. The experiment lasted 400 hours. During that time the amount of strontium was progressively decreasing. This was accompanied by the appearance of the element Mn (molybdenum) on the other side of the foil. The number of atoms of Sr that disappeared was about the same as the number of atoms of Mo detected on the other side of the foil.

The provided interpretation was that atoms of strontium are turned into atoms of molybdenum. Instuments used in this investigations were totally unknown to me. But I was fully aware of the significance of the reported results. It was alchemy--a change of one element into another. The obvious question, which I did not ask, was about a possibilty of Mo contamination--how do you know that the detected molybdenum was not originally present in the setup? Anticipating such questions, Iwamura provided an answer. The isotopic composition of molybdenum was very different from that found in nature.

Alchemy, by the way, is known to be impossible unless nuclear reactions are involved. Production of plutonium from uranium, to make atomic bombs, is an example of "nuclear alchemy." Turning atoms of one element into another, by means of nuclear reactions, is usually called transmutation. Even turning mercury into gold is now possible; but one gram of gold produced via transmutations would cost more than one billion dollars. Presence of transmutation products in a reproducible-on-demand CMNS process would be a very strong indication that nuclear reactions do indeed take place at a low temperature.

Letts' setup was much less sophisticated than that of the Japanese scientists. It was an F&P type of experiment, designed to demonstrate production of excess heat. What impressed me was the fact that results--generation of excess

heat at the rate of about one watt--became reproducible when the cell was irradiated with a beam of laser light. Absence of reproducibility was the Achilles' heel for this kind of experiment.

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16) Cooperation With Oriani

I was also impressed by Richard Oriani's ICCF10 presentation. He reported emission of alpha particles during electrolysis. These particles were detected in a plastic material known as CR-39. Most eyeglasses are now made from that transparent material. It turns out that an alpha particle, stopped in CR-39, creates an invisible track. Such tracks become microscopically visible after the material is chemically processed. I learned about this method of detection of nuclear particles in Europe, about four years before coming to the US. But the material I used was natural mica, not CR-39.

Let me digress and describe how I became one of the first Europeans to use mica to detect fission fragments (not alpha particles). I was working on my Ph.D. project, in Orsay, near Paris. At that time I was already an expert in using several kinds of detectors of nuclear particles. One afternoon I received a telephone call from our librarian. She told me that she had an American guest interested in fission. "Please come and take him with you," she said, "I am too busy with other things." That is how I met John Walker, who invented the method of detection of fission fragments with common mica. He wanted to demonstrate the method to people who might be interested. I had everything he needed and two days later we observed tracks of fission fragments.

I did not use mica in my dissertation project but I used it in the US, when I became a postdoc at Columbia University, several years later. My most important scientific contribution to nuclear physics, during that time, was made by using mica detectors. CR-39 is used in the same way as mica, except that turning invisible tracks into visible tracks is slightly different. Learning how to detect alpha particles with CR-39 presented no difficulty to me; I mastered that skill very quickly. I was lucky to be invited to Oriani's laboratory, at the University of Minnesota. Two of our replications of his results, which had been reported at the conference, turned out to be highly successful.

After each replication we examined the CR-39 chip removed from the electrolytic cell and a control chip that was not in the cell during the experiment. The second chip was chemically etched in exactly the same way as the first one. Then pits on each chip were counted under the microscope. The control chip was used to measure the unavoidable background, such as alpha particles from cosmic rays, radon, etc. In each case tracks on the control chip were much less numerous than tracks on the experimental chip. The next step for me was to perform similar experiments independently. To be sure that my cell was exactly the same as his I ordered it to be made in the shop in which Oriani cells were made. I returned home and started experimenting immediately.

Unfortunately, my results were not as reproducible as our results in Minneapolis. In the first experiment I saw nothing except the natural background, on both chips. In the second experiment the number of observed alpha particls on the experimental chip was much higher that on the control chip. But the third experiment's result were essentially the same as the first one. Subsequent experimental results were also not reproducible. In other words, I was not able to either confirm or refute the results described by Oriani. This, however, was not the end of the story; I will return to this topic on Chapter 22. Results from my cooperation with Letts and Jones are described in the next chapter.

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17) Next CMNS Conference: My Three Presentations

The next CF conference, ICCF11, took place in Marseille, France. This time I did have some experimental results to share, plus two 'philosophically-oriented' presentations. Jones, Letts and Cravens were coauthors of my experimental

report, entitled "Charged Particles from Ti and Pd Foils." Referring to my cooperation with Jones I said that his Ti foil was *"sandwiched between two CR-39 detectors for the period of 55 days. [...] The number of tracks on the experimental chip was 225; the number of tracks on the control chip was 132. Such results, if generated by a Geiger counter, for example, could be used as evidence of nuclear particles being emitted from the foil."*

Then I explained why the difference, 225-132=92 was not sufficiently large to draw the same conclusion from our CR-39 experiment. Additional experiments, I said, will be performed. Unfortunately, such experiments were not performed; Jones was apparently distracted by other matters. According to Wikipedia, "he retired on October 20, 2006 with the status of Professor Emeritus." Fortunately, the low-numbers-of-counts problem did not exist in my analysis of palladium foils from Texas. Here is how my cooperation with Letts was presented in the conference report:

"[... L.K.] asked for a chance to look at a possible 'nuclear signature.' Three palladium cathodes: Pd-613, Pd-616, and Pd-615 were sent to L.K. and he exposed them to the CR-39 detectors. [...] The tracks were counted, under the microscope. The results were: (a) about 500,000 tracks on the two detectors sandwiching the Pd-613 cathode, (b) about 11,000 tracks on two detectors sandwiching the Pd-616 cathode, and (c) no tracks above the background on the detectors sandwiching the Pd-615 cathode. ....Only then was L.K. informed that the Pd-613 generated an unusually high amount of excess heat, the Pd-616 generated much less excess heat, and Pd-615 generated no excess heat at all. He was also informed that all three cathodes were cut from the same sheet of pure palladium, and that the electrolyte from the Pd-13 was known to be contaminated with uranium."

In other words, the huge number of tracks from the Pd-613 was most likely due to uranium. The Pd-616 and Pd-615 results, on the other hand, were highly significant. They demonstrated a correlation between the amount of excess heat generated and the number of alpha particles produced. [.] In the same presentation I said that cooperation with Letts also did not have a happy ending. To establish a correlation between the excess heat and emission of alpha particles one needs more than two samples. But new samples were not sent to me. In 2005, alarmed by the situation, I sent the following e-mail message to Dennis Letts.

"I am going to galley proof our Marseilles presentation. This puts me in an awkward situation. If I were reporting on my own work I would add a short paragraph, something like this: 'No additional experiments were conducted to confirm observations made 6 months ago. The unexpected delay is due to [.]' or something like this. But in this case I was only a messenger; you are the real player. A reader is likely to be interested in the current status of our investigation. I think that it is not right to report positive results only and keep negative results hidden. Do you agree? [.]," The following reply was received several hours later.

"No additional experiments were conducted to confirm observations made 6 months ago. The unexpected delay is due to the fact that experiments seldom work on a schedule. The calorimeter had to be modified slightly to re-store design stability and precision. Also, we have not observed laser-triggered excess power since August 2003. Of course I agree [with your last statement] - since changing metals at the end of 2004, my success rate has been zero. This is compared to a success rate of 87% during the years of 2000-2004. Other than changing Palladium stock, I don't know what has caused the sudden loss of the laser effect. Experiments have been conducted in a high quality calorimeter, in a moderate quality calorimeter (my Avanti) and on the open bench. The laser effect has not re-appeared under any of the above calorimetric conditions. Experiments are being conducted now to re-establish the laser effect or to explain why it stopped working. You may use this information in any way you wish, including an addendum.

With regard to reporting negative results, consider this: Cravens and Letts discovered the laser effect in September 2000 and reported the positive results publicly in August 2003. We spent 3 years testing the credibility of our result before reporting publicly. We anticipate behaving in a consistent manner now - we have negative results but we're not in a rush to report until we're sure that we have negative results and try to provide some reasons why the results are negative. I believe that reporting results formally by 2007 will be consistent with our previous work and should not be considered 'keeping negative results hidden'.,"

I am sure that neither Letts nor Cravens are trying to hide negative results. Like most CMNS researchers, they are honestly following scientific methodology of validation of claims. But, as explained by McKubre, outomes of experimental results often depend on unknown parameters. That was a good illustration. No new samples were ever

sent to me.

What a coincidence! While I am describing this past cooperation Dennis Letts has just announced (March 2012) the development of a single mathematical equation that is able to accurately reproduce results from 40 experiments he conducted with Cravens and Hagelstein in 2007-2008. Additional experiments will soon be performed to test theoretical predictions. Is this going to lead to a great step forward? I hope so.

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18) My Two Other ICCF11 Presentations

Those who attend scientific conferences know that some contributions are presented as posters. This is unavoidable when time is limited. My non-experimental presentations appeared as posters; both were published in the Conference proceedings. The first, entitled "Recent Cold Fusion Claims: Are They Valid," can be read online at:

http://pages.csam.montclair.edu/~kowalski/cf/152summary.html

It was a manuscript--a review of the CMNS field--with 37 references. I wrote it for publication in a mainstream scientific journal. Unfortunately, that manuscript was rejected by editors who received it. The second poster, as shown in the next Chapter, described my personal experience with the process of rejection of a CF paper.

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19) History Of Attempts to Publish

My unsuccessful attempt to publish a letter to the Editor of one journal--Physics Today--has already been described in Chapter 13. What follows is a description of other unsuccessful attempts. Knowing that the second DOE review of the CMNS field was approaching, I summarized what I had learned about CF. This took the form of a manuscript that I wanted to publish, for the benefit of people inerested in science and technology. The title of the article was "Recent Cold Fusion Claims: Are They Valid?" Seven journals to which my manuscript was submitted were:

Physics Today, USA
American Scientist, USA
Scientific American, USA
Nature, UK
New Scientist, UK
The Physics Teacher, USA
Science, USA

a) The Cover Letter

Each submission had essentially the same cover letter. In that letter I wrote: *"I am sure that you are aware of the DOE move to review the cold fusion field, as reported in The New York Times (3/25/04). Attached is a review article which, I hope, can be published in [your journal]. The title is 'Recent cold fusion claims: are they valid?' It is not a paper defending cold fusion claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers.*

Some of these claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem

to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. The subject is interesting no matter what the final verdict of the second DOE evaluation will be.

Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. In writing the review I was not aware of the pending DOE investigation. I deliberately avoided references to social aspects, which are interesting but highly controversial. I am a physics teacher at Montclair State University. Studying cold fusion was my 2003/2004 sabbatical project."

b) Reply From the Editor Of Physics Today

," Dear Dr. Kowalski: We received your article submission titled, "Recent Cold Fusion Claims: Are They Valid?," and appreciate your sending it to Physics Today. After reviewing it, however, we have concluded that it does not meet our editorial needs. Thank you for your interest in Physics Today. Sincerely, Stephen G. Benka Editor-in-Chief."

c) **My Comment**

That is it. Not a single word about the content of the article. How can the phrase ,"does not meet our editorial needs," be interpreted? Why was the article not sent to referees? They do publish many field summaries each year. Why was my summary not given the same chance to be reviewed by experts? Was I writing about sociology, poetry, business or something else unconnected to physics? Are recent cold fusion claims described in the article already widely known to most physicists? Was my description of these claims erroneous? Was the article rejected because of its style, its limited scope, or its disregard for ethical standards?

d) Reply From the Editor Of American Scientist

"Dear Dr. Kowalski: Yes, we've received your original manuscript and the follow-up. I'm afraid we're not always able to acknowledge receipt immediately. I try to give a prospective author an idea of whether we'll be able to consider a manuscript, and sometimes it takes a little time to determine that. We have certain basic criteria for submissions. When a submission does not meet those criteria, I prefer to say that it cannot be considered rather than simply acknowledge receipt.

In the case of this submission, I'm unsure. We publish feature-length articles and commentaries based on original published research. The authors of American Scientist articles are the people who have done the work and therefore are in a position to survey their own field. I don't actually have evidence (in the form of cited publications or a c.v.) that you have done original research on the topic you propose to write about.

If you would like to publish a short commentary, we do have a department with different criteria, called "Macroscope." This is where we publish short essays conveying a scientist's point of view on a matter of personal or professional interest to scientists and engineers. The maximum word count is 1,500. If you would like us to consider publishing your piece in a short form, please let me know, and I'll share it with my colleagues and let you know the response. Sincerely, Rosalind Reid Editor, American Scientist."

e) My Reply:

"Dear Dr. Reid: Thank you for your prompt reply. I understand your hesitation. Protecting readers of American Scientist from people who are not qualified to write about science should be one of your tasks. To help you decide here is a little summary about myself.

I am an experimental nuclear physicist (Ph.D., 1963) with a large number of publications (mostly as coauthor) in that field. The attached abbreviated list of publications, spanning four decades, makes it clear that my teaching

commitment has not prevented me from active participation in nuclear physics research. Like most scientists, I accepted the 1989 verdict about cold fusion. And you are correct, I have no publications about cold fusion. My new interest in this field was triggered in October 2002. I attended a nuclear conference in New Mexico and heard several scientists talking about cold fusion research. It was the beginning of my sabbatical year. The paper I submitted is the product of that work.

I hope your hesitation will not prevent you from sending my article to competent and unbiased reviewers. Please let me know what your decision will be. Meanwhile I would like to follow your suggestion about writing a short commentary on the anticipated review of cold fusion by the DOE; see the attached file. Thank you for your consideration. Sincerely yours, Ludwik Kowalski.
A list of my selected publications, and a file containing my ,"short Golden Egg piece," (see below), were attached.

f) Seek Not The Golden Egg, Seek The Goose (attached file)
According to a recent article in The New York Times (3/25/2004) the US Department of Energy (DOE) is going to review the field of cold fusion this year. This is a significant event; the controversial field of cold fusion (CF) has often been called pseudoscience. If it were up to me I would suggest that the panel of DOE scientists focuses on essential scientific questions and not on practical applications which are far away, at best. Promising too much, and too early, was one of the mistakes made fifteen years ago. In my opinion the six most important scientific questions are:

1) Are unexpected neutrons, protons, tritons and alpha particles emitted (at low rates) in some CF experiments?
2) Is generation of heat, in some CF experiments, linearly correlated with the accumulation of ^{4}He at the rate of 24 MeV per atom of ^{4}He?
3) Have highly unusual isotopic ratios been observed among the elements found in some CF systems?
4) Have radioactive isotopes been produced in some CF systems?
5) Has transmutation of elements occurred in some CF setups?
6) Are the ways of validating scientific findings in the areas of CF research consistent with accepted methodologies in other areas of science? I think that a positive answer to even one of these six questions should be sufficient to justify an official declaration that ,"cold fusion, in light of recent data, should be treated as a legitimate area of research.," The normal peer review mechanisms will then be used to separate valid claims from wishful thinking.

g) After Waiting Several Days I Sent This Addendum

"I already mentioned two reasons making such review urgent: the 15th anniversary of the Utah announcement and the pending DOE investigation. In my opinion, by publishing my paper, or a review written by somebody else, you will contribute to something desirable. Nobody is happy with the unhealthy feud between a group of well motivated researchers and official representatives of 'mainstream science.' Most people are passive but those who do take extreme positions often use highly perjorative adjectives, such as 'pathological', stubborn, misguided, and fraudulent.' Please do not miss an opportunity to contribute to ending this unnecessary feud. I would be happy to give you names and addresses of top people in five main areas of cold fusion.

So now you have several excuses for bending a rule of your editorial policy. They are: a) the anniversary, b) the pending DOE investigation, c) my paper is a review describing (very objectively, and without accusations of any kind, as you probably noticed) several very different areas of a broad field, d) my background as an active nuclear physicist, and e) my unpublished research in two areas of cold fusion. You are certainly aware how difficult it is to publish cold fusion research papers in important scientific journals. Will the situation change after the pending DOE investigation of cold fusion? I hope so. Please help to contribute to this cause.

If you decide to approach Fleischmann, be aware that he is an electrochemist; I do not consider him to be an expert in nuclear physics. This became clear in 1989 and contributed heavily to the cold fusion controversy. One can only imagine what would happen if Fleischmann and Pons, who are chemists, refused to participate_in the infamous press release, organized by the administrators of the University of Utah, and decided to work with Steven Jones, who is a physicist. A year or two later they would publish a peer reviewed paper and [...] But I refuse to speculate; my goal is to heal the wound by focusing on purely scientific topics and by ignoring stupid things people said or wrote before.

Please help me. I think that cold fusion, no matter what the final verdict will be, is a highly significant episode in the history of science. Let your journal be a part of that history."

I also gave Dr. Reid names and e-mail addresses of five people (who are certainly much more knowledgeable than myself) suggested that she contact one of them to write a longer review paper for the journal. Steven Jones, Martin Fleicshmann and George Miley were among the scientists I selected. I did not hear from Dr. Reid again. Will she accept my ,"Golden Goose" item? Probably not.

h) Reply From The Editor of Scientific American

,"Dr. Kowalski: Thank you for your offer to contribute to SCIENTIFIC AMERICAN. After much consideration, I regret to say that the piece you propose is not suited to our somewhat limited editorial needs. We appreciate your interest in SCIENTIFIC AMERICAN. Regards, Jacob Lasky Editorial Administrator.,"

i) Reply From The Editor of Nature

,"Thank you for your inquiry about submitting your paper entitled 'Cold fusion 15 years later' to Nature. I regret that the paper that you describe seems unlikely to prove suitable for publication in Nature, and we accordingly suggest that you pursue publication elsewhere. I am sorry that we cannot respond more positively on this occasion. Yours sincerely Dr Karen Southwell, Senior Editor.,"

j) My Comment

I was aware, from browsing their web site, that the rate of acceptance in Nature is about 1 out of 10. On that basis I should have expected a rejection. Frustrated that my timely review of the Cold Fusion field is being delayed I decided to send it to another UK journal, **New Scientist**. But they never responded.

k) Reply From The Editor Of The Physics Teacher

, "Dear Professor Kowalski: We have reviewed your manuscript ,"Cold Fusion 15 Years Later," in the light of the recent Physics Today article 'DOE Warms to Cold Fusion.' While a paper in TPT on this subject may be warranted, we do not believe there is any great urgency to publish one immediately. After all, according to the Physics Today piece, DOE Deputy Director Decker says that their 'review of cold fusion will begin in the next month or so [that was back in April]' and it 'won't take a long time--it's a matter of weeks or months.'

We believe that it would be premature to publish a cold fusion paper in TPT before the results of the DOE review are announced. Were we to do so, a follow-up piece would almost certainly be required later, regardless of how that review turns out, and we don't feel that two papers on the subject are warranted. We will consider your paper again (along with any revisions induced by the DOE report) after the report is made public.,"

l) My Reply

"Dear Dr. Mamola": Was my manuscript examined by referees? I would very much like to see what they had to say about its content. Thank you in advance.," This message has not been answered. Will I ever see the referee's comments? Probably not.

m) Reply From The Editor of Science

"I've consulted with our editorial staff in the physical sciences. Unfortunately, we don't think this topic is an appropriate one for review in Science at this time. Thanks for thinking of Science. Sincerely yours. Donald Kennedy."

n) My Comment

Hmm, it was rejected on the basis of the topic, not on the basis of the content. George Miley was right; the editors of

most journals put this topic on their blacklist. The scientific methodology of validation of claims, made by recognized experts, does not count anymore.

I can now say that I have had personal experience with peculiar aspects of CMNS area: irreproducibility of experimental results and censorship imposed by editors of journals.

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20) Transmutation Of Radioactive Nuclei--Or An Artifact.

As mentioned in the Introduction, I came to the Albuquerque conference to learn about a claim made by a physicist who was not a CMNS researcher. He was discussing practicality of the idea of destroying radioactive materials by bombarding them with high-energy neutrons. That topic was at the center of my attention earlier, when I was on sabbatical leave in France. I had a chance to participate in the collection of experimental data on production of energetic neutrons by using energetic protons, from a large accelerator.

What a coincidence! One of the CF researchers I met at Albuquerque, a university professor, claimed that he was able to reduce radioactivity of uranium via a CMNS process. I definitely wanted to participate in the next phase of his experiment. But the scientist was not enthusiastic about this idea, claiming that it might create patent-related complications. Fortunately, I later learned that another CF researcher, Hal Fox from Salt Lake City, UT, made a similar claim, destroying radioactive thorium via a CMNS process. This time my suggestion that we should replicate the experiment together, in Hal's laboratory, was welcome.

Their high voltage electrolysis cell was similar to that shown in Chapter 9. The essential difference was that the cathode was made from zirconium, not tungsten, and that the substance dissolved in water was a thorium salt. To prepare myself for this task I read the paper by Hal Fox and Dr. Shangxian Jin very carefully. It had been published in Journal of New Energy. Hal was more an organizer and businessman than a scientist. But his younger co-worker was a highly qualified nuclear physicist.

It occurred to me, while reading their paper, that the destruction of thorium might have been an illusion, resulting from not taking under account one possible effect. I described this to Hal, before coming to Salt Lake. He agreed that the effect I suggested should be investigated. That is what we did during one week in 2003, in his Salt Lake City laboratory.

The experiment was performed twice, first more or less as described in their 1998 paper and then in the way I suggested. The first experiment consisted of the following four steps:

Step 1: A small amount of radioactive thorium salt was dissolved in water, in an open glass jar.

Step 2: A sophisticated detector placed outside the jar was used to measure gamma rays emitted by thorium. The result was close to 20,000 counts.

Step 3: An electric current was passed through the salt solution in the jar, a process claimed to be responsible for the destruction of radioactivity. After about 30 minutes the current was turned off. The immersed zirconium cathode delivering electricity to the solution was partially decomposed during this step; metallic fragments could be seen at the bottom of the jar.

Step 4: Our detector was placed in the same position as before and thorium radioactivity was again measured for two hours. This time the number of counts was close to 10,000. In other words, the final count was one half of the intial count. In that sense the replication of the 1998 experiment was very successful.

Why was the experiment performed in an open jar and not in a pressurized container, as in the original experiment? Because we believed that the setup could eventually be used by teachers to demonstrate a CMNS effect. Jars are widely avalable in school laboratories. And they are safer than sealed pressurized containers. Let me now describe our second experiment. The open jar and all instruments were the same as in the first one. The underlying idea of counting gamma rays before and after Step 3 was also the same. But the way of counting was modified, according to my suggestion.

The salt was first placed on top of the detector and gamma rays were counted for ten minutes. The number of counts was close to 100,000. This was not surprising, considering the very short distance between the source of radioactivity and the detector. Then the salt was dissolved in water and the electric current was passed through the solution for 30 minutes, as in the Step 3 of our first experiment.

Next we heated the jar and allowed all the water to evaporate. The thorium salt remained in the jar, forming a white precipitate, mixed with metallic debris from the partially destroyed zirconium rod. The deposit was collected and placed on top of the detector. Gamma radiation was again measured, for ten minutes. Suppose that the original claim--destruction of 50% of thorium by electric current--was correct. In that case the final count would be about 50,000, one half of the initial count. Our final count, however, was close to 90,000, a strong indication that the fraction of destroyed thorium was much lower than 50%.

Results from the two experiments allowed us to reach the following conclusions: (a) The final count was indeed reduced by a factor of two, as originally reported by Fox and Jin. (b) The reported reduction of count, however, was not due to destruction of atoms of radioactive thorium; it was due to the suspected artifact, redistribution of thorium in the jar, during Step 3. (c) The approximately 10% reduction of counts was attribued to thorium escaping with steam during the process, or deposited on the inner walls of the jar. Hal did not participate in this experiment personally. But he took the remaining precipitates and sent them to a chemical laboratory.

This episode put me in a rather delicate situation. Being a scientist, I wanted to publish the result, perhaps in the same journal in which the original claim appeared. But it was not my experiment and I decided to do nothing more. Hal had generously invited me (I stayed in his home, met his family, etc.) and the task of publishing the new result should have been on his shoulders. He said that he would wait for the result of chemical analysis. Did the results of that analysis reveal presence of transmutation products, expected by Hal? I do not know.

This was the first successful CMNS experiment in which I personally participated. The term success does not mean finding what was suspected. In the context of a scientic exploration to succeed means to obtain a clear yes-or-no answer--in this case about destruction of thorium atoms. I would be equally happy, or probably happier, if the last count were close to 50,000 rather than close to 100,000. That would be a strong indication that about 50% of thorium was indeed destroyed (rather than redistributed) during the process. Two other successful CF experiments, in which I participated, are described in the next two chapters.

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21) The Galileo Project

In November 2006 Steven Krivit, editor of the online magazine New Energy Times, started recruiting researchers for what he called The Galileo Project. They were to replicate experimental results obtained in the US Navy laboratory in San Diego, known as SPAWAR. A team of scientists from that laboratory, headed by electrochemist Pamela Boss, he wrote: " produced something unique in the 17-year history of the scientific drama historically known as cold fusion: simple, portable, highly repeatable, unambiguous, and permanent physical evidence of nuclear events using detectors that have a long track record of reliability and acceptance among nuclear physicists."

The task was to follow the SPAWAR protocol and detect alpha particles by using CR-39 detectors. Naturally I was

one of the six people who immediately agreed to participate. Scott Little, with whom I had worked in Texas, also became one of the independent participants. Oriani refused to join because, in his opinion, the SPAWAR CR-39 pits were due to corrosion rather than to alpha particles. I was well prepared for this task. Working at Montclair State University, with a student helper, I was able to replicate SPAWAR results in about three weeks. The Winter Meeting of the American Chemical Society was approaching and I knew that Pamela Boss was scheduled to talk about the original SPAWAR results.

That prompted me to sign up for the conference. Fortunately, my "last minute" application was accepted. They gave me a time slot to present the results, at the same session. The meeting was in Denver and I was able to be there just on time. Photos on my slides were practically the same as on those shown by the SPAWAR team. That was the happy part of my presentation; I could see smiles on faces of SPAWAR researchers.

But the ending was not happy; my conclusion was similar to Oriani's--the pits were not due to alpha particles. But I am not a corrosion expert, like Oriani. I said that the pits might have been due to much larger nuclear projectiles, such as fission fragments. To reach this conclusion I measured sizes of typical SPAWAR-like pits and compared them with sizes of pits due to particles from my alpha radioactive source. Friendly personal relations with Pamela deteriorated rapidly after the conference.

But we debated the issue on pages of European Physics Journal. Fortunately, its open-minded editor does send CF-related submissions to referees. The experiment was successful, a clear "yes" answer was obtained to the replication-of-experimental- results question. The disagreement was about the interpretation of results, not about the results themselves. I respect SPAWAR scientists; their numerous publications are certainly up to high standards. But R. Park, and others like him, probably continue calling them pseudo-scientists, without any justification. That is very unfortunate.

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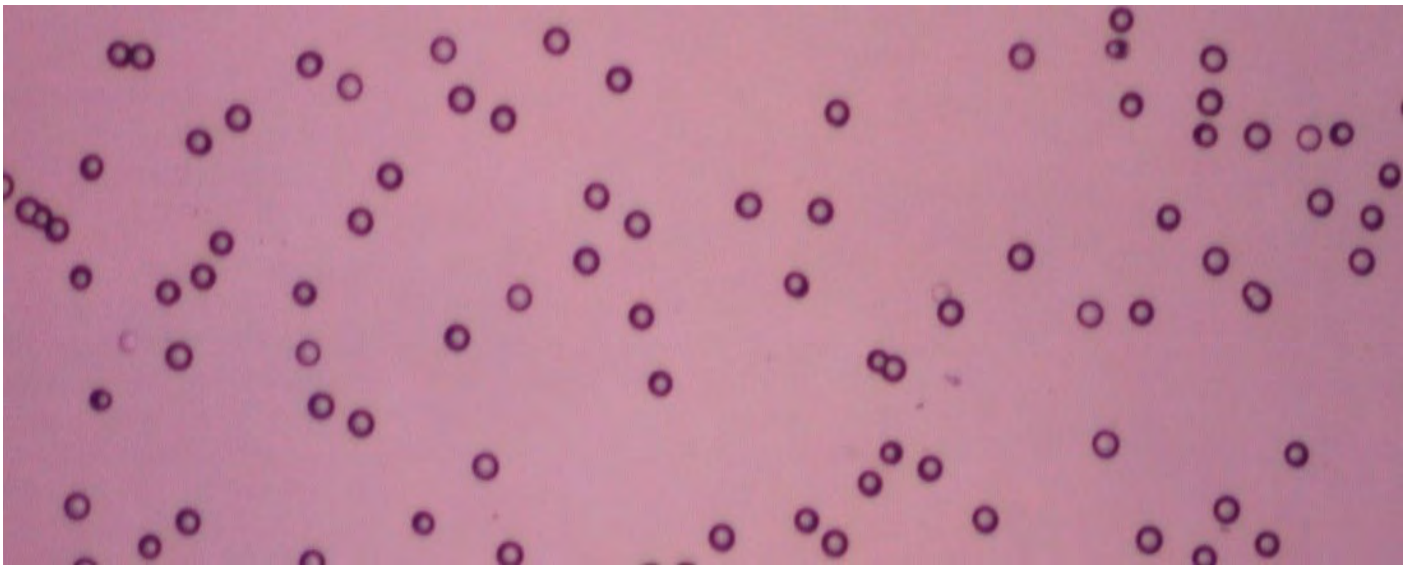
22) The Curie Project

My cooperation with Richard Oriani, described in Chapter 16, was not successful; the answer about reproducibility of results was neither "yes" nor "no." That was not a pleasant situation. But our cooperation was resumed after he changed the procedure (covering experimenta CR-39 chips with thin foils of Mylar). He submitted a paper with a new impressive results to a prestigious US journal, Physical Review. That manuscript was rejected but results were presented at our 2008 conference (ICCF14), in Washington, DC. His paper prompted me to make another attempt to replicate new data.

My 20 consecutive experiments, lasting three days each, were performed on the 27th floor of an apartment building, rather than in a private home with granite walls. The probability of being exposed to radon at this location was relatively low, especially in my study, where the window was kept slightly open. Additional precautions were made to minimize exposure of CR-39 to radon. Chips ready to be used were kept in distilled water, not in air. Etching of CR-39 chips and counting of pits, on the other hand, was done at the university. The photo below shows typical circular tracks of alpha particles in CR-39, as seen through a microscope.



The mean track density reported by Oriani was 122 per square centimeter; it was significantly higher than his measured background. My mean density, from 17 experiments, was only 16 per square centimeter; it was not significantly different from my measured background. Total elimination of background is not possible, due to radon, cosmic rays, and other contaminants.

Track densities on three of my experimental chips turned out to be very much higher than those reported by Oriani. My results, in other words, were not in agreemen with those reported by Richard. The exceptional results from these three experiments were attributerd to contamination. In a subsequently published paper I wrote that "attempts to identify contaminants were unsuccessful. Alpha radioactive substances such as uranium, thorium and radium are known to be present in our environment. One nano-gram of radium, for example, emits 37 alpha particles per second. Atmospheric testing of nuclear weapons in the 1960s contributed to contamination of our environment with long-lasting alpha-radioactive isotopes."

No one knew about my undeniable disagreement with Oriani's published results. Before sharing my data with others I summarized his results and asked, using the Internet, for people interested in replicating them, in the same way SPAWAR results were replicated in the Galileo Project. Three individuals expressed interest. One was a young engineer I had met in Denver, after the ACS conference (Jeff Driscoll). Two others (Mike Horton and Pete Lohstreter) were high school physics teachers.

Our cooperation, called the Curie Project, was very successful. Working independently, and unaware of each other's results, each experimentalist came to the same conclusion--measured track densities were not significantly different from those due to the measured background. The results were subsequently published in the Journal of Condensed Matter Nuclear Science, a peer-reviewed journal of the International CMNS Society. It can be downloaded as

<www.iscmns.org/CMNS/JCMNS-Vol5.pdf>.

Numerous details and illustrations are in this short article (pages 34 to 41). Would I volunteer to participate in another CF project? Probably not. But I will continue paying attention on future claims, as illustrated in the next chapter. My most recent publication in this area is the letter to the editor of Progress in Physics. The title is " Social Aspects of Cold Fusion: 23 Years Later." The link is:

<www.ptep-online.com/index_files/2012/PP-29-L2.PDF>

In the concluding section I wrote: *"The CF controversy is unprecedented in terms of its duration, intensity, and caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. CMNS researchers are mostly also Ph.D. level scientists. The same is true for those scientists who believe that the announced discovery of CF was a "scientific fiasco". We are still waiting for at least one*

reproducible-on-demand demonstration of a nuclear effect resulting from a chemical (atomic) process. In the case of CF the self-correcting process of scientific development emphasized by Huizenga has not worked. This fiasco seems to be due to the fact that scientists appointed to investigate CF claims did not follow [the welll established] rules of scientific methodology."

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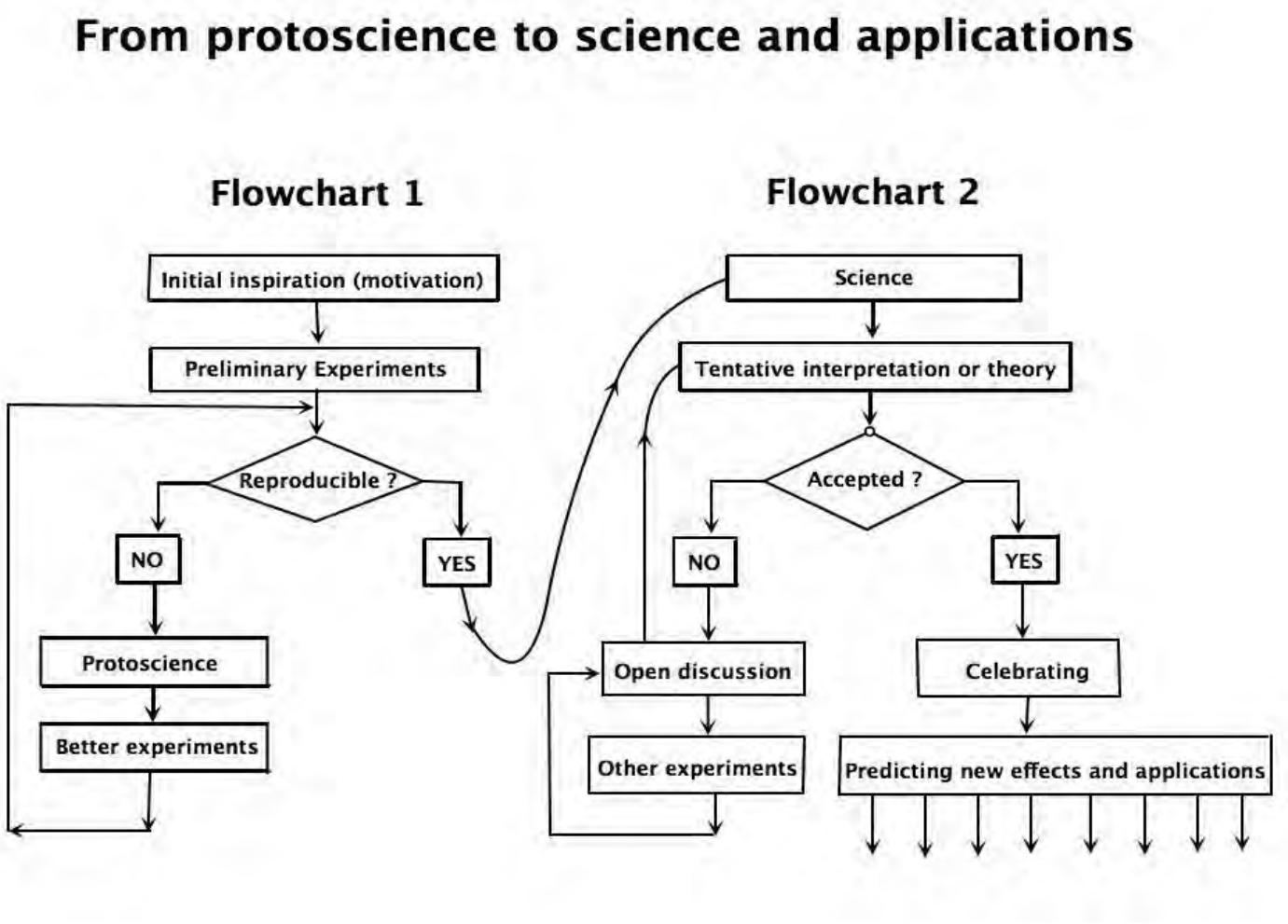
23) Flowcharts:The Last CF Conference I Attended

CMNS conferences take place each year. The next one will be in South Korea (summer 2012). But the last CF conference in which I participated took place in 2008, in Washington DC. The title of my presentation was "Nuclear Or Not Nuclear: How To Decide?



The above diagram was part of my presentation. It illustrates a typical experimental discovery process. What triggers the process (see the top of the left flowchart) is irrelevant. A subsequent experiment might or might not be reproducible. Non-reproducible results, in my opinion, belong to protoscience, not science. But they may become science, sooner or later. The next task is to interpret (understand) the result. The scientific community might or might not agree on a proposed explanation (theory).

This typically leads to other debates, and to calls for additional experimental data, as shown in the diagram. Practical applications might or might not emerge immediately, after a discovery is recognized as valid. But a valid discovery does become part of science, like each little stone is part of a magnificant cathedral. Practical applications usually

result from sets of many discoveries, including those made long ago.

CMNS claims, I said, still belong to protoscience. *"It is unfortunate that, except for The Galileo Project, researchers work in isolation from each other. This is understandable, each resercher does what matches his/her expertise and limited resources. This kind of work was going on for 19 years. [...] The task of turning protoscience into accepted science is still waiting for us. How to approach this difficult task and how to proceed more effectvely? In my opinion, well-focused cooperative investigations, as in The Galileo Project, are likely to be more productve, in the next two or three years, than uncoordinated efforts of many individuals."*

What is the main difference between hot fusion and cold fusion communities? In both cases the goal was to build a device whose energy output exceeds the energy input, without consuming chemical fuel. The hot fusion community has been trying to achieve the "break-even" point for five decades and it knows exactly why reaching it is so difficult. The cold fusion community, on the other hand, started by experimenting with break-even devices without understanding what was going on and why. What is the probability that something profoundly new will emerge from hot fusion? It is much lower than from cold fusion, in my opinion. In any case turning hydrogen bombs into candles is not going to be any easier than turning swords into plowshares.

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24) Andrea Rossi's Unbelievable Claims

As mentioned at the end of Chapter 2, I have a website devoted to CF--a cross between a logbook and a diary describing participation in CMNS activities. Writing this book would be much more demanding if this free online resource were not available to me. Another resource was a private Internet list for CMNS researchers. On February 26, 2010, the following patent description was posted on our list: "The patent applicant is engineer Andrea Rossi, owner of a small company, employing 2-5 people. In the patent he claims that *'A practical embodiment of the inventive apparatus, installed on October 16, 2007, is at present perfectly operating 24 hours per day, and provides an amount of heat sufficient to heat the factory of the Company EON of via Carlo Ragazzi 18, at Bondeno (Province of Ferrara).' (Italy).* This suggests that power output is at least tens of kilowatts!"

This is a reasonable estimate. Excess heat generated at the rate of tens of thousands of watts, for several months, was indeed a sensational claim. That would be equivalent to burning several tons of coal. Most excess heat demonstrations generate excess heat at the rate of one watt, or less, for much shorter durations.

Several people commented on the above announcement. I responded. *"A suggestion was made, two days ago, that someone should visit the place where spectacular results are available on demand. The visitor would either confirm or refute what has been reported. I do not think that an outsider would be able to evaluate the setup. What is needed is a blueprint and a detailed protocol. Following the protocol a team of competent researchers would try to build the device from scratch and to measure excess heat with their own instruments. Only team members should be allowed to enter the room in which the device is being constructed.*

Yes, I am thinking about the possibility of fraud. Fraudulent people, such as identity thieves and those who solicit profitable partnerships by email, do exist. Someone replied: "I strongly support your suggestion to organize a group to replicate the Rossi patent and I offer myself to assist. I have a modest laboratory and am well equipped to undertake gas absorption experiments."

Unfortunately, things did not develop along the path I suggested. What happened was a demonstration, on January 14, 2011, at Bologna University in Italy. It was followed by a press conference, etc., as photographically illustrated at:

http://www.journal-of-nuclear-physics.com/

The Bologna demonstration could have been more effective than it was, without revealing the nature of the secret catalyst. Rossi could have provided the blueprint of the apparatus to a trusted authority, for example, an Italian government laboratory, asking them to manufacture his simple device. They could have brought it to the University of Bologna and allowed Rossi to place the secret fuel (nickel powder mixed with something else) into the cylinder. He would not have been allowed to do anything else to the apparatus. That would eliminate any suspicion of a hidden energy source somewhere within the apparatus.

This, however, would not have eliminated another possible suspicion--that a chemical fuel was mixed with nickel. But suppose the powder supplied by Rossi is weighed, both before and after the experiment. Suppose the change in weight is negligible, in comparison with what it would have been if a suspected chemical fuel were present. That would rule out a possibility of the chemical-fuel fraud.

_
Generation of a huge amount of excess heat was not the only claim made by Rossi. He also wrote that 30% of nickel was transformed into copper, during six months of operation of his 12 kW reactor, that the radiation level was negligible, etc. Such claims were in conflict with everything I knew about nuclear physics. After becoming aware of this I decided to publish a paper that even undergraduate physics students would be able to understand. Unable to find a publisher I posted the paper at my website. This was in April of 2011. The article, entitled "Rossi reactors--reality or fiction," was subsequently published, in Progress in Physics (January 2012). The link is:

http://www.ptep-online.com/index_files/2012/PP-28-07.PDF

Rossi does not want to be involved in discussing physics--he is an inventor, not a scientist, he keeps emphasizing. He believes that the validity of his discovery will be confirmed by a large number of satisfied customers. At one time (January 2012) he stated that he had found two customers for his 1000 kW power plants. But their identity has not been revealed. I agree with him that a large number happy users will be a convincing argument. But I would not advise anyone to invest in his "secret technology" at this time. The possibility that Rossi has discovered something totally unknown is real but the probability of it is very low, in my opinion. I wish him well. We do need alternative sources of pollution-free energy.

"Is Andrea Rossi the world's greatest inventor since Nikola Tesla and the savior of mankind, or is he one of the worst scoundrels of the year? It's very difficult to say at this time, but the question really is that basic. There are those who would like to tread some middle ground on the topic, but there is no middle ground; it's either one way or the other. The mystery remains, and we have no way of knowing for sure which is the truth. The good news is that, given a little bit of time and patience, the answer to this question will be clear. Meanwhile this is indeed such an incredibly fun and interesting story to watch unfold." I am quoting John Ratcliff, the author of an online article "Andera Rossi: Sinner or Saint?," published on January 21, 2012. <www.examiner.com>.

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25) What Is Next?

The process of sharing what I know and think about CMNS will consist of three steps. The first step was to address mainstream scientists. This has been accomplished in a short Letter To The Editor of "Problems In Physics", published in January 2012 (see the link in Chapter 22). The second step was to address generally-educated people; this book is written for them. The third step will be to address philosophers of science, at a congerss in Montreal (June 2012). My paper, entitled "Cold Fusion 23 Years Later: Social And Philosophical Aspects Of That Controversy," has been accepted. What follows are excerpts from slides I am now preparing.

Excerpt 1:

(a) Mathematics, in my opinion, is much closer to theology than to science.

(b) Validation of claims in theology is based on acceptance of initial axioms (self-evident truth) and on logical consistency. The only way to justify the rejection of a claim (either in theology or in mathematics) is to find a logical error in the derivation.

(c) In science--both physical and social--claims are not based on logic only; in the final analysis they are based on experimental data. Mathematics is formalized logic; it is not science.

Excerpt 2:

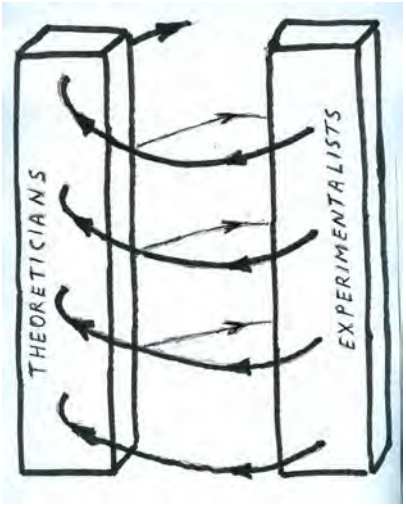
Reproducibility on demand is an important scientific requirement. **But suppose a reproducible result conflicts with an existing theory. What should be rejected, the experimental result or the theory?**

Excerpt 3:

Basic scientific assumptions, derived from philosophy, are: **(a)** reality is objective, **(b)** humans have the capacity to perceive reality accurately, and **(c)** rational explanations of phenomena in our material world are useful.

Excerpt 4:

"Theoreticians guide while experimentalists decide? Yes, but theories are based on verified results from experiments and observations. **The chicken and the egg dilemma? Not really.** Why not? Because the process of accumulation of scientific knowledge is not circular; it is spiral, as illustrated below:



Excerpt 5:

In 1942 Robert Merton described **CUDOS,** the prevailing Norms of Science. In this acronym,
C is for communalism (discoveries are not private property, they belong to all scientists),

U is for universalism (principles of validation of claims are universal, not subject-specific),
D is for disinterestedness (primary motivation for scientists is not money; it is love of truth), and
OS is for organized scrutiny (skepticism is very useful).

Excerpt 6:

A leading CMNS researcher Edmund Storms once asked this question: "**Which is the greater threat to science and mankind, accepting a claim that can have no possible benefit or rejecting a claim that can have great benefit?**

This question was addressed to editors of scientific papers who often deprive CMNS researchers of the peer review process.

Excerpt 7:

(a) Why are scientific investigations usually more effective than investigations in any other field? This is due to the so-called **"scientific method,"** a set of rules developed to deal with difficulties, especially with mistakes and controversies.

(b) Most scientific mistakes are recognized when new results are discussed with colleagues, or via **the peer review process**.

(c) Depriving PhD-level scientists of the peer review process is **a crime against science**.

This is the end of the book.
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ABOUT THE AUTHOR

Ludwik Kowalski, born in 1931 in Warsaw, Poland, became a nuclear physicist. The link below will lead to additional details about his unusual life, in the USSR, Poland, France and the USA. **CLICK to see additional details**

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404) Cold Fusion Is Not Like Hot Fusion

Ludwik Kowalski

April 4, 2012

As most of visitors to this Cold Fusion web site know, I occasionally report voices from a private discussion list for CF researchers. Let me do this again. Instead of revealing names To begin with I will refer to authors as M1, M2, M3, etc., where M stands for "Message." Later I will ask authors for names or initial, to be used, if they prefer. My contributions will be identified by LK.

M1 (in another thread) :

... I would instead suggest the following procedure:

1. Imagine a mechanism.
2. Is that mechanism internally consistent?
3. Is that mechanism compatible with what is generally known in physics and chemistry?
4. Does some kind of special structure (Nuclear Active Environment--NAE) exist, where this mechanism can produce detectable amount of heat and helium? (As a first approximation ignore transmutation, energetic particles and other less prominent features.)

If the answer to any of the questions 2-4 is negative, return to step 1) and start again; otherwise proceed further.

LK:

That sequence makes sense. Fleischmann and Pons (F&P) had experimental data and the mechanism they imagined was the D-D fusion. They even reported emission of neutrons, probably thinking about the He3+n decays of compound nuclei. But presence of neutrons was not confirmed by later investigators. Premature interpretations of experimental data backfired. Referring to this episode, in the article I am preparing, I write:

"Fleischmann and Pons made a devastating mistake in 1989. They announced experimental facts and they speculated about interpretation. They had no evidence that the claimed excess heat was due to a nuclear reaction. The only thing they knew was that it could not be attributed to a known chemical reaction.

Suppose that the experimental facts had been described without any interpretation, and the phenomenon had been named 'anomalous electrolysis.' Such a report would not have led to a press conference; it would have been made in the form of an ordinary peer review publication. Only electrochemists would have been aware of the claim; they would have tried to either confirm or refute it.

The issue of 'how to explain excess heat' would have been addressed later, after the reported phenomenon were confirmed. But that is not what happened. Instead of focusing on experimental data most critics focused on the disagreements with the theory. Interpretational mistakes were quickly recognized and this contributed to the premature skepticism toward their experimental data."

M2:

I agree Ludwik, this would have been the better approach, except for one fact - the involvement of Steven Jones. Jones was actually studying hot fusion but both he and F-P imagined they both were seeing the same process. In fact, F-P were not seeing hot fusion but an entirely new process. But, they both wanted credit. This confusion between hot fusion and LENR, which continues even today, is the source of all the problems.

M3:

Ludwik, I would rephrase this statement a little to be consistent with my experience (and recollection). In 1989 there was probably nobody better qualified in the world by intellect and experience than Martin Fleischmann to know that what he and Stan had seen was not "cold hot fusion". He knew this more certainly than any man. The idea that deuterium involved itself in some form of fusion process to produce what we now call the Fleischmann Pons Heat Effect (FPHE) was certainly worthy of speculation and the original paper used the word fusion with a question mark (Fusion?) which was later stripped off. Neutrons were confirmed multiply by later investigators (Menlove, Wolf, Gozzi, Scaramuzzi, many others) but at levels between 8 and 10 orders of magnitude down on expected hot fusion rates. So it is not "cold hot fusion" - but nobody serious ever believed that it was. This is a straw man argument used maliciously to discredit F&P and the field.

The only "faults" I lay at Martin's door are:

1) His implication that repeating the effect would be "easy".
2) His exaggerated faith in the talent of the average electrochemist and belief that a well trained physicist could somehow pick up this skill.
3) His belief that his mode of calorimetry would/should be easily understandable by all.

I am not sure what you mean by "Premature interpretations of experimental data". Without theory all our discussion is premature, but we have that right and so did Martin - particularly since he had done a number of experiments. I agree with Ed that the prematurity had a lot to do with the U of U *vs*. BYU rivalry. I do not agree, however, that anything Martin and Stan did or did not do could possibly have changed the reaction of the more vocal and hostile parts of the physics community. If we were to wipe away everything in the last 23 years except the specialized knowledge of those few of us who have done actual experiments or calculation, and then hire the "best" PR firm to help us release this discovery to the public, the outcome would be essentially the same today. It is simply too important.

LK:

1) I have no doubt that Fleischmann was one of the best electochemists in the world. This is confirmed by several formal recognitions he received.

2) Mike wrote "the original paper used the word fusion with a question mark (Fusion?) which was later stripped off. That does not convince me that M@F were originally not thinking in terms of the compound nuclei resulting from fusion of individual D ions. On the other hand, Fleischmann's paper at ICCF10 (last paragraph on page 1) does refer to "many-body effects." Hot fusion, on the other hand, is known to be a two-body reaction. In 2003 he disassociates himself with the incorrect interpretation, used by Huizenga, and other opponents.

3) What other old evidence do we have (beside the later-removed question mark) that F&P were already thinking about the "many-body effects" in 1989? I have no reason to think that what he said in 2003, about their thinking in 1989, was a lie. I am asking about an old document in which the term "many-body effects," or something similar, was used.

4) In their 1989 position I would be loudly emphasizing that the CMNS heat has nothing in common with simple two-body collisions of individual ions, if that were my way of thinking.

M4:

Not necessary. This was obvious - to everyone - even to me after I spent a few minutes thinking about it and

considering the conservation laws. A hideous misconception/misdirection lingers to this day because "fusion" means something very specific to one (tiny) group of people (but the very group whose territory was/is threatened) and something quite general to others. This misconception has been used cynically by some - and carelessly by others. I only speak up because it is time to stop feeding the trolls.

LK:

1) Rejections of experimental data on the basis of a disagreements with models developed to explain something else makes no sense. But it became very common since 1989. Unfortunately, Fleischmann did not ridicule this in 1989, as far as I know.

2) It would probably be useful to have a documented description of the "cold-fision-is-not hot fusion" idea, between 1989 and 2003, when Fleischmann used the term "many body effects." Was this term, or something equivalent used before 2003, either by F&P or anybody else. Please share what you know. I would be happy to write a web page (at my CF web site) on this important topic. Feel free to contribute anonymously, if you prefer. But be specific (proving references, dates and quotations).

4) Rejections of experimental data on the basis of a disagreements with models developed to explain something else makes no sense. But it became very common since 1989. Unfortunately, Fleischmann did not ridicule this in 1989, as far as I know.

5) It would probably be useful to have a documented description of the "cold-fision-is-not hot fusion" idea, between 1989 and 2003, when Fleischmann used the term "many body effects." Was this term, or something equivalent used before 2003, either by F&P or anyone else. Please share what you know. I would be happy to write a web page (at my CF web site) on this important topic. Feel free to contribute anonymously, if you prefer. But be specific, providing references, dates and quotations. Even old e-mail messages are worth sharing. I am addressing all of you, not only M4. Future generations will appreciate our desire to record the CF events correctly.

M5
I have been reading all the papers describing the field over the years and have reached a basic conclusion. Hot fusion and LENR are two entirely different processes having different mechanisms and NAE. A basic problem present early in the field's history, and even now, is that the detected radiation is not properly assigned to these two different processes, which can occur at the same time under some conditions. Jones used the conditions expected to produce hot fusion by fractofusion and detected neutrons having an energy typical of hot fusion. F-P tried and failed to detect neutrons as did dozens of efforts that followed. Neutrons are not produced by LENR and when they are, tritium appears to be produced as well. I will make a paper available soon that explains these conclusions very clearly. Meanwhile, you need to consider the possibility that people have been seeing two entirely different kinds of nuclear reactions, one related to hot fusion and the other related to LENR. To be clear, LENR involves fusion, but that is caused by a different mechanism.

M6:

Well stated M5! Trying to fit into your adversary's pidgeon hole was counterproductive.

LK:

On Apr 5, 2012, at 11:02 AM, M5 wrote: I have been reading all the papers describing the field over the years and have reached a basic conclusion. Hot fusion and LENR are two entirely different processes having different mechanisms and NAE.

THAT IS UNDENIABLE, CONSIDERING WHAT HAS BEEN REPORTED

A basic problem present early in the field's history, and even now, is that the detected radiation is not properly assigned to these two different processes, which can occur at the same time under some conditions.

I AGREE; THE EARLY HISTORY IS NOT CLEAR TO MANY. THAT IS WHY IT SHOULD BE BETTER DOCUMENTED.

1) M4 WROTE: THAT THE CF=HF "MISCONCEPTION HAS BEEN USED CYNICALLY BY SOME --AND CARELESSLY BY OTHERS." WHO WERE CYNICS AND WHAT EVIDENCE DO WE HAVE THAT THEY DELIBERATELY SPREAD THE CONFUSION? A REASONABLE SUSPICION BASED ON THE CONFLICT OF INTERESTS IS NOT SUFFICIENT.

2) ANOTHER PART OF OUR HISTORY, WORTH DOCUMENTING--I AM THINKING ABOUT ED'S NEXT ARTICLE--HAS TO DO WITH THE MULTI-BODY MECHANISM. WHAT ADDITIONAL EVIDENCE DO WE HAVE THAT FLEISCHMANN'S PATH TOWARD THE 1989 EXPERIMENTS WAS VIA THEORETICAL QED CONSIDERATIONS, AS HE CLAIMED IN 2003? WHEN DID HE FIRST SAY THAT CF IS A MULTI-BODY PROCESS AND NOT THE TWO-BODY PROCESS, SIMILAR TO HF?

3) MY EXAMINATION OF THE TWO DOE INVESTIGATIONS OF THE CMNS FIELD, IN:

(L. Kowalski, " Cold Fusion: Reality or Fiction," Progress in Physics, April 2012, L17-L19.

SEE ONLINE: http://www.ptep-online.com/index_files/2012/PP-29-L2.PDF)

CAN ALSO BE ENRICHED BY THOSE WHO WERE PERSONALLY INVOLVED. MY EMPHASIS WAS ON DISAGREEMENTS WITH THE SCIENTIFIC METHODOLOGY OF DEALING WITH CLAIMS. BUT I SUSPECT CYNICISM AS WELL, ESPECIALLY DURING THE SECOND INVESTIGATION, WHEN MORE EVIDENCE WAS AVAILABLE.

Jones used the conditions expected to produce hot fusion by fractofusion and detected neutrons having an energy typical of hot fusion. F-P tried and failed to detect neutrons as did dozens of efforts that followed. Neutrons are not produced by LENR and when they are, tritium appears to be produced as well. I will make a paper available soon that explains these conclusions very clearly. Meanwhile, you need to consider the possibility that people have been seeing two entirely different kinds of nuclear reactions, one related to hot fusion and the other related to LENR. To be clear, LENR involves fusion, but that is caused by a different mechanism.

THAT IS NOW CLEAR TO MOST OF US. BUT IT WAS NOT CLEAR TO ME IN 1989. F&P EMPHASIZED VERY HIGH PRESSURE OF IONS INSIDE PALLADIUM. THAT IS WHY I WAS THINKING ABOUT HIGH PRESSURE PLASMA (INSTEAD CONDENSED MATTER). [I DID NOT REALISE THAT THE TERM "PRESSURE" WAS USED METAPHORICALLY. THEY HAD IN MIND THE "EQUIVALENT PRESSURE," NEEDED TO REDUCE THE AVERAGE DISTANCE BETWEEN THE IONS TO ABOUT $1.5*10^{-8}$ CM. WERE I THE ONLY ONE TO BE CONFUSED BY THE TERM PRESSURE? IN ANY CASE, OUR EVOLUTION TOWARD THE IDEA OF MORE THAN TWO DIFFERENT MECHANISMS (VIA PUBLISHED PAPERS) IS ALSO WORTH DOCUMENTING.

Abd ul-Rahman Lomax:

From 1994, Take a look at

Chechin, V.A., et al., Critical review of theoretical models for anomalous effects in deuterated metals. Int. J. Theo. Phys., 1994. 33: p. 617. Coauthors: Tsarev, V. A., Rabinowitz, M., Kim, Y. E.

http://lenr-canr.org/acrobat/ChechinVAcriticalre.pdf

M8

This quote from Fleischmann and Pons 1989 paper still seems insightful. I have added the bold. "It is known that at potentials negative to + 50 mV on the reversible hydrogen scale, the lattice is in the beta-phase, **hydrogen is in the form of protons** (as shown by the migration in an electric field) and is highly mobile (D = 10-7 cm2 s-1 for the ?-phase at 300 K). The overall reaction path of D2 evolution consists of steps (i) and (ii) [2] so that the chemical potential of dissolved D+ is normally determined by the relative rates of these two steps. The establishment of negative overpotentials on the outgoing interface of palladium membrane electrodes for hydrogen discharge at the ingoing interface [3] [determined by the balance of all the steps (i) to (iv)] demonstrates that the chemical potential can be raised to high values. Our own experiments with palladium diffusion tubes indicate that values as high as 0.8 eV can be achieved readily [4] (values as high as 2 eV may be achievable). The astronomical magnitude of this value can be appreciated readily: attempts to attain this level via the compression of **D2 [step (iv)] would require pressures in excess of 10E26 atm. In spite of this high compression, D2 is not formed; i.e. the s-character of the electron density around the nuclei is very low and the electrons form part of the band structure of the overall system.** A feature which is of special interest and which prompted the present investigation, is the very high H/D separation factor for absorbed hydrogen and deuterium (see Figs. 4 and 6 of ref. 2). This can be explained only if the H+ and D+ in the lattice behave as classical oscillators **(possibly as delocalised species) i.e. they must be in very shallow potential wells.** In view of the very high compression and mobility of the dissolved species there must therefore be a significant number of close collisions and one can pose the question: would nuclear fusion of D+ such as" Fleischmann, M., S. Pons, and M. Hawkins, Electrochemically induced nuclear fusion of deuterium. J. Electroanal. Chem., 1989. 261: p. 301 and errata in Vol. 263.

LK:

Thank you, M8; the link to the entire paper is:

http://www.lenr-canr.org/acrobat/Fleischmanelectroche.pdf

Note that reactions V and VI describe traditional thermonuclear collisions resulting in D+D fusion. Were F&P aware that inclusion of these two reactions (on the opening page of their first publication) was an open invitation for a very confusing situation--thinking that CF is like HF? I do not think that they were aware of this. The main source of confusion was their paper, in my opinion.

Abd ul-Rahman Lomax:

At 10:02 AM 4/5/2012, Edmund Storms wrote: I have been reading all the papers describing the field over the years and have reached a basic conclusion. Hot fusion and LENR are two entirely different processes having different mechanisms and NAE.

This is highly likely, as far as it goes. I'll point out that there is no particular reason to believe that there is only one LENR, and some good reasons to suspect that there is more than one. Trying to stuff PdD and NiH and biological transmutation results into the same small box may be a fool's errand.

We don't know yet how any of these work, and we have only hints, proto-theories, ideas. Few serious confirmations. Helium was predicted by Preparata. So that's one for Preparata. Helium is confirmed as the primary ash, there isn't any serious competitor. That was major progress, as Huizenga noted in 1993 or so. What do we have since then? Not a whole lot!

A basic problem present early in the field's history, and even now, is that the detected radiation is not properly assigned to these two different processes, which can occur at the same time under some conditions. I think this might not be clear. If we have cold fusion, releasing, say, 23.8 MeV/He-4 in some way, and even if the main reaction produces no detectable radiation, it might "leak" a bit. As a speculative example, suppose BECs in which Be-8 are created by fusion are the normal mechanism, and suppose that these ordinarily release their energy, relatively slowly, through a series of phonon emissions (I think that's more or less Hagelstein's idea, or similar to it, but I haven't studied it). What if something disturbs that BEC while it's doing its business? We might get some hot alphas, which can then cause secondary reactions, including hot fusion. Suppose the BEC bumps into a nucleus before decaying? We might

see some transmutations, Z+2. (The BEC is presumably neutral, so there is no Coulomb barrier.)

My point here is not the specific idea of a possible theory, but only that if fusion is happening, there could be some consequences resulting in hot fusion. Cold fusion can cause hot fusion, to some degree.

Jones used the conditions expected to produce hot fusion by fractofusion and detected neutrons having an energy typical of hot fusion. F-P tried and failed to detect neutrons as did dozens of efforts that followed. Neutrons are not produced by LENR and when they are, tritium appears to be produced as well.

I think this is correct, if we add the word "normally" before "not produced." If neutrons are produced, they are produced at levels below even that of tritium, which is produced at levels way, way below helium. Helium has been well-correlated with excess heat. Neutrons and tritium, largely not.

F-P did report low levels of neutrons, a famous error. Because of that error, they mentioned the hot fusion reactions in their initial paper. That confused the hell out of everyone. They did not help matters by posing their work as an answer to the question of whether or not classical fusion reactions could occur in the dense environment of PdD. So their comments, "It is evident that reactions (v) and (vi) are only a small part of the overall reaction scheme and that other nuclear processes must be involved," and "that reactions (v) and (vi) are only a small part of the overall reaction scheme and that the bulk of the energy release is due to an hitherto unknown nuclear process or processes (presumably again due to deuterons)," were largely ignored.

That was the error of Pons and Fleischmann. The error of the general physics community was in rejecting standing experimental evidence based on only theoretical arguments. It does appear that Pons and Fleischmann did their work as an attempt to confirm standing theory, based on approximations, that fusion could not occur even in an environment like PdD, so what was being rejected was experimental evidence from work designed to test the very theory that was used to reject it.

That was, indeed, a "scientific fiasco," transcending the mere errors of a few individuals.

I will make a paper available soon that explains these conclusions very clearly. Meanwhile, you need to consider the possibility that people have been seeing two entirely different kinds of nuclear reactions, one related to hot fusion and the other related to LENR. To be clear, LENR involves fusion, but that is caused by a different mechanism.

This is, by far, the most likely state of affairs. There is still some remote possibility that classical hot fusion is taking place in PdD, and that an entirely unanticipated mechanism is producing a different set of results. This requires the famous "triple miracle," though. I don't think so. While it's possible we have two miracles here, more likely there is just one. I.e., something that was not anticipated. It's extremely difficult to investigate the FPHE, in terms of trying to understand what's going on. It's hidden from our view, at least some of it. With plasma physics, it's all naked. I fully understand why physicists might not want to get their hands dirty with this!

An electrochemical cell is a true mess, the cathode attracts all kinds of gorp. The FPHE was erratic and would appear and disappear mysteriously, in the very same cell, under apparently the same conditions. However, physicists, from this, assumed that the effect must be near the noise. That was a failure to pay attention. It was not near the noise, it was far above it. Once you saw this fabulous beast, you became a believer. It wasn't a vague ghost spotted at night under poor conditions. But few exercised themselves to be able to see it. As soon as a few replication failures were reported, most gave up.

And we don't like to think that we failed at something. I'm learning to recognize that admission of failure is the key to progress, in fact.... Those who never fail aren't trying hard enough....

LK:

Thank you for the link, Abd ul-Rahman Lomax

http://en.wikiversity.org/wiki/Cold_fusion/Excess_heat_correlated_with_helium/Sources

I did not know that Huizenga commented on CF in the second edition of his book. Let me preserve what you reported and commented about.

“Huizenga, John R., [in his book] “Cold fusion, scientific fiasco of the century,” 2003, 2nd edition, [wrote]:

The invited paper by Miles, Bush, et al, made the most spectacular claim at the [1991] conference. It was reported that, The amount of helium (^{4}He) detected correlated approximately with the amount of excess heat and was within an order of magnitude of the theoretical estimate of helium production based upon fusion of deuterium to for ^{4}He.

This claim has been published elsewhere [cited, J. Electroanal. Chem] and I have commented on it previously (see p.136 and 212). If it were true that ^{4}He was produced from room-temperature fusion in amounts nearly commensurate with excess heat, one of the great puzzles of cold fusion would be solved! However, as is the case with so many cold fusion claims, this one is unsubstantiated and conflicts with other well-established experimental findings. [p. 243-244]

Old comment by Abd ul-Rahman Lomax: Huizenga's "well-established experimental findings" conflict, not with a finding of helium, but with the assumption that a heat/helium correlation must result from deuterium-deuterium fusion. Another, previously unknown, process that produces helium from deuterium would not conflict with those findings. Huizenga notes the "failure" of Miles to detect ^{3}He: --Abd 23:20, 26 September 2010 (UTC)

"hence, it is highly likely that the 4He is a contaminant from the atmosphere. In addition, if ^{4}He is produced in the amount claimed, it must be accompanied by large intensities (in fact lethal doses) of the associated 23.8 MeV gamma ray. Only when the 23.8 MeV gamma rays are observed on-line, can one be sure that the ^{4}He is produced by fusion and is not an experimental artifact. Finally, the 23.8 MeV gamma ray transfers essentially all the d+d -> ^{4}He + gamma reaction energy outside the cell and destroys the relationship between the helium production and the excess heat based on the assumption that all the reaction energy stays inside the cell. More recently, Miles, Bush et al reported that they can produce neither excess power nor ^{4}He from their electrolysis experiments.

... The editor of the Journal of Electroanalytical Chemistry thought the new claim was so exciting and so important that he accepted the author's manuscript without peer review. It is inconceivable that such an editorial decision could be made at this late date in the cold fusion saga. Due to the many ways that one can get spurious results on 4He in the gas phase, one would have expected the authors to have at least obtained confirmatory evidence that they had observed the 23.8 MeV gamma ray from the reaction D+D -> ^{4}He + gamma ray. This they did not do! Instead, the authors chose to repeat the mistake of most cold fusion proponents. After obtaining fragmentary evidence, the authors went to the press before performing the necessary checks and obtaining confirmatory experimental evidence. The validity of the Bush et al. claim requires three miracles: namely the fusion-rate, branching-ratio and concealed nuclear products miracles!

Present Comment by LK: This is a very good 1993 argument that the CMNS fusion, associated with production of helium, cannot possibly be the same reaction as the corresponding thermonuclear collision of two ions. If it were then the 23.8 MeV energy would not appear as heat released in the electrolytic cell. Huizenga used the argument to conclude that CF does not exist, rather than “cold fusion is not like hot fusion.” Why was this argument not used by F&P, in their first paper? When was it used for the first time and by whom?

M10

This is a good observation and question, Ludwik. Although many people may have realized that LENR was not the same as hot fusion, the question of the difference is a matter of degree. Many the theories focused on how much energy is required to cause hot fusion and on getting the deuterons closer together. No one acknowledged the need for a NAE, an idea that was rejected with great enthusiasm initially and to a lesser extent even now. People looked for neutrons and tritium, as would be expected to result from hot fusion. Failure to detected neutrons was explained by

failure to initiate LENR in the device being studied, not by realizing that no neutrons were produced by LENR. Even now, when a few neutrons are found, they are used to "explain" LENR. My opinion is that the realization that LENR is not related to hot fusion came gradually and is still not complete in some minds.

M11:

Julian Schwinger was a far better physicist than Huizenga. Julian was good. This, what I would call superficial debate (since, after all, only the experimental results are or were relevant at that time) was quite common at least for a few days, weeks or months after March 23. At that time I did not know what "must" or "must not" be true about nuclear processes. But I was completely comforted (to the point that I did not worry about it any more) by Schwinger's statement that concluded "the circumstances of cold fusion are not those of hot fusion". Huizenga's "must" [as quoted by LK] is a huge red flag. This is a political statement, not scientific. Huizenga claims omniscience?? He knows every possible way that 4He can be produced? It is the conservation of angular momentum that requires the energetic gamma ray from to isolated D's (which, by the way is not 23.8 MeV so even in detail he was wrong). What if 3, 4 or n bodies are involved? Huizenga's statement is worse than silly.....it is PR.

Abd ul-Rahman Lomax:

The confusion [cold fusion is like hot fusion] was easy, and might have happened anyway. That F&P paper was written quickly, and I doubt that the authors anticipated what ensued.

And no matter what mistakes they made, we don't depend on the character or flaws or mistakes of individuals in determining what is established in science.

Basically, they were brilliant, testing this wild idea, doing basic research. Then they enountered social forces beyond what they imagined. The University required them to announce, they clearly were not ready. We can look back and say that they did it wrong, they should have done this or that, but hindsight is always clearer than foresight.

What they found, if not for their diligent search, undertaken with their own energies, largely without support (for the first five years), might have escaped notice for a long time.

This was extremely difficult work -- that was one of the errors, making it look or sound easy. "Fusion in a jar." Right. It took the world's foremost electrochemist five years to pull this up to a rate of about 15% of cells showing excess heat, it took other electrochemists months of effort to start to see results, and a few scientists, with utterly inadequate information, tried for a few weeks, saw nothing, and started to claim that Pons and Fleischmann were frauds or idiots. And so it went.

It's really a shame that scientific politics inhibited research in this field for so long. It still has not recovered.

This "scientific fiasco of the twentieth century" (Huizenga's book title) carries some deep lessons about the hubris of scientists, where inadequately established conclusions become an orthodoxy, challenged only at personal risk. I met a skeptic who was afraid to let anyone know that he would even discuss cold fusion, he considered that he would suffer professionally.

I have a friend who is a mathematician, and when he found out that I was involved in CF research, he assumed I was being duped by con artists, and he proceeded to act to exclude me from an organization I'd helped to found -- since I was obviously dangerously crazy. The guy is actually brilliant. But absolutely certain that he's right, and unwilling to check out contrary evidence.

All he could see was Rossi (and worse, in his imagination).

M13:

Schwinger proposed H+D fusion would make He3. Unfortunately, this was not found. Instead, He4 was found with a

little tritium. This kept focus on the products from hot fusion. As for Huizenga, I agree with Mike, he was tying to eliminate CF for various reasons, as his book made clear. We once again have witnessed the ability of several leaders to create a myth in lesser minds for the purpose of promoting their self-interest. We see this happen all the time in politics, especially in the Republican debates. These leaders are not stupid. They knew that if CF were real, hot fusion and every other energy source would be at risk. They were able to stop the discovery process for awhile, which I'm sure they hoped would have been much longer. Now they have another problem not so easy to solve.

Abd ul-Rahman Lomax

... Look, my opinion is that cold fusion has been clumsily presented, even up to the present day. Look at the reviews of the field on that Wikiversitiy page you cited, and how they treat heat/helium. It's almost as if it is a footnote. And, in fact, much of the heat/helium evidence presented in the report by Hagelstein et al in 2004 to the DoE was in an appendix. It took me many readings of that appendix to understand what they were getting at. And I had to scour through other work to make heads or tails of it. I recognized very quickly that not all the necessary information was explicitly stated. The negative reviewer and the summarizing bureaucrat made an obvious -- and wrong -- assumption.

That was a result of ineffective communication. I'm suspecting that this comes from a habit of writing for scientific publication where the tone is very abstract and avoids bald assertion. Rather, the reader is supposed to figure it out. This requires very active, sympathetic readers. It's not a way to reach those who might be on the fence, not to mention hostile. Hagelstein et al are scientists, not writers of polemic. Nor would I want them to be anything other than what they are.

If there is another DoE review, the documents should be rigorously reviewed and read from a skeptical point of view, to discover what skeptical objections are possible, to anticipate them and answer them. Doing this without turning a document into a tome is not simple, it takes rare skill, and may take the work of more than one person. ...

Abd ul-Rahman Lomax:

Well, I suspect that Huizenga was, by that time, not thinking very clearly. He was making an obvious assumption, and appears not to have realized it.

I actually find the whole passage remarkable, because Huizenga does realize the import of Miles' work, to his credit. He didn't flap on about leakage, etc. He knew that correlation would cut through that noise.

A whole series of assumptions were made, each one being more or less reasonable or at least understandable. For example, multibody fusion is mentioned. At the time, it was not known that multibody fusion (hot fusion!) is greatly enhanced in PdD targets. I don't recall when Takahashi published that work, it might not have been available to Huizenga. The normal skeptical response to the idea of multibody fusion is that it would be rare upon rare, which follows from thinking in terms of collisions in a plasma.

People drew conclusions from what they were familiar with. Not surprising.

LK:

My recollection is that Pd targets, loaded with deuterium, were often bombarded with monoenergetic deuterons (in 1960s) to produce monoenergetic neutrons at varius angles via hot fusion. I was not aware of the “greatly enhanced” observations. That would indeed mean that cold fusion was discovered long before 1989. It is tempting to speculate about an accidental discovery of excess heat in a ready-to-use target. This would be like production of unexplained heat in radium, discovered by Pierre Curie.

M16:

Great enhancement only occurs when the bombarding D+ have low energy, where the neutron production rate is already very small. As a result, the term "great enhancement" has to be applied to a very small initial production rate.

No LENR is involved.

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Abd ul-Rahman Lomax:

At 11:32 AM 4/7/2012, Ludwik Kowalski wrote:
My recollection is that Pd targets, loaded with deuterium, were often bombarded with monoenergetic deuterons (in 1960s) to produce monoenergetic neutrons at varius angles via hot fusion. I was not aware of the “greatly enhanced” observations. That would indeed mean that cold fusion was discovered long before 1989.

I am not aware of such "greatly enhanced" results, and Ludwik may have misinterpreted what I wrote. I did not claim any observation reported before 1989, and, in fact, I was speculating that the multibody results were not available until after Huizenga wrote his statement.

I had a recollection that Takahashi's work on multibody fusion rates was fairly early, but what I could find at this point was this:

http://newenergytimes.com/v2/archives/fic/N/N199811.PDF page 5, a review of Takahahsi's 1998 publication in Fusion Technology.

In http://iccf9.global.tsinghua.edu.cn/LENR%20home%20page/acrobat/TakahashiAstudiesond.pdf, the earliest reference is to
A. Takahashi, et al: Fusion Technology, Vol.27 (1995) pp.71-85.

Since 1991, we have studied possible occurrence of highly enhanced multi-body deuteron fusion in metal-deuteride samples under low energy deuteron beam irradiation, based on our speculation that the order of atoms and electrons under transient motion around lattice focal points should greatly enhance the three-body fusion process, compared with random nuclear reactions.1-3
Our experimental results suggest that there exists strong screening effect on the Coulomb repulsive force of d-d interaction by transient “electronic quasi-particles”5, 6 and this greatly enhances 2D and 3D fusion reactions, and even 4D fusion reactions.

However, "multibody fusion" is mentioned in the title of a 1992 Paper by Takahashi, "Excess heat and nuclear products by D2O/Pd electrolysis and multibody fusion." From the Dieter Britz abstract of this paper:

The authors present their theory to explain the dearth of neutrons. At high
loadings, 3-body and 4-body fusions might take place, some producing no
neutrons or tritons, but alpha particles instead.}

Any review of the history of cold fusion theory should look at a paper I cited a few days ago:
http://lenr-canr.org/acrobat/ChechinVAcriticalre.pdf

Critical Review of Theoretical Models for Anomalous Effects (Cold Fusion) in Deuterated Metals
V.A. Chechin1, V.A.Tsarev1, M. Rabinowitz2, and Y.E. Kim3. Int. J. Theo. Phys., 1994. 33: p. 617.

4.5 Multibody Fusion
Four independent conjectures (Becker, 1989; Rabinowitz, 1990a; Kim, 1990c; and Takahashi, 1991) have been made suggesting the possibility of multibody fusion. Rabinowitz pointed out that although the probability for a three-body collision in free space is extremely smaller than a two-body collision -- in a solid, "Channeling increases the probability of a nearly one-dimensional collision, with essentially the absence of angular momentum in the final state. This may permit low energy resonances which greatly increase the fusion cross-section -- particularly for energy and

momentum conserving three-body collisions."

The references are:

Becker, E.W. (1989). Naturwissenschaften 76, 214.
Rabinowitz, M. (1990a). Mod.Phys. Lett. B4, 233.
Kim, Y.E. (1990c). AIP Conf. Proc. (Provo, Utah) 228, 807.
Takahashi, A. (1991). Fusion Technology 19, 380.

This "critical review" states, in the abstract,

we critically examine more than 25 theoretical models for CF, including unusual nuclear and exotic chemical hypotheses. We conclude that they do not explain the data.

and the last words of the paper's conclusion are:

It is an understatement to say that the theoretical situation is turbid. We conclude that the mechanism for anomalous effects in deuterated metals is still unknown. At present there is no single consistent theory that predicts or even explains CF and its specific features from first principles.

This situation appears to remain true. What theories we have that are plausible (the Chenin paper considers some of the theories they present as implausible), may explain this or that aspect of cold fusion, but no theory is complete, as published so far. However, given this, the Chechin paper is remarkably sanguine on multibody fusion:

There are two main consequences of this hypothesis. 1) In multibody fusion reactions, the released energy is carried by some particles that cannot escape the solid. So this energy heats up the lattice without the emission of visible nuclear products. 2). In reaction (46), energetic protons are released. In reactions (42,43,44, 46, and 48), energetic alphas (4He) are released which in turn can produce energetic n's and p's via the process 4He + d Ø 4He + p + n. This might explain the high energy components that have been seen (Chambers et al, 1990; Cecil et al, 1990; Takahashi et al,1990, 1992). Takahashi (1991) has suggested a number of multibody fusion scenarios in a solid to answer all the CF enigmas.

(Hindsight comment: it appears that energetic products at significant levels, with energies above 20 KeV, can be ruled out in the primary mechanism behind the FPHE, if Hagelstein's limit is valid. This, together with a *specific* predicted rate of fusion, remains the greatest difficulty remaining with multibody fusion theory, as far as I can tell.)

Critique: If one proceeds from habitual thinking drawn from conventional nuclear and plasma physics, the idea of multibody fusion appears wild to say the least. However, one has to take into account that the situation with CF is drastically different due to the presence in the solid of both periodically ordered positions for embedded d's and preferred directions for the motion of nuclei. This makes multibody collisions in principle more likely than in free space. However, no firm calculations have yet been presented which are testable. Quite independent of CF, interesting and unusual results have been obtained in channeling experiments of energetic particles in solids (Sorenson and Uggerhoj, 1987).

So "channeling effects" were known before 1989. Takahashi's experimental work as reported later shows, if his analysis is correct, an enhancement of 3D multibody fusion of 10^26 over naive expectation, if I've understood it correctly.

The reference: Sorenson, A.H. , and Uggerhoj, E. (1987). Nature 324, 311.

This does not mean that the deuteron bombardment that Takahashi used is relevant, in itself, to the mechanism behind cold fusion. Rather, the take-home point here is that condensed matter is experimentally known to be a different environment -- enormously different -- from a plasma, and that some reactions become possible or enhanced in condensed matter that are impossible, or ridiculously rare, in a plasma.

Huizenga would have been putting together the second edition of his book in 1993, and he reports 1993 publication by Miles. There was a flood of published work on cold fusion in 1989-1991; but Huizenga was writing a book on the topic that goes into great detail, when he wants to. In fact, certain details he repeats over and over. My own conclusion is that Huizenga wasn't really familiar with the work, he wasn't in "learning mode." He was just regurgitating his prior opinions. I notice that the only mention of Miles in this book was regarding the "most spectacular claim" presented at the Second Annual Conference. I.e., heat/helium.

Yet by the time Huizenga was writing his book -- even the first edition -- one might think he'd have known that Miles, whose preliminary negative results were shown in the 1899 ERAB report, later reported positive results, and even later, of course, reported the helium correlation that definitely got Huizenga's attention.

It is a common human practice to selectively observe evidence that confirms what we believe. We all need to recognize this, and guard against it. Huizenga interpreted the information coming to him, it's quite obvious, through his filters.

So, for example, Huizenga reports (2nd edition, page 243) in the same section on the Second Annual Conference,

[At the Conference] the substance of these claims was neither new nor different from that repeatedly proclaimed by believers over the past two years. ...The McKubre group of SRI reported excess heat of a few percent when integrated over the entire run. This was achieved in a closed isothermal flow calorimeter with high cathodic charging of deuterium (D/Pd > 0.9). The authors concluded that they cannot account for their excess heat by any chemical or mechanical process, and the inference is that the excess heat is due to a nuclear reaction. Although the McKubre experiment is considered by many advocates to be the premier evidence for excess heat, no nuclear reaction products were reported!

Right. There are no nuclear reaction products from the FPHE, at significant levels, except helium. At that point, McKubre et al had not, I think, investigated helium, but Huizenga, if he'd been thinking synthetically, would have known that helium was a possibility. But he wasn't. He was looking for reasons to confirm his own beliefs. *He was a believer.* (One of the characteristics of pseudoskepticism is to attribute comment and evidence from others, that might appear to contradict the pseudoskeptic's own beliefs, to "believers," and "proponents.") What Huizenga fails to do here is to credit the experimental competence of McKubre et al, who were, after all, consultants hired by the Electric Power Research Institute to investigate cold fusion. He doesn't really look at what they were reporting, he simply extracts from it sufficient "fact" to allow him to quickly dismiss it. Only a "few percent"? Why, piffle! That could easily be experimental error, some undiscovered artifact.

Not when you look at how the work was conducted. The FPHE suffers from a procedural problem. The input energy is necessary to build and maintain high deuterium loading in palladium. Most of it has nothing to do with the reaction itself. What is significant about, say, SRI P13/P14, is the difference between (1) the deuterium cell vs the hydrogen cell, in series with it, and (2) three different runs, with the same current excursion, yet the third run shows drastically different results, very significant excess heat, with *the same cathode* -- as it would appear. Obviously, something is different. And that difference represents the story of cold fusion, a story that Huizenga didn't tell. Instead, he gives us this:

As of March 29, 1993, the Droege experiments with high deuterium loading (D/Pd>0.9) continue to report null results. Tom Droege has reported that "the cleaner I get, the better I control the experiment, the more accurate I make the apparatus; the fewer bumps and anomalies I see."

Of course! Droege, very obviously, was not seeing the FPHE. The same as with the first two runs of P14 at SRI. Huizenga, note, by this time clearly gets that loading ratio might be important. If he was paying attention, he'd have known that FPHE work on a short time scale was very unlikeliy to see excess heat, so he'd have known that almost all the "negative replications" reported in his ERAB report were doomed to failure. So why wasn't Droege seeing results? It's quite apparent that loading is not a sufficient condition to see the effect, because P14 at SRI was also highly loaded, in all three excursions, but only showed the effect in one of them.

Remarkably -- and we should remember this -- Huizenga actually points out the problem. "No nuclear product." He knows that helium is a nuclear product, and his only problem there is that he expects the helium to be accompanied by a gamma. Did Droege ever look for helium? With no excess heat, we can be pretty sure that, if he had, he wouldn't have seen helium. On the other hand, McKubre did, later, look for helium, and found it, correlated with the heat, confirming Miles.

Huizenga, following Droege, here created a common pseudoskeptical trope: that when experimental technique was refined, excess heat results disappeared. Yet Droege had not ever reported XP, apparently. He simply found that noise that might have been interpreted as "maybe" excess heat disappeared. It was a unsubstantiated assumption that the negative replications were using more careful calorimetry than that of, say, McKubre et al.

But they may have been! It's actually irrelevant. More careful calorimetry is useless if you don't actually set up the effect!

No, this would have been a true negative report: a would-be replicator shows results the *same* as in the original report (or varying in the same range), and then improves the calorimetry and shows that the apparent results were from error -- or other artifact. Basically, to do replication, one should attempt to reproduce the *errors* of the original experiment. Basically, do it the same way, or don't claim that it's a replication, especially not a negative one. Don't "improve" your experiment first!

Back to Ludwik:
It is tempting to speculate about an accidental discovery of excess heat in a ready-to-use target. This would be like production of unexplained heat in radium, discovered by Pierre Curie.

It happened. Mizuno. He reports it in Nuclear Transmutation: The Reality of Cold Fusion (trans. Jed Rothwell, 1998), page 34. It's unfortunate that Mizuno did not report his observations at the time. My guess is that others saw cold fusion effects, as well.

August, 1978: Mysterious evaporation of heavy water electrolyte from an electrochemical cell using a three-cm palladium disk as a cathode, saturated with deuterium, used as a neutron target. "The 200 cc should have lasted a month [but evaporated in a day]... only two possibilities: the electric current might have increased, rapidly electrolyzing all of the liquid, or a large amount of heat might have caused the fluid to boil away. But at the time we could not imagine either of those scenarios, so we finally wrote off the incident as a mystery with no solution. We did not understand it until many years later."

May 1981: Loading titanium with deuterium, "... we activated an X-ray detector adjacent to the cell. It suddenly began to register continuous bursts of X-rays. In a panic, we checked our clothese and belongings for radioactive contamination. We moved the detector around to find the source of the X-rays. We finally concluded that the cell must be the source. But at the time we never imagined electrolysis could produce X-rays, so after careful consideration we decided it must be some kind of electrical interference. It turned out the cell really was generating X-rays, but I did not realize that until later."

(He goes on to note that the idea of fusion in electrolysis cells did occur to him as a possibility, later, "this kind of hypothesis would occur to any researcher studying metal and hydrogen systems." But..... "I never thought to pursue the matter and research the phenomenon further.")

FOLLOW-UP.

I decided to look to see if Droege ever found any excess heat. He seems to disappear from the cold fusion scene by about 1991. But he's mentioned by Richard Garwin in a review of Mallove's book on CF. There is this remarkable passage:

`Tom Droege, a superb engineer who has built state-of-the-art instrumentation for the particle physicists at Fermilab,

now ... perfects an extraordinary calorimeter," Mallove reports. Indeed, and in the process Droege has identified and overcome many problems that must have afflicted less cautious workers. At present, with electrolytic power input of some 1000 milliwatts his sensitivity is about 1 milliwatt, with no dear indication of net excess heat. Those who claim to know how to treat their cathodes to obtain excess heat would do well to adopt Droege's apparatus.

He's really doing quite the same as Huizenga, assuming that negative work is more accurate than positive. As to cold fusion results that are close to the noise, sure. Some CF reports may be flawed and a result of errors in calorimetry. However, Garwin goes on:

if anyone can show me a history and demonstration of strong, reproducible, emission of neutrons, tritium, or heat in a cold (or dry) fusion cell, I will not only urge support but repeat the experiment.

Liar, liar, pants on fire. Of course, he adds a qualifying word, "strong." From the point of view of science, though, "strong" means that a signal is clearly above noise. This was written in 1999. Garwin surely knows by this time that FPHE cold fusion doesn't produce significant neutrons or tritium, just helium. Surely Garwin would have recognized the significance of helium, as did Huizenga, and by this time, Miles' helium results had been confirmed.

As to any sensible intepretation of Garwin's promise, heat that is produced in a specific ratio to helium detected is "reproducible." You could run this blind: detect helium and continue running the cell until helium generated reaches a certain value. How much excess heat has been generated? That is the reproducible demonstration of heat. Some cells don't work, and..... so what? No heat, no helium, that actually confirms the calorimetry and helium measurement techniques, if the cells are otherwise identical.

What was "reproducible" was the heat/helium ratio. Heat, by itself, remained not reproducible under most conditions. Indeed, I'm interested in the work with dual laser stimulation because it appears that those results are reliable. That would be, I speculate, because the cell is being run below normal CF loading levels, and the reaction only takes place under the influence of the stimulation. It's controlled.

(We can also make a weaker argument for reproducibility with certain designs; for example, Miles' cells seem to have roughly a two-thirds rate of showing excess power. Reproducibility can be statistical.)

And I'll add: the effects and results of dual laser stimulation have not been confirmed. My impression of reliability is only based on what Letts is reporting.

M18

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405) AmoTerra Process:

Destruction of Low Level Radioactive Waste?

Ludwik Kowalski; 5/6/2012

Department of Mathematical Sciences
Montclair State University, Montclair, NJ, USA

Nine years ago in Unit #186 click I wrote: "According to (2), a Canadian company is about ready to start destroying radioactive waste on an industrial scale. [Mrs. Anderson] said that the apparatus built for the company is able to destroy 50% of radium, whose half-life is known to be 1620 years, in only 3 days. It also changes thorium into lead. ... They plan to move to highly radioactive waste later. I said that I would like to come and observe the pyrolytic device in operation. They said that they would invite me, probably in several weeks. I hope this will happen; I would be happy to write a unit on the first industrial application of what is, according to Monti, a cold fusion transmutation process. Will they allow me to make some measurements? This remains to be seen." On 4/2/2012 I received the email message from Mrs. Anderson; she wrote:

"Dear Ludwik: A long time has passed since you last heard from us and much has happened. With a heavy heart, I am informing you that Ernst, my husband of nearly twenty years, passed over on November 28, 2011. It was totally unexpected and an enormous shock. However, I am proceeding on with our project." Responding to my short reply she informed me that the name of the company is now called AmoTerra, and that Roberto Monti is no longer part of it.

We started corresponding and I had a chance to familiarize myself with their ATM process. Here is a brief discription of it, based on what I read in materials sent to me by Eleonora. They begin with several grams of thorium, in the form of an oxide (ash from burning commercially available lantern mantles). They mix this original material with nearly 2 kg of proprietary combustibles and burn the mixture in a stainless furnace, for about 2 minutes, at the temperature of about 1000 C.

They claim that approximately 50% of thorium (plus or minus 20%, depending on conditions) is transmuted into non-radioactive isotopes, or into isotopes which decay much more rapidly than thorium (within days). The equipment used is commercially available, except the secret combustible mixed with the initial radioactive material. They used three approaches to validate the thorium destruction claim: gamma ray spectroscopy, mass spectroscopy, and nuclear activation analysis.

Suppose I accidentally discovered that placing powdered thorium into milk, for ten minutes, destroys a large fraction of that radiactive metal. What would I do to convince myselve, and others, that my claim is valid? I would also begin with gamma ray spectroscopy. The amount of thorium can be determined from its speciphic activity, A, that is from knowing how many atoms decay in each second in each gram of ^{232}Th. That well known number, calculated from the half-life (14 billin years), is 3940. It does not depend on the kind of structure in which the element is found: solid metal, thorium oxide, thoriom salt, thorium solution, etc.

Suppose one gram of thorium is placed at a distance of 20 cm from a gamma ray detector and that the number of counts recorded per second is M=1000. That allows us to calculate the coefficient of proportionality, k, between the

activity A and the measured counting rate, M. Knowing that

A=k*M

I would be able to say that k=3940/1000 = 3.94. The value of k depends on the size of the detector, and on its distance from the source. These two parameters--the detector and its distance from the source--must thus be kept constant when the amount of thorium is determined by measuring the counting rate, M.

Suppose the original counting rate, from a sample of thorium oxide, is M1=4000 per second. That means that the original activity is A1=3.94*4000 = 15760 per second. The corresponding mass of thorium is 15760/3940 = 4 grams. Suppose the final counting rate (after the sample was processed) is M2=2000 per second. The corresponding mass of thorium (calculated in the same way) is 2 grams. Could I conclude that the processing destroyed 50% of the original thorium? Yes I could, but not before convincing myself (and others) that no thorium was lost via escaping into the atmosphers, or remained in the apparatus. Loosing thorium in that way would have nothing to do with my claim--the destruction of thorium atoms.

Showing that nothing was lost would call for additional control experiments. This part of the procedure would probably be very demanding. Several independent confirmations of the "no hidden losses" would be necessary, considering the extraordinary nature of the claim--transmutation of thorium into another element, or elements.

The same approach would be used in the case of the activation analysis. The only difference would be that M1 and M2 would refer of counting rates due to radioactivity induced by neutrons, rather than to radioactivity of thorium.

In the case of the mass spectromerty, on the other hand, the M1 and M2 could refer, for example, to measured heights of the ^{232}Th peak in the mass spectra, before and after processing. The most difficult part of the procedure--showing that nothing was lost--does not depend on which approach is used.

Added on 5/8/2012

Using my numerical illustration (4 grams of Th-232 initially and 2 grams finally) let me imagine the worse possible scenario. Suppose that these 2 grams of thorium are lost rather than transmuted. Even in this case the lost activity is only:

2*3940=7880 decays per second

Suppose that all this is uniformly distributed over the area of 100 square meters (10^6 cm^2). This is the floor of a small classroom or of a large living room. The activity per unit surface would be:

7880 / 1,000,000=0.008 decays/cm^2 per second (or 28 decays/cm^2 per hour.)

Monitoring this kind of contamination would not be easy. To demonstrate the "nothing is lost" I would use radium instead of thorium. ^{226}Ra decays about 8.6 million times fastes than ^{232}Th. Suppose two grams of radium (rather than thorium) is uniformly distributed over the area of 100 square meters. In that case the activity of each square centimeter would be 6.8 million decays per second. That would be much easier to measure than 7880 decays per second.

Needless to say, experimenting with grams of ^{226}Ra, ^{60}Co, or ^{241}Am, even if these materials were availabe, and if using them were legal, would be fulish. A tiny fraction of one gram would be sufficient to conduct a "nothing-was-lost" test. Furthermore, such tests can also be condicted with non-radioactive isotopes, such as ^{36}S, whose natural abundace is only 0.02% . This is only a speculation; I do not know how practical this would be. The ^{37}S, produced from this stable isotope via neutron bombardment, is beta and gamma radioactive.

As one can see, I am assuming that a potentially-possible loss via escaping would depend on how the AmoTerra

equipment is designed and not on what element is being transmutted. This is not at all obvious.

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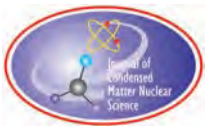
J. Condensed Matter Nucl. Sci. 7 (2012) 11–25



Review Article

Biological Transmutations: Historical Perspective

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Abstract

In this review paper, it is shown that in biological systems, chemical elements can be transmuted into other elements. These facts have been established since the early 19th century, but they have been ignored by established science ever since. The purpose of this work is to show how during the past two centuries, a number of experimentalists have questioned the mass conservation law established by Antoine Lavoisier [1] for chemical reactions. They have proved experimentally in plants, bacteria and other living organisms, some elements are transmuted into other elements.
 ISSN 2227-3123

Keywords: Biological transmutations, Cold fusion, History, LENR

1. Introduction

The discovery of Cold Fusion in 1989 by Stan Pons and Martin Fleischmann [2] has triggered new attention in the field of biological transmutations. Even though experiments have shown that transmutations of elements occur in living cells, this field has been totally ignored by the scientific community. The situation is not different now, but recently new experiments, in particular, by Vysotskii and Kornilova [3] have brought new results using modern analytical techniques.

It is interesting to recall the situation of chemistry before Lavoisier, which was the time of alchemy, when the modern scientific method had not yet been developed. Also the nature of the elements had not been clearly identified.

Most of the works come directly from Herzelee's experiments. They triggered the experiments made by Baranger, Kervran, Goldfein, Holleman and then Vysotskii. Many experimental results described in this paper are not of a good quality because they have been performed in the 19th century or in the early 20th century, and the full data are not easily available. Some are coming from a secondary source, and therefore are less reliable. However, if we consider the totality of these data, including the most modern ones, there is compelling evidence that biological transmutations are a real scientific fact.

Very few theoreticians have tried to understand the possible mechanisms involved in these kinds of reactions, and it is more likely to take a long time before a reliable theory can be developed. One of the reasons is the lack of useful data where all elements before and after are well known to the scientists.

In this review, I also recall some of the works that I have performed myself [4] with germinating seeds and bacteria.

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2. Before Lavoisier

2.1. Jan Baptist von Helmont (1579–1644)

The work by von Helmont [5] in the 17th century is probably the first experiment that tried to study the workings of plants. He wanted to prove that the alchemical theory of the four elements was incorrect. He grew a willow tree in a clay vessel with 90 kg of dried soil. He covered the vessel with an iron cover having small holes. In his report, he explained that he did not take into account either falling leaves or dust. For 5 years, he watered the plant with filtered rainwater or if necessary with distilled water. He observed that the tree had gained 76 kg, whereas after drying the soil had only lost 57 g. He concluded: "Water alone had, therefore, been sufficient to produce 76 kg of wood, bark and roots". Von Helmont proved that the elements of water and earth were not elementary, since water had changed into wood, bark and roots. This experiment proved that the elements of water could change into elements of soil.

3. Antoine Lavoisier (1743–1794)

In 1789, the famous French scientist Antoine Lavoisier [1] performed very accurate experiments showing conservation of mass during chemical reactions. He wrote:

> *" We can state as an indisputable axiom that under all conditions, artificial or natural, nothing is created; an equal quantity of matter exists before and after the experiment and nothing occurs outside the changes and modifications in the combinations of the elements".*

Unfortunately, for him and science, he was beheaded because of his function as "fermier général", i.e. an unpopular tax collector of the old regime. It is also interesting to note his inventions of an accurate ice calorimeter to measure the heat of respiration of a guinea pig, proving that breathing is actually a combustion process.

Landolt [6] has confirmed the mass conservation during chemical reactions with better accuracy in 1908 with an accuracy of one part in 10^6, and later in 1913 by Manly [7] with one part in 10^8.

4. During the 19th Century

4.1. Johann Christian Carl Schrader (1762–1826)

From 1795 to1797, the Berlin Academy of Science announced a competition with the following aim:

> *" Of which types of the earthly materials, which are encountered by means of chemical analysis, of native grain species? Do they either come into the grains as they are found or come into being by means of the life force and brought into growth by the workings of the plant?"*

In 1799, the German scientist Schrader [8] won the competition for his experiments on the formation of minerals in grains. He used the seeds of wheat, barley and rye, amongst others, to germinate in an artificial medium of flowers of sulphur (amorphous sulfur in fine powder) (that was shown to be completely ash free) and watered with distilled water. The dust contamination was prevented during the experiments. From the analyses of the developed seedlings, he compared with the seeds which are planted, he concluded that the mineral matter had indeed been created.

4.2. Henri Braconnot (1780–1855)

In 1807, the highly reputable French scientist Henri Braconnot [9] reproduced Schrader's experiments. He allowed plants from seed to grow on different artificial media (flowers of sulphur, red lead oxide, granulated lead, pure river sand and even an organic product; decomposed wood that was extracted using hot water). He concluded that considerable formation of the mineral components, especially potassium in experiments with mustard seed and radish, had taken place.

4.3. Louis Nicolas Vauquelin (1763–1814)

In 1799, the French chemist Louis Vauquelin [10] became intrigued by the quantity of lime which hens excrete every day. He isolated a hen and feed it a pound of oats, which were analyzed for lime (CaO). Vauquelin analyzed the eggs and faeces and found that five times more calcium was excreted than was consumed. He observed, not only the increase of calcium but also a subsequent decrease of silicon. He is certainly the first scientist to have demonstrated the biological transmutation of silicon into calcium.

In his conclusion he remarked that a loss of only 1.274 g of silica cannot account for an increase of 14.118 g of limestone. He concluded that lime had been formed, but could not figure out how it happened. Further more, he encouraged other scientists to replicate his experiment.

4.4. Albrecht Thaer (1752–1828)

In the 18th century organic reactions are attributed to a "life force". Thaer [11] showed that under some circumstances, calcium transforms into silicon. According to him, silicon could come from potassium. Under certain circumstances calcium in the plant became changed into silicon, whilst this substance may itself be formed from potassium

4.5. William Prout (1785–1850)

In 1822, the English physiologist, Prout [12] studied chicken eggs in incubation. He found that hatched chicks had more lime (calcium) in their bodies than originally present in the egg, and it was not contributed from the shell.

4.6. Wilhelm Augustus Lampadius (1772–1842)

In 1832, Lampadius [13] thought that plants themselves create silicon in plants.

4.7. Vogel

In 1844, a German researcher named Vogel planted watercress seeds (Lepidum satirum) in a bell jar in crushed glass in a controlled air environment. They were fed nothing but distilled water, yet when grown they contained more sulphur than had been in the seeds originally. J.J. Berzelius reported the experiment in his book [14]. Vogel's answer was that sulphur was not a simple element or that sulphur was introduced from sources unknown.

4.8. Choubard

In 1831, Choubard [15] germinated watercress seeds in clean glass vessels and showed that the sprouts contained minerals, which did not previously exist in the seeds.

4.9. John Bennet Lawes (1814–1900) and Joseph Henry Gilbert (1817–1901)

In 1856–1873, two British researchers, Lawes and Gilbert observed an inexplicable variation in the amount of magnesium in the ashes of plants. They could "extract" more elements from the soil than the soil actually contained in the first place, in particular the formation of magnesium in grass.

4.10. Albrecht Von Herzeele(1821–?)

In 1876 Herzeele [16], a German pharmacist published a series of books in which he showed research proving that plants continuously create material elements. From 1875 to 1883, in Berlin, he conducted 500 analyses with different types of seeds. He worked with: clover, crimson, vetch, rapeseed, barley, watercress, bean, white beans, kidney beans, turnips, rye, peas lupine, coltsfoot and angelica. A typical experiment showed the variation of calcium, potassium and phosphorus in Vicia sativa during germination with or without addition of mineral salts in distilled water.Also he showed that the addition of various calcium salts to the medium increased the formation of potassium. The addition of K_2CO_3, increased the formation of calcium.

He concluded that "Plants are capable of affecting the transmutation of elements". His publications outraged so much the scientific community of the time that they were removed from libraries. His writings were lost for more than 50 years until about ca.1930 when a collection was found by accident in Berlin by Dr. Hauschka, who subsequently published Von Herzeele's findings (the philosopher W.H. Preuss had dedicated an article to him; Preuss defended the idea that inorganic nature was a product of the organic; Herzeele was in agreement, apparently inspired by Goethe).

5. During the 20th Century

5.1. Freudler

Freudler was a Professor at the famous French University, La Sorbonne. In 1928, he published a book based on his 10 years of research on the production of iodine by algae. He noticed a connection between tin and granite in which the algae produced and iodine in the plants.

5.2. Earle Augustus Spessard

In 1940, Spessard [17] performed an experiment in which an organic process was studied in a hermetically sealed container. The bottles were weighed after some years. At the end, living protozoa were still seen through the glass walls. Presumably plant assimilation and animal respiration followed each other more or less in balance. There was a weight *increase* of a few tenths of a milligram (with a balance accuracy of 0.02 mg). Sources of errors, so far as they were known, were carefully eliminated. The predicted continuation of this work did not appear. The increase in weight that was found was far too big to be considered as a "materialization" of the received light rays.

5.3. Rudolph Steiner (1861–1925)

Rudolph Steiner [18] in 1924 gave a series of lectures giving indications for the development of a new approach to agriculture that later became known as biodynamics. In the 5th series of his lectures, he referred to composting, he stated "even according to the purely external standards of analytical chemistry, this ought to betray the fact that there is a kinship between the way in which oxygen and nitrogen are connected in the air and that in which lime and hydrogen are connected in organic processes. Under the influence of hydrogen, lime and potash are constantly being changed into nitrogenous matter, and finally into actual nitrogen. And the nitrogen, which has come into being in this way, has a

tremendous value for plant growth. Silicic acid, as we know, contains silicon and this in turn undergoes transmutation in the living organism. It is changed into a substance which is of exceptional importance but which is not reckoned by present-day science to be among the elements."

5.4. Henri Spindler

In 1946–1947, the French Scientist and Director of the Laboratoire Maritime de Dinard, Spindler [19] discovered Herzeele's work on the decrease of phosphorus and increase of calcium. In 1959, he measured an increase of iodine by 30% in algae, Laminaria flexicaulis and 80–100% in Laminaria sacharina.

5.5. Rudolf Hauschka (1891–1969)

An Austrian chemist, Hauschka [20] during the years 1934–1940, in sealed glass containers, weighed cress seeds, and found an increase in weight of 0.54% during the full moon, and a decrease of 0.58% during the new moon. He published several books in which he re-evaluated Herzelee's work, which he included as appendix in his books, *Substanzlehre* (though it has not been included in the English translation, *The Nature of Substances*).

5.6. Perrault

French scientist Perrault [21], from the Paris University, found that the hormone aldosterone provoked a transmutation of Na to K, which could be fatal to a patient.

5.7. Julien

Julien [22] was a French Scientist, from the Besançon University. In 1959, he proved that if tench are put in water containing 14% NaCl, their production of KCl increased by 36% within 4 h.

5.8. George 0shawa (1893–1966)

Oshawa [23] was a Japanese scientist, and an inventor of macrobiotics. He collaborated with Louis Kervran. His opinion was that transmutation occurs during mastication.

5.9. Pierre Baranger (1900–1970)

Pierre Baranger was a French Scientist, a professor of organic chemistry at the famous Ecole Polytechnique, and head of the Laboratory of Chemical Biology. He became intrigued with Herzeele's experiments, but he thought that the number of trials had been too limited and the precautions against error were insufficient. Baranger decided to repeat the experiments with all possible precautions and a very large number of cases, which would allow a statistical study. His research project from 1950 to 1970 involved thousands of analyses. Baranger verified the content of phosphorus, potassium, calcium and iron of vetch seeds before and after germination in twice-distilled water to which pure calcium chloride was not added. Hundreds of samples of 7–10 g each were selected, weighed to 1/100th milligram, and graded, then germinated in a controlled environment.

Baranger found an increase of 4.2% in calcium, and 8.3% of iron, and subsequently a decrease in phosphorus of 1.9%, and of potassium of 1.1%. Interestingly, an addition of $MnCl_2$ increases the amount of iron produced.

None of the specialists who examined Baranger's work were able to find any experimental errors. Baranger concluded:

"These results, obtained by taking all possible precautions, confirm the general conclusions proposed by Von Herzeele and lead one to think that under certain conditions the plants are capable of forming elements, which did not exist before in the external environment".

In May 1959, he submitted an article for publication in the French Academy of Sciences, but was not accepted. Later in 1972, his family tried another submission without success. He had difficulties in publishing his findings, and died without being able to do so. Later, in 1977, his family asked Jean Marie Gatheron, a close friend of Baranger to publish Baranger's work [24]. In 1976, his family submitted the final report of Baranger to the Academic Commission of the French Academy of Agriculture. It was decided that the work would be presented to the full assembly in a secret meeting. The proposal of publication in a public meeting was rejected without any reason.

Baranger failed to provide relevant theory to explain his findings.

5.10. Leendert Willem Jacob Holleman (1906–1994)

From 1975 to 1989 Holleman [25], a Deutch scientist, performed experiments with alga Chlorella. He observed a decrease, then subsequent increase, of potassium. However, in spite of several attempts, he could not reproduce his own first positive experiments.

5.11. Correntin Louis Kervran (1901–1983)

Kervran is certainly the most well-known scientist having worked in the field of biological transmutations. He had a broad knowledge of plants, geology and nuclear science. His findings have been published in French in ten books [26], some of them have been translated into English [27]. He was also nominated for the Nobel Prize.

Observations

From 1935 Kervran [28] collected facts and performed experiments, which showed that transmutations of chemical elements do indeed occur in living organisms. It started when he investigated fatal accidents from carbon monoxide poisoning when none was detectable in the air. Next he analysed why Sahara oilfield workers excreted a daily average of 320 mg more calcium than they ingested without decalcification occurring.

Kervran pointed out that the ground in Brittany contained no calcium; however, every day a hen would lay a perfectly normal egg, with a perfectly normal shell containing calcium. The hens eagerly pecked mica from the soil, and mica contains potassium. It appears that the hens may transmute some of the potassium into calcium.

Experiments with seeds

From 1960 to 1980, Kervran reported the astounding results of his research showing that living plants were able to accomplish limited transmutation of elements. Then Kervran was the Conferences Director of the University of Paris, and his first paper was published in La Revue Générale Des Sciences, July 1960.

Kervran found that in nuclido-biological reactions, oxygen is always in the form of O, never O_2; reactions with nitrogen occur only with N_2, insofar as is known. The following reactions have been proposed:

$Na^{23} + H^{1} \rightarrow Mg^{24}$ $Na^{23} + O^{16} \rightarrow K^{39}$ $Na^{23} - O^{16} \rightarrow Li^{7}$

$Na^{23} \rightarrow Li^{7} + O^{16}$ $K^{39} + H^{1} \rightarrow Ca^{40}$ $Mg^{24} + Li^{7} \rightarrow P^{31}$

$Mg^{24} + O^{16} \rightarrow Ca^{40}$ $F^{19} + O^{16} \rightarrow Cl^{35}$ $C^{12} + Li^{7} \rightarrow F^{19}$

$Cl^{35} \rightarrow C^{12} + Na^{23}$ $Fe^{56} - H^{1} \rightarrow Mn^{55}$ $2\ O^{16} - H^{1} \rightarrow P^{31}$

$O^{16} + O^{16} \rightarrow S^{32}$ $2\ N^{14} \rightarrow C^{12} + O^{16}$ $N^{14} + Mg^{12} \rightarrow K^{19}$

$Si^{28} + C^{12} \rightarrow Ca^{40}$ $Si^{28} + C^{12} \rightarrow Ca^{40}$ $P^{31} + H^{1} \rightarrow S^{32}$

In 1980, Kevran [29] performed an experiment with oat seeds analysed using mass spectroscopy. They looked at phosphorus and calcium variations. They observed the following:

	Phosphorus (mg)	Calcium (mg)
Seeds	485	76
Plants	310	115.5
Difference (mg)	–175	+39.5

It is clear that the calcium increased with germination, whereas phosphorus decreased. There are certainly other elements that played a role, but they were not analysed in this experiment.

The French Society of Agriculture

In 1971, the laboratory of the French Society of Agriculture sprouted rye seeds under controlled conditions.

	Seeds	Sprouts	Difference (mg)	Difference (%)
Mg (mg)	13.34	3.20	–10.14	–335%
K (mg)	7.36	16.67	+9.31	+133%

These results are in good agreement with Kervran's previous findings.

Kervran was very active in promoting his work through books, conferences and mass medias. However, the Academy of Agriculture strongly opposed his efforts. In October 7, 1970, Stéphane Henin on one side and Léon Guéguen and Allez on the other side sent reports to the Academy by criticizing Kervran's results [30].

5.12. J.E. Zündel

Zündel [31] was a Swiss scientist, head of a paper company, and a chemical engineer at the Polytechnic School of Zurich (ETH Zurich) in Switzerland. Following Kervran's observations from 1970, he studied germinating seeds and observed a 54–616% increase of calcium. In another experiment, he grew 150 oats seeds (flämingkrone) in a controlled environment for 6 weeks. 1243 sprouts were analysed using atomic absorption spectroscopy for the presence of magnesium and calcium. The potassium decreased by 0.033%, the calcium increased by 0.032%, and magnesium decreased by 0.007%. The variation of magnesium was not significant, but the decrease in potassium balanced the increase of calcium. In 1972 with oat seeds, he observed an increase of calcium of 118%, a decrease of magnesium of 23%, and potassium 29%.

In 1971, he gave a lecture at the French Academy of Agriculture (Bull No. 4, 1972). In his lecture, he announced the following variations between seeds and sprouts:

	SiO_2	Ca	Mg	K
Seeds	111 mg	28 mg	27 mg	108 mg
Sprouts	123 mg	116 mg	27 mg	70 mg
	+10%	+314%	0%	–35%

In spite of the excellent quality of his works, the audience criticized him, including S. Henin, the head of the Department of Agronomy.

Later in 1979, Zündel, using a mass spectrometer at the the Microanalysis Laboratory of the French National Scientific Research Center, and neutron activation analysis at the Swiss Institute for Nuclear Research in Villigen (Aargau), confirmed the increase of calcium by 61%. There was also an increase of 29% in phosphorus and 36% in sulphur) [32].

However, the French Atomic Energy Commission has analysed Zündel's experiments in 1975 by neutron activation analysis of oat seeds. They found no change in calcium, sodium and manganese, but only a small decrease of potassium, also no isotopic variation in Ca^{48} and K^{41}.

5.13. Hisatoki Komaki

Following Kervran's work in 1970–1980, Komaki [33] from the Biological and Agricultural Research Institute studied the development of bacteria, mould and yeast. Among those: *Aspergillus niger*, *Penicillum chrysogenum*, *Rhizopus nigricans*, *Mucor rouxii*, *Saccharomyces cerevisiae*, *Torulopsis utilis*, *Saccharomyces ellipsoideus* and *Hansenula anomala*. Komaki reported that eight strains of microorganisms grown in potassium deficient culture media increased the total of potassium by transmutation of calcium to potassium. He also showed that phosphorus can be formed by the fusion of nitrogen and oxygen: $N + O \rightarrow P$. He even marketed a brewer's yeast product that, when applied to composts, increases their potassium content.

6. Present Times

6.1. Panos *T. Pappas*

In 1998, Pappas [34] published an article suggesting that biological transmutation occurs as a form of cold fusion in the cellular membrane sodium–potassium pump. According to Pappas, the ions are not pumped back and forth through the membrane, but instead transmute back and forth between Na and K.

6.2. Jean-Paul Biberian

Experiments were performed with seeds: wheat and oats as well as bacteria: Marine bacteria (Marinobacter sp strain CAB) and Lactobacilius [4]. In most of the experiments, variations in the concentration of minerals have been observed. In particular, it is interesting to note that when the seeds grew, heavy metals decrease in large amounts. Even though these results are only preliminary, they confirm the observations made by others, in particular Kervran.

6.3. Vladimir Vysotskii

Vysotskii is a scientist from Ukraine. He started working on biological transmutations in the 1990s. He is well known for using modern analytical techniques. In particular, he used Mossbauer spectroscopy, very sensitive to Fe^{57}, to measure its production. In natural iron, Fe^{57} represents only 2.2% of the total. The main isotope of iron is Fe^{56}, which represents 91.7%. Measuring Fe^{57} is also very easy by mass spectroscopy, since there is no possible interference with another element. The proposed transmutation is

$$Mn^{55} + D^2 \rightarrow Fe^{57}.$$

The experiments conducted by Vysotskii and Kornilova [3] were performed with bacteria capable of developing in heavy water. They chose *Bacillus subtilis*, *Escherichia coli* and *Deinococcus radiodurans*, as well as a yeast culture *Saccharomyces cerevisiae*. When manganese was introduced with $MnSO_4$, a clear spectrum was measured, indicating that manganese had been transmuted into iron. The authors analysed the material by time-of-flight mass spectroscopy showing that the mass 57 peak was as large as that of mass 56. This is another confirmation of the production of Fe^{57}. Vysotskii and Kornilova have also analysed another reaction

$$Na^{23} + P^{31} \rightarrow Fe^{54}.$$

In natural iron, Fe^{54} represents only 5.8%. The bacteria developed in a medium without iron, and after development they measured Fe^{54} as large as Fe^{56}.

In similar experiments they observed the following reaction

$$Cs^{133} + H^1 \rightarrow Ba^{134}.$$

In experiments destined to reducing radioactivity, they conducted experiments with synthetic microbiological cultures, which were up to 20 times more effective than the standard microbiological cultures. It was shown that Ba^{140}, which is radioactive with a half-life of 12 days, transforms into Sm^{152}, which is stable with the possible following reaction:

$$Ba^{140} + C^{12} \rightarrow Sm^{152}.$$

Interestingly, Cs^{137}, which is radioactive with a half-lifetime of 30 years, transmutes into Ba^{138}, which has a much shorter lifetime of 310 days.

$$Cs^{137} + H^1 \rightarrow Ba^{138}.$$

This work is certainly the best proof of biological transmutations.

6.4. Edwin Engel, Rudolf Gruber

In 2006, Engel and Gruber [35] from Germany wished to confirm Kervran and Baranger's works. They showed that during germination, manganese transmutes into iron. They used mung beans sprouted in $MnCl_2$. They showed an increase of iron. They assumed the following reaction

$$Mn^{55} + H^1 \rightarrow Fe^{56}.$$

7. Negative Experiments

Even though many positive experiments have been performed by indicating the reality of the phenomenon of biological transmutations, several experiments contradict these findings.

7.1. Nicolas Théodore de Saussure (1767–1845)

In 1804, de Saussure published his work: "Recherches chimiques sur la végétation", Nyon, Paris (Chemical Researches on Végétation). As a follower of Lavoisier, Saussure stood strongly with the standpoint of the conservation of matter and referred all transmutations and creation to the realm of fables. He puts special emphasis on the necessity of this field to be absolutely certain, with experiments, that the so-called created matter was not already present in the environment. So he demonstrated, for example, that the presence of silicates in plants, which were attributed to the life-force by Lampadius, was in reality determined by the amount of silicon present in the soil.

7.2. Jean-Louis Lassaigne (1800–1859)

Lassaigne initially worked in the laboratory of Louis-Nicolas Vauquelin. He was a professeur at Ecole Vétérinaire d'Alfort. His works were published in 1821 with the germination of grains supported the findings of de Saussure. His later works on the development of chicks contradicted the results of Vauquelin.

7.3. P.E. Jablonski

In 1836, Jablonski found no increase in the amount of ashes in the plants above the one in the seed. Therefore, criticizing Schrader and Braconnot.

7.4. Arend Joachim Friedrich Wiegmann (1770–1853) and A.L. Polstorff

Wiegmann and Polstorff made the following experiment: They followed the techniques of their predecessors, but with a soil that consisted of the most inert material known to them at that time. They let 28 seeds of garden cress (Lepidium sativum) germinate in distilled water in a platinum crucible that was filled with fine platinum wires. The crucible was placed under a glass bell jar through which circulated a mixture of 1% carbon dioxide. The seeds germinated and grew into small plants until, after 26 days, they began to die. After drying the crucible and its contents, ashing and weighing obtained 0.0025 g of ash. The weight of the ash obtained from 28 seeds was likewise 0.0025 g. Therefore, there was neither weight change nor new elements formed. However, this conclusion does not contradict previous experiments, since only the weights were compared. No attempt was made to check for the transmutation of one element into another.

7.5. M. Emile Rinck

In 1947, Rink checked Hauschka's work and found only 0.02% weight change.

7.6. Léon Guéguin

In 1970, Guéguin from the INRA (The French Institut of Agronomical Research) has shown that there was no transmutation in Kervran's type experiments.

7.7. D.B. Long

Long was a British scientist, from the Michaelis Nutricional Research laboratory, Harpenden, UK. In 1971, he published a report [36] indicating that he did not observe differences in K, Mg, Ca, Mn, Fe, Zn and Cu when looking at rye and oat seeds germinated. The table shown below shows his experimental results.

Elements	Seeds		Plants		Change (%)
	Average value	Standard deviation	Average value	Standard deviation	
Potassium (mg)	1.582	0.009	1.506	0.016	–4.8
Magnesium (mg)	1.270	0.006	1.273	0.006	+0.2
Calcium (μg)	2.122	0.016	2.157	0.019	+1.6
Manganese (μg)	28.8	0.1	24.9	0.2	–13.5
Iron (μg)	49.7	0.2	48.6	0.5	–2.2
Zinc (μg)	20.9	0.2	21.7	0.05	+3.8
Copper (μg)	6.22	0.2	6.57	0.1	+5.6

7.8. L. Soubiès and R. Gadet

Following Baranger's work, and his attempts to get his findings accepted by the scientific community, Soubiès and Gadet performed similar experiments with the Baranger's protocol and a more rigorous one. They presented their results in 1972 in the bulletin of the French Chamber of Agriculture. They demonstrated that using their better protocol no transmutation is detected, whereas with the original one there is less sodium produced. They proposed that the anomalies were due to diffusion of minerals from the glass used in the experiments.

7.9. Horber

In 1976, Horber, a Swiss scientist from Zurich, looked at calcium variations by neutron activation analysis. He found a 2% variation, but with a precision of ±5%.

7.10. J.A. Jungerman and Murphy

In February 1977, Professor Jungerman and Murphy from the University of California, Davis reported the results of an experiment: the growth of oat seeds under carefully controlled conditions. Analysis was made by atomic absorption and X-ray fluorescence for Ca and K. They found no evidence of transmutation.

7.11. Carolyn E. Damon

In 1978, Damon from the U.S. Customs Technical Service Division conducted tests for biological transmutation with *Aspergillus terreus* and *Rhizopus nigricans*, he obtained negative results.

7.12. Bernd Franke

In 1978, Franke defended his thesis for States Exam at the Botanical Institute of the University of Heidelberg in Germany, the title of which was: "Critical examination of tests on the transformation of biological elements". He analysed calcium, magnesium and potassium during the growth of oats seeds (*Avena sativa*) and yeast (*Sacharomycus cervisiae*). He did not find any significant changes in the composition of the seeds and yeast during their analyses.

7.13. Enrico Di Vito, Carla Candian, Luigi Garlaschelli and Antonio Triassi

In 2002, these scientists from Italy failed to replicate Kervran's work. They looked at the growth of oat seeds (cultivar *Nave*). They analysed their products using ICP emission, but found no variation in Ca, Mg and K in sprouting oat seeds.

8. Theory

8.1. Kervran

Kervran thought that the nuclear reactions that occur in biology were connected to the structure of the nucleus. He developed a unique model of the nucleus with a design that explains the potential transmutations.

8.2. Costa de Beauregard(1911–2007)

Costa de Beauregard [37] was the Research Director at the Centre National de la Recherche Scientifique, Paris, and Professor of Theoretical Physics at the Institut de Physique Théorique Henri Poincaré. He studied Kervran's works in 1962 and began to correspond and met with him. He suggested that such transmutations neither take place through strong interactions nor through electromagnetic forces, but through the weak interaction. This takes place through the neutral current of the intermediate vector boson, the so-called Zo, particle. Kervran's reaction for a biological transmutation from potassium to calcium in germinating oats is thus explained as being initiated by neutrino capture (from cosmic rays) and the weak interaction follows mediated by the *Z*, neutral current.

$$\vartheta + {}^{1}_{1}\mathrm{H}^{+} + {}^{29}_{14}\mathrm{K}^{+} \xrightarrow[+\text{enzyme}]{+Z^{0}} {}^{40}_{20}\mathrm{Ca}^{++} + \vartheta^{-}.$$

In 2006, I called Costa de Beauregard at his home in Paris, and asked him if he continued his research. He replied that he did not.

8.3. Goldfein

In 1978, an officially funded effort from the U.S. Army Mobility Equipment Research and Development Command, Fort Belvoir, Virginia positively confirmed that mechanisms for elemental transmutations could occur in biological systems. The work was performed under the direction of Emil J. York, Chief of the Material Technology Laboratory. Solomon Goldfein was the principal investigator for the effort. Robert C. McMillan, Chief of the Radiation Research Group of the laboratory, provided guidance on matters of physics and nuclear physics. The abstract of the final report [38] reads as follows:

"The purpose of the study was to determine whether recent disclosures of elemental transmutations occurring in biological entities have revealed new possible sources of energy. The works of Kervran, Komaki and others were surveyed, and it was concluded that, granted the existence of such transmutations (Na to Mg, K to Ca and Mn to Fe), a net surplus of energy was also produced. The proposed mechanism was described in which Mg adenosine triphosphate, located in the mitochondrion of the cell, played a double role as an energy producer. In addition to the widely accepted biochemical role of Mg-ATP in which it produces energy as it disintegrates part by part, Mg-ATP can also be considered to be a cyclotron on a molecular scale. The Mg-ATP when placed in layers one atop the other has all the attributes of a cyclotron in accordance with the requirements set forth by E.O. Lawrence, inventor of the cyclotron." "It was concluded that elemental transmutations were indeed occurring in life organisms [sic] and were probably accompanied by a net energy gain."

Goldfein postulated a conformational structure of a stack of Mg-ATP molecules forming a helical chain. The Mg-ATP chelate produces oscillating electrical currents, which act as a micromini-cyclotron that accelerates hydrogen ions to relativistic speeds with sufficient potential to transmute an element to the next higher number. It was concluded that the elemental transmutations occurring in living organisms are accompanied by losses in mass representing conversion to thermal energy and that such energy probably is a net gain when compared to the amount required to effect the transmutation.

8.4. Conclusion

Lavoisier has established a mass conservation law which is valid in chemistry. Now we know that it is not true when nuclear reactions are involved. The review of more than two centuries of research demonstrates that this is not true in biology. It appears that all living organisms can under some circumstances produce nuclear reactions. However, there is an important need of finding an adequate theory to explain these results. It is highly probable that such a theory should also be capable of explaining Cold Fusion, or more generally, nuclear reactions in condensed matter. Another point is the irreproducibility of some experiments. Probably, in order to produce significant transmutation of an element, it is necessary that another element be missing. It seems that nature has a tendency to find ways to transmute an element into another to provide the necessary ingredients for the healthy growth of the four kingdoms of bacteria, fungi, plants and animals, including human beings.

Historically, the sequence of discoveries in biological transmutation is the following: Vauquelin was the initiator in 1799. Later Herzelee in 1876–1883 did a lot of research, but his work was removed. Later Hauschka rediscovered Von Herzelee's work. Baranger and Kervran discovered this work. As a consequence of their contribution to the field Zündel continued the work as well as Goldfein. Finally Vysotskii knowing the work of Kervran brought an important contribution.

This review shows that biological transmutations deserve a lot of attention from the scientific community. The consequences of this are important for science, medicine, agriculture and diet. The cost of research in this field is so low compare with other fields that it is unacceptable not to do it.

Studies of the process called cold fusion or Low Energy Nuclear Reactions (LENR) over the past 22 years show that nuclear reactions of various kinds can be initiated in inorganic solid materials under conditions similar to those present during the claimed biological transmutations. These reactions all have the basic characteristic of producing energy as would be required of a spontaneous reaction. In contrast, many of the proposed nuclear reactions in biological systems cannot result in energy production because mass is not lost in the process. Instead, the mass gain would require the concentration of significant energy from the environment. This violates the basic laws of thermodynamics and makes the suggested reactions impossible. In addition, the elements involved in the proposed nuclear reaction must have a way to find each other in the same biological structure and experience a reduction in their Coulomb barrier before interaction can occur. These several limitations severely limit possible explanations. However, these limitations do not make the nuclear reactions impossible, just more of a challenge to explain. The basic question to be answered is do such reactions actually occur in Nature? The evidence strongly indicates that some of the observed reactions actually occur, requiring an explanation to be found.

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Research Article

Issues Related to Reproducibility in a CMNS Experiment

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Abstract

Unexplained emission of charged nuclear projectiles due to electrolysis has been reported by Richard Oriani. Experimental results were said to be highly reproducible. Working independently, we were not able to observe emission of charged nuclear particles (in a chemical process similar to Oriani's) and therefore are unable to provide supporting evidence that the effect is reproducible.

Keywords: Cold Fusion, CMNS, LENR, Nuclear Reactions

1. Introduction

Radioactivity, such as emission of alpha particles, has long been thought to be independent of chemical processes, such as electrolysis. The same is true for nuclear reactions including fusion and fission. The reason for this is that the kinetic energies of molecules in a typical chemical process are too small to overcome the Coulomb repulsion between positively charged nuclear particles [1]. This expectation was challenged by Pons and Fleischman [2], Oriani [3, 4], and other scientists, as described by Storms [5]. We were attracted by Oriani's work because his findings were impressive

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(a nuclear process taking place during electrolysis) and his methodology was simple. Our goal was to either confirm or refute the claimed reproducibility of the effect.

The electrolyte in Oriani's cell, Li_2SO_4, is usually dissolved in light water at the concentration of 0.022 g per cubic centimeter. The cathode electrode is usually nickel while the anode electrode is usually platinum. To detect charged nuclear particles, Oriani uses CR-39 chips. This method of detection is widely used in personal neutron dosimeters as well as in other fields of research [6] such as measuring the amount of radon in the air and studying thermonuclear reactions. In Oriani's 2003 experiment, described in [3], the detector was suspended in the electrolyte between the anode and the cathode. The mean track density on the experimental chips was 470 tr/cm^2 while the mean track density on control chips was 168 tr/cm^2. The corresponding standard deviations were 384 and 99 tr/cm^2, respectively.

Referring to such results, Oriani et al. concluded in 2003 that: "a nuclear reaction of as-yet unknown nature can accompany the electrolysis." Experimental results, according to Oriani's more recent 2008 paper [4], are now highly reproducible. The purpose of the present study was to verify this assertion.

2. Oriani's Cell and New Experimental Results

The cell used in Oriani's new study [4] is shown in Fig. 1. It was similar to the cell used in [3]. The essential difference was the addition of a thin Mylar film (6 μm) placed between the CR-39 detector and the nickel cathode. That film was chosen to protect the CR-39 detector from the electrolyte without interfering with detection of expected nuclear particles.

Twenty-one experiments were performed and 42 CR-39 surfaces were examined, as described in his 2008 paper [6]. The track density on one of these surfaces was reported as "too large to count." Track densities on the remaining 41 experimental surfaces ranged from 9 to 498 tr/cm^2, as illustrated in Fig. 2. The histogram was drawn without making a distinction between surfaces facing the cathode and surfaces facing away from the cathode. Note that the distribution of densities is not bell-shaped. Track densities on control CR-39 chips were usually much lower (mean 21 tr/cm^2 and standard deviation 9.7 tr/cm^2) than on most experimental chips (mean 122 tr/cm^2 and standard deviation 124 tr/cm^2). Mean track densities on surfaces facing the cathode, according to data published in [4], were essentially the same as mean track densities on surfaces facing away from the cathode.

On the basis of these observations Oriani concluded that "the present technique has consistently produced evidence that a nuclear reaction of some sort has been generated in the course of electrolysis." Investigations described in this paper were undertaken to determine if Oriani's results could be reproduced in another laboratory.

3. Our Cells and Experimental Results

Our experiments, done in four separate laboratories, were performed using cells that were similar to Oriani's cell. Figure 3 displays a generic diagram of our cells. We used the same electrolyte (Li_2SO_4 in distilled H_2O, at the initial concentration of 0.022 g/cm^3) as Oriani; our electrodes were also made from Ni and Pt. Oriani's nickel cathode wire and our nickel cathode wires were from the same spool. All of us used the 6 μm Mylar which is capable of transmitting alpha particles with energies higher than 2 MeV. Distances between the cathodes and the anodes were approximately 3 cm, which is approximately the same as Oriani's. The essential cell parameters of Oriani's and our experiments were the same.

Differences between Oriani's experiment and our experiment are the following: Platinum wires for the anodes did not come from the same manufacturer. Oriani's cathode was spot-welded to a vertical titanium rod, placed inside a glass tube. Vertical parts of our electrodes, on the other hand, were placed into long heat shrink tubes (not shown in Fig. 3). Lower parts of our shrink tubes were thermally compressed, to prevent capillary creep of the electrolyte. Also note that Oriani's cell exposes the bottom surface (the side away from the electrolyte) of the CR-39 detector to air while our

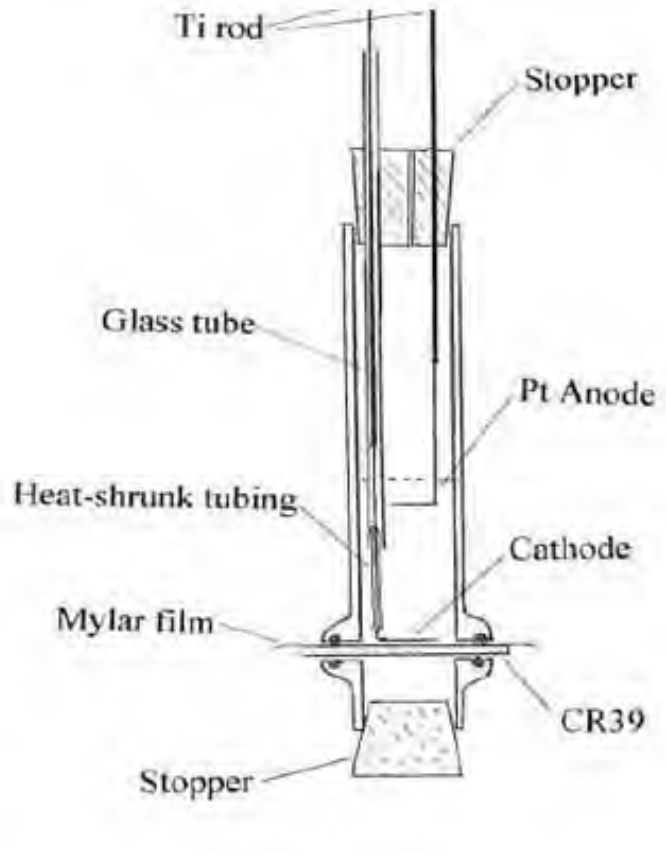

Figure 1. A diagram of an Oriani cell from his 2008 paper [4]. Small circles are cross sections of O-rings placed between the two sections of the cell. A clamp (*not shown*) is used to press the sections toward the CR-39 detector.

cells do not. In our cells, the bottom surface of the CR-39 is in contact with a 25" layer of polyethylene. That layer was clamped to the cell. The leakage of the electrolyte was prevented by squeezing the CR-39, Mylar and O-ring between the polyethylene and the cell.

Both we and Oriani used Fukuvi CR-39 material purchased from Landauer. The delivered sheets of that material are protected from scratches and alpha particles in air by a thin layer of polypropylene plastic. That layer usually peels off when the sheet is cut into small chips. Our unprotected chips were kept in salty distilled water (NaCl concentration of about 10 mg/cm^3). Furthermore, all control chips were kept in unused electrolyte during electrolysis. Salty water prevents potential accumulation of electric charges on CR-39 surfaces kept in air. This precaution was taken to reduce the possibility that electric charges on CR-39 surface in air might attract or repel radioactive ions. No chips were exposed to air for more than one hour. One author (P.L.) compared track densities on two CR-39 chips, one kept in

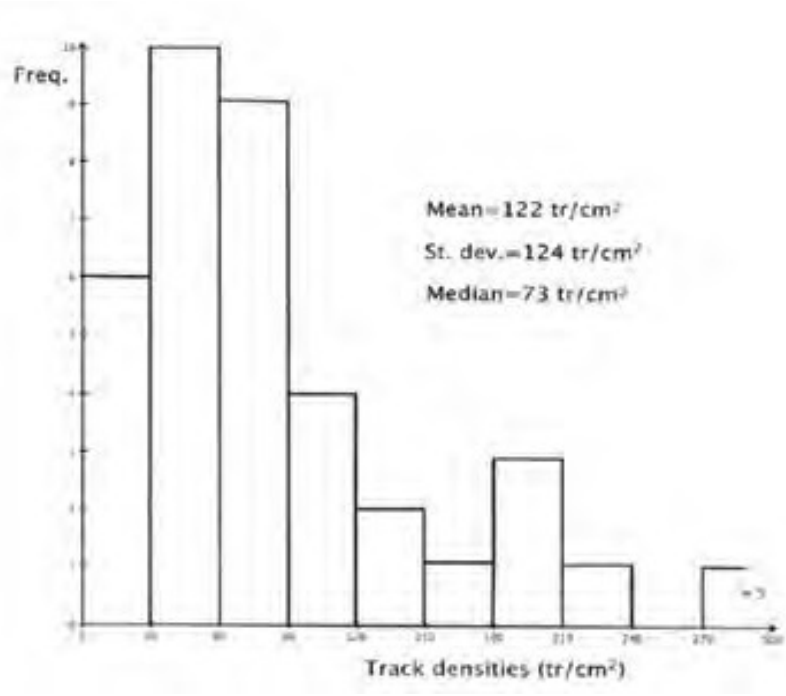

Figure 2. Oriani's distribution of track densities from his 2008 paper [4]. The +5 and 500, near the lower right corner, refer to five surfaces where track densities were between 300 and 500.

distilled salty water and in air, for 15 days. The track density turned out to be 30 tr/cm^2 on the chip kept in salty water and 505 tr/cm^2 on the chip kept in air. This can be explained by the big difference between ranges of alpha particles in air (centimeters) and in water (microns). Oriani did not use salty water; he wrapped the chips in aluminum foil and kept them in air. All of our "control" chips were kept in unused electrolyte during the time that the "experimental" chips were exposed to electrolysis.

Like in [4], we typically exposed the experimental chips to electrolysis for three days. During this time the electric current was changing slowly from 60 to about 90 mA, due to the loss of water. The corresponding voltage across the electrodes was typically 7– 8 V and was kept constant during the experiment. The cell was dismounted after each experiment and the CR-39 chip was removed. It was then etched for 12 h in the stirred water solution of NaOH (concentration 6.5 M, temperature 72˚C). The pits on CR-39 surfaces, identified as tracks of nuclear particles, were subsequently counted under the microscope. The uncertainty of up to plus or minus 5% (in reported track densities) was common due to difficulties in distinguishing tracks from certain surface defects. Each experimenter was given a CR-39 chip with one corner that was exposed to alpha particles from an ^{241}Am source. This helped the experimenter learn the difference between pits and defects. Pits due to particles from that source are shown in Fig. 4. They had the same size as pits found on control and experimental chips.

The mean and standard deviation on eight of P.L.'s experimental surfaces were 87 and 46 tr/cm^2. The mean and standard deviation on his five control surfaces were 59 and 38 tr/cm^2. This information, as illustrated in the Appendix, can be used to justify the statement that the reported difference between mean track densities on experimental and

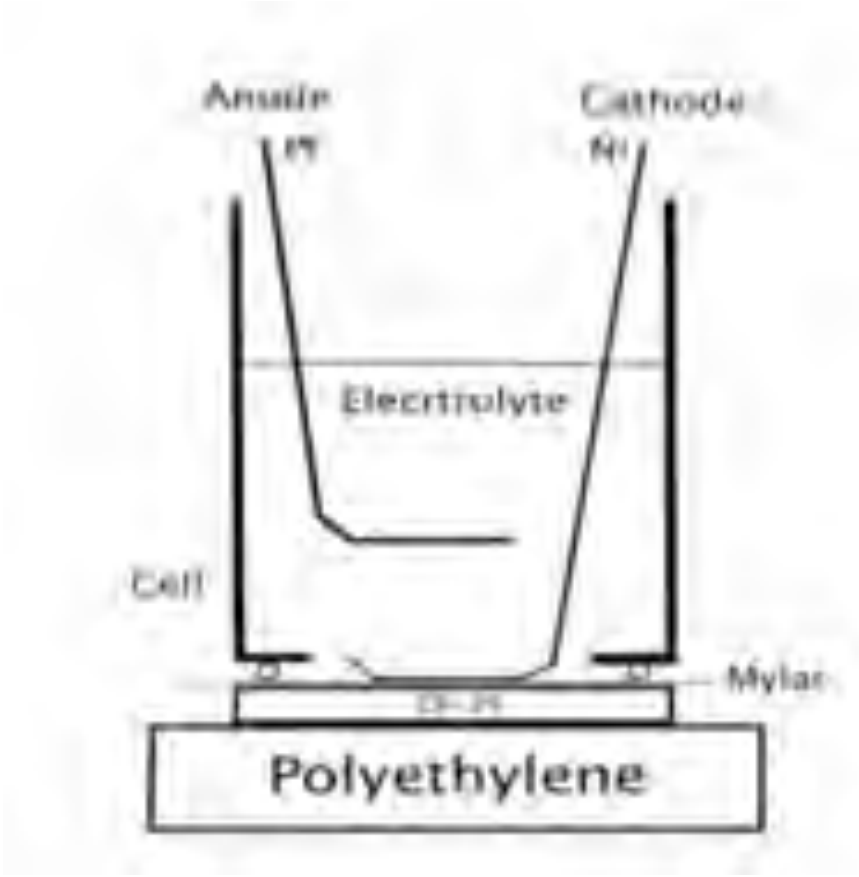

Figure 3. A generic diagram for our electrolytic cells. Small circles are cross sections of the compressed O-ring.

control surfaces, 87–59 = 28 tr/cm^2 was not statistically significant, considering small numbers of examined surfaces.

The mean and standard deviation on six of M.H.'s experimental surfaces were185 and 73 tr/cm^2. The mean and standard deviation on his eight control surfaces were 192 and 72 tr/cm^2. This information was also used to justify the statement that the reported difference between mean track densities on experimental and control surfaces, 185–192 = –7 tr/cm^2 was not statistically significant.

The mean and standard deviation on eight of J.D.'s experimental surfaces were 98 and 17 tr/cm^2. The mean and standard deviation on his six control surfaces were 125 and 29 tr/cm^2, respectively. This information was used to justify the statement that the reported difference between mean track densities on experimental and control surfaces, 98–125 = –27 tr/cm^2 was not statistically significant.

The mean and standard deviation on nineteen of L.K.'s experimental surfaces (excluding three exceptional surfaces from experiments 13, 14 and 15 — see below) were 16 and 8 tr/cm^2. The mean and standard deviation on his 16 control surfaces were 14 and 5 tr/cm^2, respectively. This information was used to justify the statement that the reported

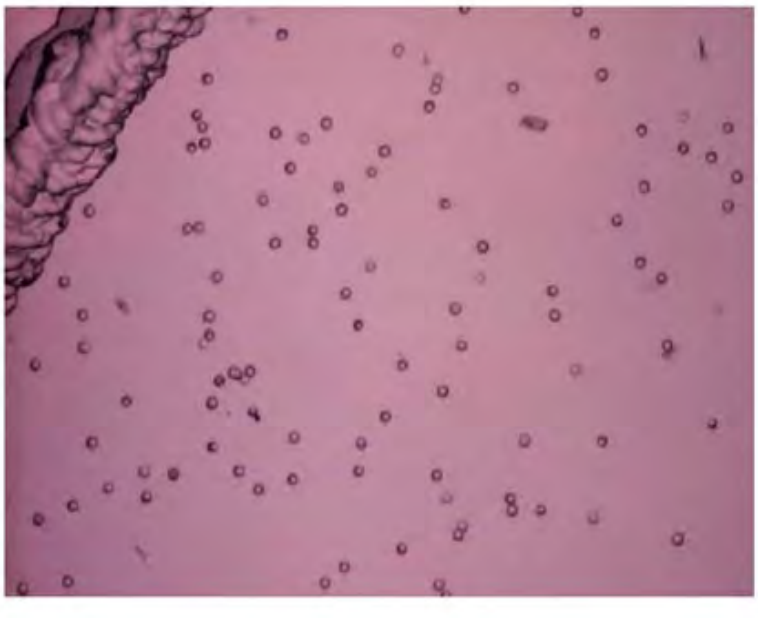

Figure 4. A CR-39 surface seen under the magnification of 40, after 12 h of etching. Small circles are pits due to alpha particles from an Americium source. The upper left corner shows a mechanically damaged area.

difference between mean track densities on experimental and control surfaces, 16–14 = 2 tr/cm^2 was not statistically significant. In other words, experimental results from our four sets of experiments are consistent with each other. But they are not consistent with the results reported by Oriani.

Let us address the issue of extremely high track densities on three surfaces from experiments 13, 14 and 15. The estimated track density on the up-facing surface of the CR-39 chip from Experiment No. 13 were around 15,000 tr/cm^2. This is two orders of magnitude higher than typical densities reported by Oriani [6]. The other side of the chip had nothing but the usual background. The area covered by copious tracks, nearly two square centimeters, matched the circle delimited by the O-ring. Distribution of tracks in that area was more or less uniform, except near the boundaries, where the track density decreased progressively.

Copious tracks from Experiment Nos. 14 and 15 were very different from Experiment No. 13. On these two surfaces, tracks were concentrated in small areas, near a hole drilled in each CR-39 chip. The holes were drilled to facilitate etching; each CR-39 chip was suspended in the etching solution by means of a thin copper wire threaded through the hole. These copper wires were from a telephone cable of unknown origin. The area covered by copious tracks from Experiment No. 14 was about 10 mm^2; the estimated track density was 200,000 tr/cm^2. The area covered by copious tracks from Experiment No. 15 was approximately 3 mm^2; the estimated track density was close to 12,000 tr/cm^2. Track densities outside of the small affected areas were about the same as on control chips.

It is remarkable that in each of the three cases of high track density, the mean density on the other side of the chip was essentially the same as on the control chips. That is very different from what was reported in [4]. The most likely cause of copious tracks was migrating radioactive contamination. Attempts made to identify it were unsuccessful. Alpha

radioactive substances such as uranium, thorium and radium are known to be present in our environment. One nanogram of radium, for example, emits 37 alpha particles per second. Testing of nuclear weapons in the 1960s contributed to contamination of our environment with long-lasting radioactive isotopes, tritium and uranium for example. Note that track densities on our three exceptional surfaces were much higher than those reported in [4].

Project participants worked independently of each other in four different states. Collective results, shown in Table 1, became available to participants only after their own results were submitted to L.K. He collected results after counting tracks on his own CR-39 surfaces. Note that Experiment No. 12 was performed without using the Mylar film. Low track densities resulting from this experiment seem to indicate that all experiments could have been performed without Mylar. Oriani used Mylar to protect the CR-39 from potential chemical effects. Most tracks on control chips were probably due to natural background, most likely due to radon, radium, etc. Concentrations of such substances are known to be location dependent. That fact was probably responsible for differences between mean track densities on the control chips of individual researchers.

4. Conclusion: Facts and Interpretations

The purpose of our four independent investigations was to find evidence for reproducible emission of nuclear particles described in Oriani's 2008 paper [4]. No such evidence was found. We examined 40 CR-39 surfaces and found only three cases of excessive tracks. No excessive tracks were found on the remaining 37 surfaces.

We do not know why we were not able to observe emission of charged nuclear particles (in a chemical process similar to Oriani's). There could have been a setup or procedural difference between the experiments. Further experimentation is needed to confirm the existence of nuclear particle emission in light water electrolysis, understand the causes of such

Table 1. Track densities on 34 surfaces of our experimental chips. No Mylar was used in Experiment No. 12.

Exp. No. ID	Experimental chip (upper side) tr/cm^2	Experimental chip (lower side) tr/cm^2	Control chip averages tr/cm^2
1 P.L.	20	28	
2 P.L.	125	148	
3 P.L.	89	94	59 (from five surfaces)
4 P.L.	99	91	
5 M.H.	128	118	
6 M.H.	128	192	192 (from eight surfaces)
7 M.H.	278	267	
8 J.D.	121	113	125 (from six surfaces)
9 J.D.	87	85	
10 L.K.	21	23	
11 L.K.	24	24	
12 L.K.	18	8	
13 L.K.	> 10000	11	
14 L.K.	10	> 10000	
15 L.K.	> 10000	17	14 (from 16 surfaces)
16 L.K.	31	20	
17 L.K.	23	8	
18 L.K.	12	24	
19 L.K.	10	8	
cr 20 L.K.	11	3	

emission and determine the conditions required for reproducibility.

Appendix: An Example of Statistical Analysis

The mean and standard deviation on eight of P.L.'s experimental surfaces were 87 and 46 tr/cm^2. The mean and standard deviation on his five control surfaces were 59 and 38 tr/cm^2. Is the difference between mean track densities on experimental and control surfaces, 87–59 = 28 tr/cm^2 statistically significant? To answer this question one must estimate uncertainties associated with actually measured track densities (87 and 59). The uncertainty about 87 is 46/2.8 = 16, where 46 is the reported standard deviation while 2.8 is the square root of 8 (the sample size consisted of eight surfaces). Likewise, the uncertainty about 59 was 38/2.23 = 17 tr/cm^2, where 2.23 is the square root of 5 (the sample size consisted of five surfaces).

The difference of 28 tr/cm^2 is too small to be significant, considering large uncertainties associated with the reported mean densities (59 and 38 tr/cm^2).

The same approach was used to show that differences between mean values on experimental and control chips of –7, –27, and 2 tr/cm^2, as reported by M.H., J.D., and L.K. respectively, were also statistically insignificant.

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Dr. Richard Oriani helped us to design experimental set-ups; Dr. Helen Roberts helped us to perform statistical analysis of experimental data. Constructive criticism from two anonymous referees was also highly appreciated.

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Rossi's Reactors – Reality or Fiction?

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A tabletop prototype of a new kind of nuclear device was demonstrated at the University of Bologna, several months ago. It generated thermal energy at the rate of 12 kW. A set of one hundred of such interconnected devices, able to generate energy at a much higher rate (up to 1000 kW) is said to be now commercially available. The inventor claims that the energy was produced via nuclear fusion of hydrogen and nickel. This note addresses conceptual difficulties associated with such interpretation. Experimental facts reported by the inventor seem to conflict with accepted knowledge. This, however, should not be a justification for the rejection of experimental data. Refutations and confirmations should be based on independently performed experiments.

1 Introduction

An interesting website, describing an ongoing research project, has been created by an Italian engineer Andrea Rossi [1]. He is the inventor of a tabletop device in which powdered nickel, mixed with common hydrogen, reported to generate thermal energy at the rate of 12 kW, for six months. A large percentage of nickel was said to be converted into copper, during that time. The device was recently demonstrated at the University of Bologna. The most obvious questions, raised by the reported features of the reactor are:

1. What lowers the coulomb barrier, between the atomic nuclei of hydrogen and nickel?
2. Is the reported accumulation of copper consistent with the well known half-lives of radioactive copper byproducts?
3. Is the measurable isotopic composition of nickel, in spent fuel, consistent with the amount of released energy?
4. The radiation level, outside the operating 12 kW reactor, was said to be comparable to that due to cosmic rays. Spent fuel, removed from the reactor, one hour after the shutdown, was found to be not radioactive [1]. How can these purported facts be explained?

Results from earlier experiments (2008 and 2009) are described in [2]. In one case the device was used to heat a "small factory" (probably two or three rooms) for one year.

2 Reported 2011 results

One demonstration of the device – January 14, 2011, at the University of Bologna – is described in [3–5]. Subsequent experiments – February 10, and March 29, 2011 – are described in [6–8]. In both cases the apparatus consisted of a cylinder containing nickel. Pure hydrogen was forced to flow through the hot nickel powder. The amount of powder was 100 grams [8, 9], or slightly more than one cubic inch, depending on the level of compression. Reactions between nickel and hydrogen turned out to be extremely exothermic, generating thermal energy at the rate of about 12.4 kW. This was 31 times higher than the rate at which electric energy was supplied, to operate the equipment [4].

In the February experiment the amount of thermal energy was determined from the flow rate of cooling water, and the difference between its input and output temperature. In the January experiment the water flow rate was slower; the entering water was a liquid, the escaping water was a vapor. The amount of thermal energy released was determined from the amount of liquid water (initially at 15 ^{o}C) transformed into 101 ^{o}C vapor. Rossi claims that most heat is produced from nuclear reactions:

$$p + Ni \rightarrow Cu,$$

where p is nothing but ionized hydrogen. This is very surprising because the temperature of hydrogen was below the melting point of nickel. Addressing this issue in [10] Rossi reported that about 30% of nickel was turned into copper, after six months of uninterrupted operation. A schematc diagram of the reactor, and additional details are in [11, 12].

Comment 1

Many physicists have studied fusion of protons with nickel nuclei. But their protons had much higher energies, such as 14.3 MeV [13]. Rossi's protons, by contrast, had very low energies, close to 0.04 eV. The probability of nuclear fusion, expressed in terms of measurable cross sections, is known to decrease rapidly when the energy is lowered. How can 0.04 eV protons fuse with nickel, whose atomic number is 28? Rossi is convinced that this is due a catalyst added to the powdered nickel. The nature of the catalyst has not been disclosed. This prevents attempts to replicate the experiments, or to discuss the topic theoretically. Secrecy might make sense in some business situations, but it is not consistent with scientific methodology.

Comment 2

How can 30% of nickel in Rossi's reactor be transmuted into

copper? This seems to be impossible, even if the coulomb barrier is somehow reduced to zero by his catalyst. To justify this let us focus on the ^{58}Ni and ^{60}Ni isotopes–they constitute 94.1% of the nickel initially loaded into the device. The reactions, by which copper is produced, from these isotopes, would be:

$$p + {}^{58}Ni \rightarrow {}^{59}Cu \quad \text{(half-life is 3.2 s)} \qquad \text{(A)}$$

and

$$p + {}^{60}Ni \rightarrow {}^{61}Cu \quad \text{(half-life is 3.3 h)} \qquad \text{(B)}$$

The reported amount of accumulated copper – 30% of the initial nickel being turned into copper, after six months of operation–would indeed be possible, via reactions (A) and (B), if the produced copper isotopes were stable, or had half-lives much longer than six months. But this is not the case, as shown above. The produced copper isotopes, ^{59}Cu and ^{61}Cu, rapidly decay into ^{59}Ni and ^{61}Ni. Each reaction, in other words, would lead to accumulation of these isotopes of nickel, not to accumulation of copper, as reported by Rossi. The accumulation of copper would practically stop after several half-lives. Note that ^{63}Cu and ^{65}Cu, if produced from fusion of protons with ^{62}Ni and ^{64}Ni, would be stable. But natural abundance of these isotopes of nickel, 3.63% and 0.92%, respectively, is too low to be consistent with the claimed accumulation of 30% of copper.

Comment 3

How much of the original ^{58}Ni should be destroyed, after six months of continuous operation, in order to generate thermal energy at the rate of 12 kW? Let us again assume that Coulomb barriers are somehow reduced to zero by Rossi's secret catalyst. The ^{58}Ni is 68% of the total. On that basis one can assume that 68% of 12 kW is due to the radioactive decay of ^{59}Cu, and its radioactive daughter, ^{59}Ni. Thus $P'_1 = 0.68 \times 12 = 8.16$ kW. This is the thermal power. The nuclear power P_1 must be larger, because neutrinos and some gamma rays do escape from the vessel. As a rough estimate, assume that the nuclear power is

$$P_1 = 16 \text{ kW} = 16{,}000 \text{ J/s} = 10^{17} \text{ MeV/s}.$$

The excited ^{59}Cu, from the reaction (A), releases 3.8 MeV of energy, as one can verify using a table of known atomic masses. In the same way one can verify that the energy released from its radioactive daughter, ^{59}Ni, is 4.8 MeV. In other words, each transformation of ^{58}Ni into ^{60}Ni releases 3.8 + 4.8 = 8.6 MeV of nuclear energy.

The number of reactions (A) should thus be equal to $10^{17}/8.6 = 1.16 \times 10^{16}$ per second. Multiplying this result by the number of seconds in six months (1.55×10^7) one finds that the total number of destroyed ^{58}Ni nuclei is 1.80×10^{23}, or 17.4 grams. A similar estimate can be made for other initially present nickel isotopes. The overall conclusion is that the isotopic composition of nickel, after six months of operation, at the 12 kW level, would change drastically, if the reaction A were responsible for the heat produced in the reactor invented by Rossi.

The amount of ^{59}Ni, for example, would increase from 0% (natural abundance) to 17.4%. The amount of ^{58}Ni, on the other hand, would be reduced from 68% (natural abundance) to 50.6%. The isotopic composition of nickel in spent fuel was measured, according to [1], but results remain "privileged information".

Comment 4

The level of radioactivity, next to the reactor generating heat at the rate of 12 kW, was reported as not much higher than the natural background [5]. Is this consistent with reaction (A) being responsible for most of the heat? The answer is negative. How can this be justified? In the steady state the rate at which radioactive atoms, in this case ^{59}Cu, are decaying is the same as the rate at which they are produced. That rate, as shown in Comment 3, is 1.16×10^{16} atoms per second. In other words, the expected activity is

$$1.16 \times 10^{16}/3.7 \times 10^{10} = 313{,}000 \text{ Curies}.$$

The emitted radiation would include gamma rays of 1.3 MeV, able to escape. The level of radiation, next to the reactor, would depend on the wall thickness. It would certainly exceed the background by many orders of magnitude. Absence of excessive gamma radiation might be an indication that the reactions producing heat were different from the p+Ni fusion.

3 Addendum

Note that the reported fuel power density of 120 W/g would be at least ten times higher than in a fuel element of a nuclear reactor based on ^{235}U. What can be more desirable than higher safety and lower cost? Did Rossi really invent a new kind of nuclear reactor? Logical speculations, such as those above, are not sufficient to answer this question. Only independently performed experiments can do this.

Rossi's claims, if confirmed, would present a challenge to theoretical physicists. Physics, unlike mathematics, is based on confirmed experimental facts, not on axioms. Newly discovered facts often lead to improvements of accepted theories. Let's hope that Rossi's incredible results can be independently confirmed in the near future.

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LETTERS TO PROGRESS IN PHYSICS

Social Aspects of Cold Fusion: 23 Years Later

Ludwik Kowalski

The field of Cold Fusion, now called Condensed Matter Nuclear Science (CMNS), remains controversial. The original 1989 claim made by M. Fleischmann and S. Pons was that a chemical process in an electrolytic cell could initiate a nuclear reaction–fusion of two deuterium nuclei. More recent CMNS claims, made by experimental scientists, are: emission of charged nuclear projectiles during electrolysis; accumulation of ^{4}He; production of radioactive isotopes; and transmutation of elements. In the US, CMNS claims have been evaluated in two Department of Energy (DOE) investigations, in 1989 and 2004, as summarized in this article. These investigations did not lead to any resolution of the controversy. Scientists and adminstrators are not ideal; competition among them, as among other groups of people, tends to have both positive and negative influences.

1 Introduction

The so-called "scientific methodology", a set of norms developed to deal with difficulties, especially with mistakes and controversies, is well known. Most scientific mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other researchers. Scientific results, if valid, wrote Huizenga [1], must be reproducible on demand. "When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press." The scientific process, in other words, is self-corrective. The purpose of this presentation is to analyze an ongoing controversy about the so-called "cold fusion" (CF). The author of this article, and three other researchers, tried to verify one recent CF claim – emission of alpha particles during electrolysis. The results were negative, as described in [2]. Critical analysis of some CF claims, as illustrated in [3], can enrich nuclear physics courses, even at the undergraduate level.

Why is the CMNS controversy started in 1989 unresolved? Because CF claims are still not reproducible on demand, and because they conflict with accepted theories. A theory, in this context, is not just a hypothesis, or only a logical/mathematical argument. It is a logical structure that is known to agree with a wide range of already verified experimental data. Researchers know the rule–theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on additional verifications of new experimental results. Referring to such situations, Huizenga wrote: "There are occasionally surprises in science and one must be prepared for them." Theories are not carved in stone; scientists do not hesitate to modify or reject theories when necessary. Rejecting a highly reproducible experimental result "on theoretical grounds" would not be consistent with scientific methodology. Unlike mathematics, science is based, in the final analysis, on experimental data, not on logical proofs.

2 The Original Claim

It is well known that two hydrogen nuclei can fuse, releasing energy. But this happens only at extremely high temperatures. At ordinary temperatures the probability of the reaction is practically zero, due to the well known coulomb repulsion of positive nuclei. This has been confirmed by reliable experimental data. But two scientists – Steven Jones, a physicist, and Martin Fleischmann, a chemist – independently speculated that this might not always be true. The term CF was introduced by them to identify the claimed fusion of hydrogen nuclei (ionized atoms dissolved in solid metals). The DOE supported Jones' work long before Fleischmann and his colleague Pons (F&P) applied for similar support. That is why the DOE asked Jones to evaluate the new research proposal. He was later accused (by the administration of Utah University) of stealing the idea of CF from F&P. Trying to establish priority, Utah University organized a press conference (March 23, 1989) at which the discovery of generation of nuclear heat in an electrolytic cell was announced to the world. The released heat was declared to be due to fusion of deuterium nuclei – ionized atoms dissolved in palladium. At that time Jones and his co-workers had already authored numerous peer-reviewed articles [4]. But their claim was not excess heat; it was emission of neutrons.

3 The First DOE Investigation

Most scientists immediately rejected claims conflicting with well-known facts and theories. But many attempts to replicate F&P's poorly-described experiments were made. Some attempts were successful (unaccounted heat was generated at rates close to one watt), while others were not [5]. That was the beginning of the controversy. Fleischmann and Pons wanted to study the CF phenomenon for another year or so but

were forced to announce the discovery by the university administrators [6]. They had no evidence that the measured heat was due to a nuclear reaction. The only thing they knew was that it could not be attributed to a known chemical reaction.

Suppose their experimental results had been described without any interpretation, and the phenomenon had been named "anomalous electrolysis". Such a report would not have led to a sensational press conference; it would have been made in the form of an ordinary peer review publication. Only electrochemists would have been aware of the claim; they would have tried to either confirm or refute it. The issue of "how to explain excess heat" would have been addressed later, if the reported phenomenon were confirmed. But that is not what happened. Instead of focusing on experimental data (in the area in which F&P were recognized authorities) most critics focused on the disagreements with the coulomb barrier theory. Interpretational mistakes were quickly recognized and this contributed to the premature skepticism toward their experimental data.

But the significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by "vested interests" of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation. A panel of scientists, named ERAB (Energy Research Advisory Board), and headed by John Huizenga, was formed to investigate CF in 1989. The final report, submitted to the DOE several months later, interfered with the normal development of the field. It should be noted that ERAB scientists investigating the CF claims were not personally involved in replications of experiments. Their report [7], based on visits to several laboratories rather than participation in experiments, can be summarized by the following statements:
Conclusions:

1. There is no evidence that a nuclear process is responsible for excess heat.
2. Lack of experimental reproducibility remains a serious concern.
3. Theoretically predicted fusion products were not found in expected quantities.
4. There is no evidence that CF can be used to produce useful energy.
5. The CF interpretation is not consistent with what is known about hydrogen in metals.
6. The CF interpretation is not consistent with what is known about nuclear phenomena.

Recommendations:

7. We recommend against any extraordinary funding.
8. We recommend modest support for more experiments.
9. We recommend focusing on excess heat and possible errors.
10. We recommend focusing on correlations between fusion products and excess heat.
11. We recommend focusing on the theoretically predicted tritium in electrolytic cells.
12. We recommend focusing on theoretically predicted neutrons.

Note that only one conclusion (item 2) refers to CF experiments. Conclusion 4 is about anticipated practical uses of CF while the remaining four conclusions (1, 3, 5, and 6) are about various aspects of the suggested interpretation of experimental results. Instead of focusing on reality of excess heat critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion. The same observation can be made about recommendations. Only one of them (item 9) refers to possible errors in experiments. Items 7 and 8 refer to future funding while items 10, 11, and 12 refer to what was expected on the basis of the suggested hot-fusion interpretation. It is clear that the ERAB observations were based mostly on "theoretical grounds,"and not on identified errors in experimental data. Recommendations about future financial support for CF were very important. But they were ignored by the DOE. Support for CF research practically stopped in 1989. Another result of the first DOE investigation was that editors of some scientific journals stopped accepting articles dealing with CF research. Why was the scientific methodology of validation of claims – theories guide but experiments decide – not followed by the DOE-appointed scientists? Why did "rejections on theoretical grounds" prevail?

4 The Second DOE Investigation

The second DOE investigation of CF was announced in March 2004, nearly 15 years after the first one. Links to three online documents related to that investigation – Conference Agenda, Meeting Agenda, and DOE CF Report – can be found in [8]. The six most important scientific questions, based on new experimental claims, were:

a) Is it true that unexpected protons, tritons, and alpha particles are emitted [9, 10] in some CF experiments?
b) Is it true that generation of heat, in some CF experiments, is linearly correlated with the accumulation of ^{4}He and that the rate of generation of excess heat is close to the expected 24 MeV per atom of ^{4}He [9, 11]?
c) Is it true that highly unusual isotopic ratios [9, 12] have been observed among the reaction products?
d) Is it true that radioactive isotopes [9, 13] have been found among reaction products?
e) Is it true that transmutation of elements [10, 14] has occurred?

f) Are the ways of validating of claims made by CF researchers (see conference reports presened at [16, 17, 18]) consistent with accepted methodologies in other areas of science?

A positive answer to even one of these questions would be sufficient to justify an official declaration that cold fusion, in light of recent data, should be treated as a legitimate area of research. But only the (b) question was addressed by the selected referees [8]. They were asked to review the available evidence of correlation between the reported excess heat and production of fusion products. One third of them stated that the evidence for such correlation was conclusive. That was not sufficient; the attitude of the scientific establishment toward cold fusion research did not change.

5 Conclusion

The CF controversy is unprecedented in terms of its duration, intensity, and caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. CMNS researchers are mostly also Ph.D. level scientists. The same is true for those scientists who believe that the announced discovery of CF was a "scientific fiasco". We are still waiting for at least one reproducible-on-demand demonstration of a nuclear effect resulting from a chemical (atomic) process. In the case of CF the self-correcting process of scientific development emphasized by Huizenga has not worked. This fiasco seems to be due to the fact that scientists appointed to investigate CF claims did not follow the rules of scientific methodology.

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Cold Fusion, hot and cold

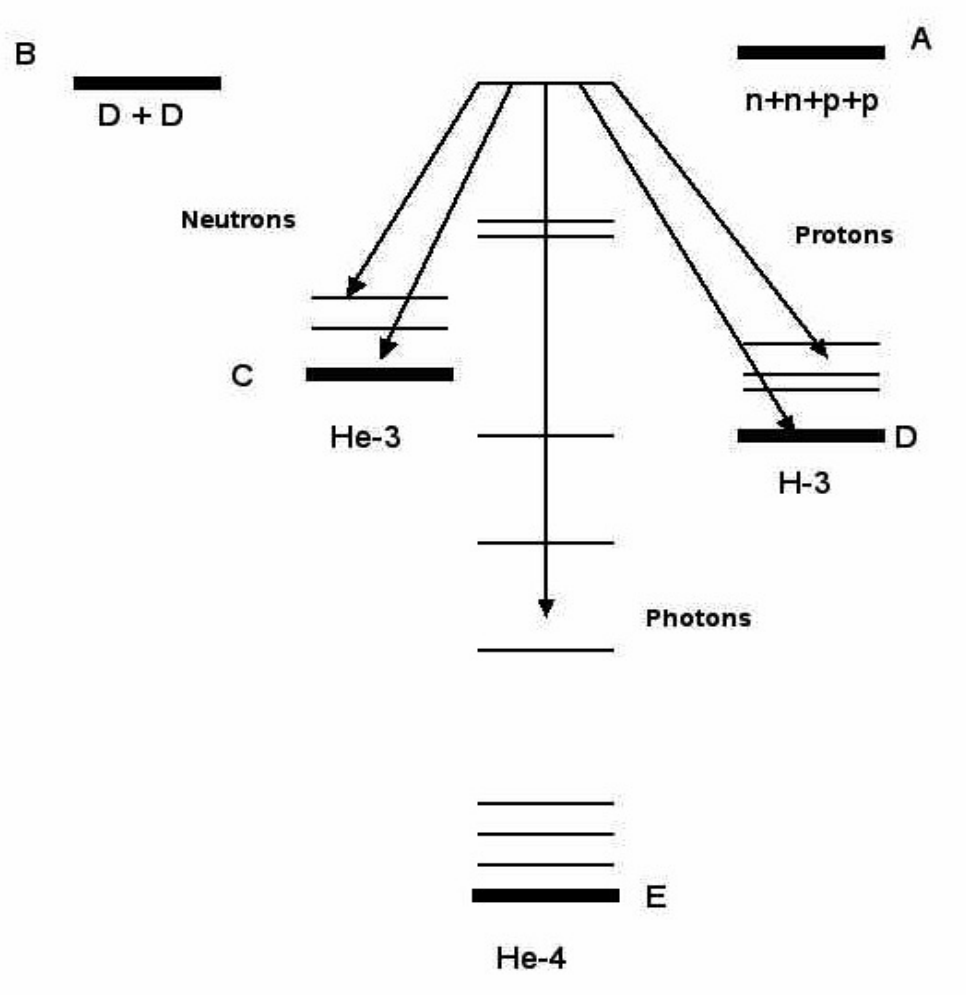
This unit is the summary of what I read from a paper of CMNR researchers, A. Meulenberg and K.P. Sinha (both from Malaysia). Their paper, entitled " Extensions to Physics: Low-EnergyNuclear Reactions," has been presented at the most recent "International Conference on Cold Fusion" (ICCF17) in Korea (September, 2012). That paper prompted me to post the following two messages, on the private Internet forum for CMNR researchers.

Message 1(9/18/2012):

1) Suppose two 2H (whose combined atomic mass is 2*2.01410=4.0282 u) fuse to form one 4He (atomic mass of 4.00226 u). The excitation energy of the "compound" 4He becomes 24 MeV, as calculated with the E=m*c^2 formula. This is trivial. Why are the quotation marks? Because the name compound nucleus is used only for much heavier structures (A>30). Which name would be more appropriate? In any case, the name has nothing to do with the question about the distribution of energy.
2) One possibility is that most of the released 24 MeV becomes the kinetic energy of one neutron, while the kinetic energy of the remaining 3He structure is negligible, or vice versa. Another possibility is that most of the released 24 MeV becomes the kinetic energy of one proton, while the kinetic energy of the remaining 3H structure is negligible, or vice versa. And what about a possibility that the pp and nn (structures inside the excited 4He) share equal amounts of energy among them? These temporary structures can be called biproton (2He) and bineutron. Such speculations are triggered by reading one of the papers written by Akito Takahashi.
3) And here is another question; it has to do with hot fusion. The probability of the 3He+n decomposition and the probability of the 3H+p decomposition (of the excited 4He) are practically identical. This is a well-known experimental fact. How can it be explained?

Message 2 (9/20/2012)

Below is a diagram that I have in mind while thinking about the D+D fusion, either hot or cold. The thick line A refers to combined mass of two neutrons and two protons. Likewise, the thick line B represents the combined mass of two deuterons. It is easy to verify, by using a table of atomic masses, that B is smaller than A. That is why the line B is below the line A.



The line C represents the combined mass of one neutron and one unexcited He-3 (ground state). It is easy to verify that C is smaller than B. That is why the C line is lower than B. Likewise, the line D refers to the combined mass of one neutron and one tritium, also in the unexcited (ground) state. It is easy to verify ...; that is why the line D is below the line C. And, as you probably guess, the line E represents the mass of He-4, in the ground state. As I demonstrated yesterday, the rest mass E is by nearly 24 MeV smaller than the rest mass B.

The thin horizontal lines refer to excited states of corresponding nuclei, He-4, He-3 and H-3, respectively. Fusion of two slow deuterons produces He-4 in the excited state; the level of that state is the same as the level of B, or slightly higher (when the kinetic energy of fusing deuterons is not negligible).

The arrows show what is energetically allowed. One possibility is emission of one neutron and one He-3 nucleus, either in the ground or excited state. Another possibility is emission of one proton and the H-3, either in the ground state or in the excited state. The third energetically allowed decomposition of the excited He-4, is emission of one or several (Andrew calls it a cascade) photons. Evaporation of two neutrons and two protons from the He-4 compound nucleus, on the other hand, is energetically impossible, unless the kinetic energy of fusing deuterons is larger than the difference between the A and B levels.

The nuclear "ash" associated with emission of a single neutron is He-3, the nuclear "ash" associated with emission of a single proton is H3, and the nuclear "ash" associated with the emission of photons is He-4.

It is interesting that the maximum "energetically-possible" energy released via the photon de-excitation is much higher than the maximum "energetically possible" energy released via the n or p channels.

The p and n channels, as we know, are dominant in hot fusion but not in cold fusion. Why is ir so? This is one of the puzzles to be solved. Another puzzle has to do with the reported accumulation of the He-4 ash, in cold fusion. Why is it not accompanied be emission of the matching number of high energy photons?

Feel free to use this post in any way you wish. But be aware that vertical distances between the horizontal lines, in my illustration, are not drawn according to energies calculated from known masses of atomic nuclei. The same applies to excitaion energy levels; their numbers and locations, for three nuclei, are not realistic.

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411) Messages from the CMNS list (December 2012)

Ludwik Kowalski; 12/17/2012

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Some of you might be interested in the following messages from the private discussion list for CMNS researchers. They were posted in the first week of December 2012.

1) Posted by X1:

... **X2 Ludwik Kowalski** suggests that some of our distinguished CMNS scientists are in a way accomplices of Rossi's scam..... [I am certainly not one of them; my critical comments on Rossi's claims can be seen at:]

http://pages.csam.montclair.edu/~kowalski/cf/408rossi.pdf

2) Posted by X3:
I have not yet received a response from X2. Regarding my wager, I am confident that commercial hot fusion energy will not happen in my lifetime despite hearing this promise of abundant energy for as long as I can remember.

3) X6 :
I also do not expect to live long enough to see commercial applications. But should I expect to see the first reproducible-on-demand demonstration of an undeniably nuclear effect resulting from a chemical process, such as electrolysis? This would be a giant step toward practical applications.

4) Posted by X5 (And ul-Rahman Lomax)
It exists. Unfortunately, it's a fairly expensive experiment. It's the X6's experiment. Use the state of the art to run a substantial series of F&P type cells to see excess heat. Run the cells in such a way as to allow the secure collection of helium and measure it. Compare excess heat and the amount of helium produced. The helium will be proportional to the heat, and if you end up capturing all the helium, which may take some special techniques, the ratio will be as expected for deuterium conversion to helium. The individual cells will vary in heat, but the ratio will be constant.

That is a reproducible experiment, it's been reproduced many times. There are approaches which have shown excess heat in most cells, such as the Energetics Technologies replications at SRI and ENEA.

It's much less expensive to do this without the helium collection, but then all you have is heat, which is not an undeniably nuclear effect.

The problem, Ludwik, is that the F&P Heat Effect produces essentially no "nuclear products" other than helium, which is not unmistakably "nuclear" by itself, unless you take the levels above ambient, and still the skeptics will carp, because they did. However, heat correlated with helium at the fusion ratio is strong

enough evidence for anyone who is reasonable.

It's possible that this could be done with tritium, but I don't see that the *reliable production* of tritium has been studied. The rumor is that tritium is not correlated with heat, but I've never seen published values that would show this, and it's a suspicious claim.

5) Posted by X6:

Yes indeed. Production of ^{4}He from ^{2}H, even without generation of excess heat, is an undeniable nuclear event, like other reported transmutations. But the correlation with excess heat, at the rate of about 24 MeV per atom of He (even if it were 24 +/- 10 MeV), would be very significant.

How much would it cost to reconstruct a setup, and to perform ten experiments? Who would be able to perform such experiments, if money becomes available?

6) Posted by X7

Evidently X2 has not done his research! None of the persons criticized in his blog are ISCMNS leaders! And his conclusions regarding the ISCMNS position do not seem to be based on any relevant facts.

I went on record, during the ISCMNS Annual General Meeting at ICCF16 (February 2011) warning the community of the brewing storm. Allow me to quote some key points from my presentation:-

"Recently a demonstration was made of a prototype energy “Catalyzer”
If it works as described, it may be a blessing to humanity and vindicate 21 years of patient work by this community. If it fails spectacularly, it will create bad publicity for everyone working in the field.

Some advice to inventors
Get your invention independently validated.
Demonstrations which hide technical details create unease.
Non disclosure agreements can protect secrets.

Advice to Users
If you acquire any technology, whether secret or not, do not accept any clauses which require you to keep quiet if it doesn't work.
We need whistle blowers.

Advice to Evaluators
It's probably not appropriate to make a public statement in support of a demo miracle device, if you have not examined it yourself.
If you do make a statement, at least make sure that you can correct any eventual errors.
Take care if you get on film, as film will be edited."

This is of course a personal perspective, but it was discussed by the ISCMNS members present at the meeting.

If X2 has any evidence of fraud, I suggest he contacts the appropriate authorities.

7) Posted by X5:

First of all, it's been done. As I recall, Miles performed about six experiments, taking a total of 33 samples for analysis. ....This is the kind of work that can be done in many ways. The exact protocol is not important,

but I do caution against going outside the basic PdH approach. Other approaches *might* involve different mechanisms.

If one can obtain or make an active cathode -- ENEA seems to be able to supply functional cathode material, and seems to have a grip on what sets up the necessary initial conditions -- measuring heat is not the most difficult part of this; one should, of course, use good calorimetry, for the accuracy of the ratio will not exceed the accuracy of the calorimetry.

The difficulty, though, is in capturing and measuring all the helium. McKubre followed an approach, in some of his work, that involved rigorously excluding helium from the cell materials and cells. Helium can diffuse through some materials. Seals must be helium tight, and tested to be so. And if the cell needs to be disassembled for any reason -- connections fail, etc., -- then the whole process must be repeated.

Storms, in "Status of cold fusion (2010)", working from the results of various studies, comes up with 25 +/- 5 MeV/He-4. That's rather obviously a bit seat-of-the-pants. I'd say, however, that the results show better than 24 +/- 10 MeV (and I'm not saying that Storms' result is incorrect).

At this point, the work is solid enough that the default hypothesis as to the ash from the FPHE is that it is helium, with the fuel being deuterium. Transmutations and other products are found at levels far too low to explain the heat, by many orders of magnitude. This does *not* establish mechanism, but it obviously puts some severe constraints on mechanism. If the mechanism involves neutron formation, why the products would so tightly focus on helium would have to be explained -- or other products would need to be identified, which has not happened. One possible mystery product, of course, could be deuterium, since it would not be detectable in heavy water experiments, nor, for that matter, in light water experiments, so plentiful is deuterium in light water.

Miles first reported helium somewhere around 1991, and his first extensive correlation report was published in time to be covered in the second revision of Huizenga's book, "Cold fusion, scientific fiasco of the century." Huizenga was highly impressed, in fact, saying that, if confirmed, a major mystery of cold fusion would have been solved, i.e., the ash. He held on to his skepticism by saying that, of course, it was unlikely to be confirmed, because no gamma rays were reported.

Huizenga was showing, clearly, how the skeptics thought about cold fusion and why they thought "it" was impossible. "It" was d-d fusion, through "overcoming the Coulomb barrier," in the classic way or something like it. And "it," when it produces helium -- i.e., rarely -- always produces a gamma ray. I consider it likely that they were correct, what they thought of as cold fusion is indeed impossible. They were gloriously and spectacularly incorrect, though, in making the assumption that if there was cold fusion, it would be a new way of making hot fusion.

(And all the theories that involve ideas whereby somehow deuterons in condensed matter attain sufficient energy to directly penetrate the barrier are missing the point. That is not happening. Piezoelectric fusion -- used in certain commercial neutron generators -- isn't cold fusion, it's hot fusion, and that's why it serves to generate neutrons. But the apparatus is at room temperature ....)

Because a notable author objected to the idea of this "replicable experiment," I'll answer his post separately, as to why what he expects has not appeared. But what I described is indeed replicable, and reliably so, I'll assert -- there is going to be a need for some detailed discussion about this -- and *it has been replicated*, quite enough that under normal conditions, the result would be a generally accepted fact.

Sitting here twenty years after a cascade, though, conditions still are not normal.

8) Also posted by X5, shortly after the above message:

X6 asked “How much would it cost to reconstruct a setup, and to perform ten experiments? Who would be able to perform such experiments, if money become available?”

What it would cost is something that could be estimated by those who have done the work in the first place. Notably, as to those who are active, and off the top of my head, this would be Miles -- first and foremost --, McKubre, who did the most accurate work to date, and Violante, who may have done the work at least expense, plus, of course, any of their co-workers and those reported in Storms, 2010.

I doubt that it would cost more than $10,000 per cell, though, as a rough guess, particularly if a worker already had good calorimetry in place or easily adaptable. If a lot of cells are run, the cost per cell may go down. Most of this cost, indeed, is labor.

As to who, my plan is to write a survey of cold fusion criticism, with a goal toward identifying significant and important unresolved issues. The replication of heat/helium is not significant as far as it is not impeding progress significantly, but because there are lingering doubts about it, it may be politically important. If heat/helium is established, if 24 MeV is confirmed, independently, and with greater accuracy, it confirms cold fusion, very amply, as a side-effect, and it narrows the possibilities for theories as to mechanism.

Matters are still at the point where Larsen can suggest that 24 MeV is only approximate and he can attempt to shoehorn his neutron transmutation ideas into it. Note that in spite of what Krivit has implied, Larsen has *confirmed* at least some of McKubre's work, as to his personal opinion.
This work must be divorced from theory. The goal of any confirmation should be, not to confirm or reject any theory as to mechanism, but simply to measure the ratio of helium to heat. The experiments might as well look for other things that can be done without compromising the heat/helium goal.

An important approach may be to define a protocol to be followed, and the broader the consensus on the protocol, the more likely that multiple workers will attempt it. Because few have access to mass spectrometers that are helium-qualified (He-4 must be resolvable from D2), the protocol will need to include a sampling protocol, which will require cooperation between experimenters and labs ready to do the measurements. If a single and simple protocol for submitting samples is followed, actual helium measurement should be relatively cheap per sample.

If every researcher does their own fabrication, that's expensive. If a common protocol is agreed upon, with identical cell design, there is *no harm in cooperation in fabrication.* What would be important would be that the cell materials would all be accessible for thorough testing. I.e., someone could analyze them to make sure that someone didn't sneak helium into the palladium, in particular. Ideally, there would be an independent supplier of materials and cells, with traceability. All that a researcher, then, in a report, need state, is that they used XYZ company's model NNN cell assembly.

XYZ company, then, is highly motivated to facilitate consensus among its potential customers as to desirable cell design. The Galileo project would have seen much wider participation if there had been such a common fabrication supplier. Indeed, I began working as a supplier of kit materials because, I saw, it should be possible to supply a Galileo-type cell, ready to hook up to a power supply and run, for about $100 per cell *and make a (modest) profit doing it.*

(But that design only looks for radiation evidence, from small palladium-plated cathodes in heavy water, and is utterly inadequate, as such, for heat/helium work.)

9) Posted by X6:

Thank you for interesting posts, X5. You are probably assuming that a high resolution mass spectrometer (able to distinguish the D-2 peak from the He-4 peak) would be available at no cost. Such instruments are not disposable.

10) Another post by X5

Other reported transmutations would be nuclear, but they occur at levels far, far below those of helium in F&P type experiments. Helium itself is problematic because helium is present in ambient air at levels that are generally higher than those expected from the heat. However, that has been addressed in several ways:

1. If enough heat is accumulated, and helium is accumulated, the helium levels can be expected to -- and do -- rise above ambient, without slowing, indicating a source of helium other than leakage from ambient.

2. Controls do not show helium.

3. If an experiment shows reasonably robust heat, and the cell environment is small, helium as an elevation above ambient can be observed. That this is what Violante did escaped Steven Krivit, who criticized Violante without understanding what he'd done.

The big problem with heat/helium work is that helium has very low mobility in palladium, yet it appears that the reaction does implant helium at some (small) depth in the palladium, so as much as roughly half of the helium can be trapped in the palladium. McKubre attempted to flush the helium by repeated deuterium loading/unloading, which appears to have worked, but this is an unconfirmed technique, and it would be useful if more definitive methods could be used. For example, earlier work looking for nuclear products in Arata/Zhang DS cathodes (hollow palladium with palladium black in the interior) not only looked in the interior gas phase, but also sectioned the cathodes and heated the pieces; helium becomes mobile at high temperatures. I've also thought that dissolving the cathodes electrolytically might work and might be simpler, if a researcher doesn't have direct access to helium measurement and must send off samples to a lab.

(With those Arata/Zhang cathodes, helium was not found above ambient, and the signs are that the cathode interior volume was breached, the helium leaking out. What was found, though, was He-3, at very significant levels, apparently as a decay product from tritium. The He-3 was found trapped in the palladium, at variable distance from the interior, indicating that it was the product of tritium that had decayed to He-3, becoming immobile, as the tritium diffused through the palladium. But this is unconfirmed work; like much cold fusion work, it's crying out to be replicated.)

11) Also by X5:

There are those on this list with substantial experience with this, perhaps they will help us understand the issue.

However, Miles did not have such a spectrometer. It is not necessary, obviously, for the researcher running the cells to have a mass spectrometer.

SRI has the necessary device, so does Dr. Storms, in his home lab. They are quite expensive, but not impossibly expensive, and, in any case, it is probably a better idea to create a sampling protocol such that a lab or labs can provide analytical services, efficiently.

If one were to run 10 cells, that could only be 10 samples to analyze, plus a few controls. It's kind of crazy to buy a mass spec to make ten measurements, eh?

It does appear, from what I've heard, that modern mass spectrometers are both cheaper and more accurate than the services that were available to Miles.

Yes, for deeper investigational work, in-line, continuous measurement of cell gas could make the investment in a dedicated mass spec worthwhile. But, note: serious exploration of the parameter space leads to a concept of running many cells simultaneously. That can be done through a sampling protocol.

Maybe an advanced cold fusion lab would indeed have a mass spectrometer that could be used for in-line, real-time analysis, and then used for analysis of samples that are stored up for later study.

It looks like a helium mass spectrometer might be rentable for on the order of $2K - $3K per month. These are used as leak detectors. Used mass spectrometers seem to be going for $10K - $40K.

A Varian 979 Helium Mass Spectrometer Leak Detector is on offer on eBay, for quite some time, at $15,000.

I think it likely that someone with access to an adequate helium mass spectrometer would be willing to provide services at a reasonable cost. It's not impossible that such services could be donated. The cost of equipment does not seem to be so high that, if a analysis services are not available, a provider could be set up for that purpose. The real cost of heat/helium measurements, as to the labor of preparing the equipment, running the experiments, and collection of samples, is quite likely much higher than the cost of helium analysis.

P.S.
I cited some figures for helium leak detectors. I don't know how capable these are of separating out the D2 peak. I do know that low-mass mass spectrometers are readily available that can easily resolve the peaks. As I mentioned, Storms has one. D2 can also be eliminated from the gas stream, but that introduces a possible source of error.

It's pretty much a non-issue, really, because it is not necessary for the researchers to own a mass spectrometer. The key will be a sampling and testing protocol, especially one that allows storage of samples for extended periods if necessary. That could be difficult enough! But it is doable. And blinding the tests so that the helium testers don't know anything about the sample origins can cover a host of contingencies, assuming that control samples are included, some as ambient air, perhaps, some as coming from dead cells, etc.

12) Posted by X7:

Here is a typical university in-house rental fee for a mass spectrometer:

Students who have a demonstrated need for the unique capabilities of this instrument can be trained to run their own samples. The training is billed at a rate of $100 per hour, with the usual training session taking 4 hours. Up to four students can attend the same training session to divide the cost.

Non-routine samples submitted to us to be run on the Q-TOF are billed at $100 per hour.
Student use of the instrument is billed at $50 per hour.

13) Posted by X8:

X5, a leak detector is useless for separating He from D2. These instruments focus on mass 4 but they are

not designed to separate D2 from He. After all, no D2 is expected to be present in the apparatus being tested for leaks by applying He.

The only error is just how much He is present. Several methods can be used to reduce this error by calibration.

14) Posted by X9:

X8, Can your spectrometer distinguish D2 from He-4? Most cannot do this.

15) Posted by X8:

The spectrometer is made by MKS and has a range of mass 1 to 6. He and D2 are cleanly separated.

16) Posted by X10:

Folks, a brand new MKS MicroVision II for measuring deuterium versus helium cost about $12,000 according to the company rep. It operates at a pressure of 1E-5 torr(?). It has a ten week lead time to order.

17) Posted by X6:

The costs reported in this thread are clearly negligible, in comparison with how much the DOE has been spending yearly to support hot fusion research. Failure to perform replication of ^{4}He experiments, during the second DOE investigation, was certainly not due to prohibitively high costs. [That investigation was described in my article at]:

http://pages.csam.montclair.edu/~kowalski/cf/409social.pdf

In philosophically-oriented article (to be published in 2013?) I wrote that "the DOE experts were not asked to perform correlation experiments; they were asked to read the report submitted by five CF scientists (21), and to vote on whether or not the evidence for the claim was conclusive. Such a way of dealing with a controversy was not consistent with the scientific method of validation or refutation of physical science claims."

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412) Summary of a Recent Discussion

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Edmund Storms (1, 2) has been studying cold fusion (CF) phenomena since 1989. He now refers to them as LENR (Low Energy Nuclear Reactions). Today, on 2/22/2011, he posted the following brief summary of his ideas, at our private Internet list for CMNS researchers:

References

1) http://home.netcom.com/~storms2/index.html

2) E. Storms "Science of Low Energy Nuclear Reaction: A Comprehensive Compilation of Evidence and Explanations about Cold Fusion" at: http://www.amazon.com/Science-Energy-Nuclear-Reaction-Comprehensive/dp/9812706208

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"The phenomenon called LENR has several basic features that have to guide a model and were, ironically, the cause of its rejection. These features are:
1. The mass-energy is released in small quanta rather than as energetic particles, as is the normal case by nuclear reactions and hot fusion in particular.
2. The phenomenon is very rare on a geological time scale and difficult to replicate in the laboratory.
3. The nuclear products are not the expected ones based on experience with the hot fusion process.
4. The process only occurs in condensed matter, especially in certain solids.
5. The process does not require applied energy to be initiated although extra energy will increase its rate.

These features do not need additional demonstration or experimental detail to be accepted as real by a knowledgeable observer. The challenge is to create a logically consistent model that does not conflict with what is known about "conventional" nuclear reactions and is consistent with what is observed. The need for such an explanation, even thought it is incomplete, flows from the fact that this phenomenon is too complex to investigate successfully using trial and error. In fact, all experiments in science are guided at some level by an explanation, which is sometimes informal and based on current observed behavior but more often is based on established laws of Nature. The best model is the one that is consistent with the largest number of observations and makes accurate predictions about previously unseen behavior. These models are not designed to or are required to justify belief that the phenomenon called LENR is real. They are required to guide effective research that might eventually provide the required justification for acceptance.

To do this, a few assumptions are required. These assumptions must be consistent with the laws or rules known to apply to the chemical systems in which the LENR effect occurs. Agreeing on which assumptions are consistent with the required rules (laws) and which are not, has been the basic cause of conflict and argument about the proposed models.

Before listing the assumptions, we need to acknowledge that several nuclear processes and reactions can occur in a material at the same time. For the discussion to be clear, we need to focus on only one reaction at a time. Initially the discussion will focus on the most active reaction that results in the major amount of detected heat energy.
Several models propose processes other than fusion. These models involve either creation of neutrons or their release

from a stabilized form in the material. The resulting neutrons then interact with nuclei to form the observed nuclear products. This discussion is not focused on this claim other than to note that the observed behavior is not consistent with this process and many parts of the model conflict with basic laws of nature. Therefore, this path will not be explored here. The present discussion focuses only on fusion of hydrons as the process called LENR.

Three basic processes have to occur at the same location and at the same time. No significant delay may separate these three events. These events are:

A. Two or more hydrons must occupy the same location at the same time in the material.
B. Two or more hydrons must overcome the Coulomb barrier separating them.
C. The resulting reduction in mass-energy must be converted to heat-energy.

The basic assumptions used here are:

1. The behavior involves only one basic mechanism that occurs at the same basic location in the active material being examined.

2. The nuclear process can involve any isotope of hydrogen.

3. The entire process must be consistent with all known laws of physics and chemistry, although gaps in knowledge are accepted.

The above assumptions and observed behavior alone allow a useful model to be proposed. To start the process, the location of the nuclear process in the material must be identified. I call this location, the Nuclear Active Environment (NAE). Consequently, a new assumption is introduced that says:

4. The NAE is a new physical structure having no connection through quantum mechanical processes or the laws of thermodynamics with the atoms that form the lattice structure.
This assumption eliminates a number of proposed models from consideration, which is discussed later.

I have explained previously why I propose that the nuclear reaction occurs in cracks of a critical size, so I will not repeat this argument here. Once the crack forms, the three basic processes (A, B, C above) must take place in this structure. The model now must describe how this series of events happens.

First, the hydons that are present in the surrounding lattice as H+ or D+ must enter the crack and create a structure that is able to reduce the coulomb barrier. The only way this process has been seen to occur is either by applying enough kinetic energy to force the two nuclei together (hot fusion) or by insertion of a muon between two D. Both methods produce the typical and expected energetic particles. Use of ion bombardment has revealed that the electrons normally present in a material are able to reduce the magnitude of the Coulomb barrier for the conventional hot fusion process. Consequently, the logical implication is that electrons are also involved in the LENR process, but in a different way.

Regardless of their involvement, the Coulomb barrier reduction process must take place in a manner to allow the mass-energy to be released gradually in small quanta before the fusion process is complete. Otherwise, if mass-energy remains in the final structure, it must result in gamma emission to be consistent with known behavior. At this point in the model, we are faced with a dilemma. What process can be proposed that satisfies the observed behavior but does not conflict with known and accepted concepts in physics? All of the proposed models are faced with this dilemma while attempting to solve the problem different ways. The only question is which of the proposed methods (theories) provides the most logical description of observed behavior and best predictions, because they all contain the consequence of this dilemma. Can we focus the discussion on this dilemma?

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Philosophical and Social Aspects of the Cold Fusion Controversy

Presented at the

IVe Congrès de la Société de Philosophie des Sciences

June 1, 2012, Montreal, Canada

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Abstract

The area of research known as Cold Fusion (CF) is the arena of a science-and-society feud. The conflict is unprecedented in terms of duration, intensity, the caliber of combatants, and the deviation from basic principles of scientific methodology. It is also unprecedented in terms of two formal governmental investigations of the field. The US Department of Energy (DOE) has evaluated the CF claim--a chemical process triggering a nuclear process. The claim was rejected on "theoretical grounds," not by performing better experiments. The self-correcting nature of scientific progress, in both social and natural sciences, depends heavily on peer review. Depriving PhD-level scientists of that process, as practiced by editors of some scientific journals, violates the "New American Physical Society's Ethics Guidelines," described in the January 2003 issue of Physics Today. Such distortions of scientific methodology are worth studying.

1. Introduction

Scientific methodology (1,2) refers to the set of norms developed to deal with mistakes and controversies in scientific research. Most mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other researchers. Scientific results, if valid, wrote Huizenga, must be reproducible on demand. “When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press (3).” The process, in other words, is expected to be self-corrective.

The so-called "scientific method" is not a list of divine commandments. It is a set of norms described by scientists, and by those who observe their ways of working. The author of this article is a retired nuclear physicist who has observed cold fusion researchers over the last ten years, and participated in several CF conferences and three cooperative replication projects (in which the claimed results were not confirmed). The CF episode is an unusual controversy resulting from a sensational 1989 announcement made by Fleischmann and Pons (F&P). The event (4,5,6,7,8) divided physical scientists into two feuding camps (9,10,11,12,13,14). This is a rare example of a situation in which the expected self-correction of the scientific process was essentially stopped by two formal governmental interventions.

2. Role of Accepted Scientific Theories.

Why is the CF controversy unresolved? Because CF experimental claims are not reproducible on demand, and because they conflict with the generally accepted theory of nuclear reactions. A theory, in this context, is a logical/mathematical structure that agrees with a wide range of already verified experimental data. Scientists know the rule--theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on additional verification of new experimental results.

Referring to such situations, Huizenga wrote (3): “There are occasionally surprises in science and one must be

prepared for them." Theories are not carved in stone; scientists do not hesitate to modify or reject them when necessary. Rejecting a claim because it conflicts with a theory is not as convincing as rejecting it on the basis of reliable empirical data. In that sense methods of validation of claims in physical and social sciences are similar. Scientific theories are models of objective reality; they are often changed, or modified, when new facts are discovered.

3. Levels of Confidence in Scientific Claims: Data and Explanations.

A discovered experimental fact is usually presented to the scientific community, to be independently confirmed or refuted. Experimental results are accepted--at a high level of confidence--when they become reproducible on demand. Absence of such reproducibility justifies suspicion of possible errors or fraud. Methods of validation of theories (explanations of facts) are slightly different. A new scientific theory is also presented to a community of experts, to be independently evaluated. Their level of confidence in a theory depends on the validity of underlying assumptions and on the rigor of quantitative analysis. But even a most reliable scientific theory, called a law, is said to be falsifiable, in principle, when conflict with reproducible-on-demand data becomes undeniable (15). Such unusual conflict could trigger a scientific revolution (16).

To explain something usually means to identify causes and to construct a logically satisfying model of reality. An attempt to explain a fact, or to resolve an apparent logical conflict, usually leads to discoveries of other facts. A classical example was the discovery of planet Neptune, in 1846. A more recent and less widely known example was the discovery of a subatomic particle named neutrino. Experimental data collected in the 1920's showed that beta rays (electrons emitted in radioactive decay) had lower mean energies than expected on the basis of the theoretical $E=mc^2$ formula. Austrian theoretical physicist W. Pauli solved this "logical inconsistency" by suggesting that tiny neutral particles, later named neutrinos, were responsible for the missing energy. His hypothesis was formulated in 1933. Experiments confirming the reality of neutrinos were performed, 23 years later.

4. Unfortunate Terminology

The essence of the discovery announced by F&P was "excess heat." Their small electrolytic cell generated more thermal energy than the electric energy supplied to it. Trying to establish priority, under pressure from University of Utah administration, the scientists announced their results at a sensational press conference (March 23, 1989). They wanted to study the CF phenomenon for another year or so but were forced to prematurely announce the discovery.

The unfortunate term "cold fusion" was imposed on them (17). Why unfortunate? Because it created the unjustified impression that cold fusion is similar to the well known hot fusion, except that it takes place at much lower temperatures. This conflicted with what had already been known--the probability of nuclear fusion of two heavy hydrogen ions is negligible, except at stellar temperatures. It was a mistake to interpret experimental data before the results were recognized as independently reproducible. In fact, F&P had no evidence for the emission of energetic ^{1}H, ^{3}H and ^{3}He products, listed in their first published paper (4). The only thing they knew was that the measured excess heat could not be attributed to a known chemical reaction. Claiming that the measured excess heat was due to a nuclear process was premature. The adjective "non-chemical" does not automatically translate into "nuclear."

Suppose the discovery had not been named cold fusion; suppose it had been named "anomalous electrolysis." Such a report would not have led to a sensational press conference; it would have been made in the form of an ordinary peer review publication. Only electrochemists would have been aware of the claim; they would have tried to either confirm or refute it. The issue of "how to explain the heat" would have been addressed later, if the reported phenomenon were recognized as reproducible-on-demand. But that is not what happened. Instead of focusing on experimental data (in the area in which F&P were recognized authorities) most critics focused on the disagreements with the suggested theory. Interpretational mistakes were quickly recognized and this contributed to the skepticism toward the experimental data. Using unconfirmed data to justify the nuclear origin of excess heat, by F&P, was inconsistent with the prevailing norms of scientific methodology. A more recent case of violation of scientific norms, by a CF researcher, Andrea Rossi, is described in (14).

5. Two US Government Investigations of CF.

The significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by “vested interests” of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation.

A panel of scientists, named ERAB (Energy Research Advisory Board), and headed by John Huizenga, was formed to investigate CF in 1989. The final report (18), submitted to the DOE several months later, negatively interfered with the normal development of the field. Modest financial support for additional CF research, by the DOE, NSF, and other agencies, was practically stopped after the report was published.

It is interesting that only one of the ERAB's six conclusions referred to CF experiments; the remaining five conclusions were about anticipated practical uses of CF, and about various aspects of the suggested interpretation of results. Instead of focusing on reality of excess heat, critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion. The same observation can be made about the six ERAB recommendations. Only one of them referred to possible experimental mistakes. It is clear that the ERAB observations were based mostly on ”theoretical grounds,” not on independently performed experiments. The unfortunate governmental intervention had one serious and unprecedented consequence--editors of some scientific journals started rejecting manuscripts written by CF scientists, bypassing peer review (19).

The second DOE investigation (20) of CF was announced in March 2004, nearly 15 years after the first. A group of 18 experts was selected to review new CF claims, such as linear correlation between excess heat and generation of helium. Helium was reported to be produced at the rate of one atom for approximately 24 MeV of released thermal energy. This was consistent with the theoretical $E=mc^2$ formula for the suggested radiation-free D+D->He fusion. But the DOE experts were not asked to perform correlation experiments; they were asked to read the report submitted by five CF scientists (21), and to vote on whether or not the evidence for the claim was conclusive. Such a way of dealing with a controversy was not consistent with the scientific method.

Ideologically and politically motivated rejections of scientific claims are not new. Giordano Bruno and Galileo Galilei are well known examples. Lysenkovism--Stalin's discrimination of geneticists--is a more recent illustration. And cybernetics, in the Soviet book entitled “Short Philosophical Dictionary,” was defined as “bourgeois pseudo-science serving American imperialism.” Ludwik Kowalski accepted this kind of "truth" as a communist student in Poland (22). What can be done to make sure that similar discrimination will not be used in the US, to impose "the truth" about evolution, stem cell research, etc.?

6) Conclusion: More Questions Than Answers

Long-lasting controversies about scientific discoveries are not new. Alfred Wegener's theory of continental drift is a good illustration. Mainstream geologists rejected experimental data supporting his now-accepted theory for half a century. The CF controversy, however, seems to be different both in terms of governmental involvement and in the caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. Most leading CF researchers are PhD-level scientists. The same is true for many scientists who reject CF claims.

The long-lasting CF episode is a social situation in which the self-correcting process of scientific development was not allowed to evolve. To what extent was this due to extreme difficulties in making progress in the new area, rather than to negative effects of competition, greed, jealousy, and other "human nature" factors? Such unanswered questions (23) are worth addressing in the context of debates about science and society.

One thing is undeniable; the world is still waiting for the first reproducible-on-demand demonstration of a nuclear effect correlated with a chemical process. No progress is possible when reported experimental data cannot be reliably replicated in other laboratories. Considering potential CF benefits, and relatively low costs of research in this area, the DOE should have helped to resolve the controversy, one way or another, in a well-equipped national laboratory, during the second investigation. But it failed to do so. How can such a policy be explained? Why is CF research

allowed to stagnate without financial support? These questions also belong to debates about science and society. Will the past 23 years be recognized as the painful beginning of a new paradigm, similar to those described in (16), or will this period be known as a pseudoscientific episode, similar to those described in (24)? How can the persistence of the CF controversy be explained? Were the US government interventions necessary or not?

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414) Another Sample of Messages

March 21, 2013

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An interesting sequence of posts appeared on the CMNS list during the last three days. On March 18, 2013, I wrote: "I suggest that X1 et al. send [to the journal Naturwissenschaften] an article describing a reliable protocol for a demonstration of biological transmutation. Confirmation of their results, in another biological laboratory, would be a highly desirable step toward recognition of the main CMNS claim--a nuclear effect due to a chemical effect."

1) Responding to this X1 informed us that his article has been "accepted for publication in the very famous physical (nuclear) Elsiver journal and will be published this summer."

2) Quoting my suggestion Abd ul-Rahman Lomax (ARL) wrote: " Yes. It's an obvious possibility, an article on biological transmutation. The problem is that there has been no replication or confirmation. However, if someone does replicate, that might make for a fantastic joint paper for Naturwissenschaften. I'm not ruling out X1's submission, and, of course, it would be up to the editors of NW whather or not to accept it. But I do have concerns."

3) Then ARL added: "Dr. X1 gave me permission to share this email with the list; he wrote: "This process is possible in:

(a) any rapidly growing microbiological system (pure culture or microbiological association)

(b) at absence in a nutrient medium of required synthesized chemical element (e.g. iron) and its biochemical analogues (atoms with similar valency and
close ionic radius) and at presence of

(c) others micro- and macro-elements (that are necessary for growth) and

(d) chemical elements that are necessary for required synthesis (e.g. Mn55
and D)..... "

4) This was followed by my post. Addressing X1, I wrote:

*) First congratulations for publishing an important article in Elsiver. I hope it contains the description of a detailed reproduction protocol. If not then such a protocol is worth publishing, either by you or by a senior microbiologist working with you, in a journal read mostly by biologists. This would increase the probability that someone will actually try to replicate your results.

*) Responding to ARL you wrote: "I have one remark to the term 'biological transmutation. It is not a correct term! In our opinion, there are no reasons to consider the process of transformation of isotopes and elements during grows of biological system to be separate and different from the general physical forms of nuclear transmutation that can occur from comparable forms of transformation of isotopes through transmutation that can occur through alternatived processes, controlled by the laws of physics."

I also used the term "biological transmutation," first because other people used it, and second, because it is correct, in my opinion. It does not imply that physics is different. As we all know, physics is at the base of chemistry and chemistry is at

the base of biology. The "induced by neutrons," or "induced by alpha particle," does not mean it is not physics. The same is true for the term "induced by bacteria." or "biologically induced." Good luck,

5) Responding to another ARL's post, X1 wrote: "You wrote that 'For 'rapidly growing biological system' substitute 'any palladium metal.' For 'at absence' substitute 'loaded with deuterium.' Perhaps my concept is clear. Let's not make that mistake again.

This is incorrect and does not lead to positive results. One of the key words in the condition of biological transmutation is 'GROWING microbiological system'. Such transmutation process is possible only in 'GROWING' (dynamical, nonstationary) systems. In such systems the process of coherent suppression of Coulomb barrier action is possible (e.g. by formation of coherent correlated state). At use of dead microbiological cultures (stationary system from the physics point of view) our experiments always were unsuccessful! This is the general rule (for both biological and pure physical systems) - in stationary system LENR experiments always are unsuccessful."

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Then a different thread of messages appeared. It was triggered by an article posted by Peter Gluck, at his own website:

http://egooutpeters.blogspot.ro/2013/03/early-history-of-cold-fusion-three.html

1) Responding to this Ed Storms, the author of a well known book on Cold Fusion, wrote: " Peter has raised an important subject, ... I see no solution to the general rejection until a device having a demonstrated level of commercial power has been achieved. Only then will the claim be accepted even by people who insist the phenomenon violates accepted theory. ..."

2) Responding to this I wrote: "Yes, durable commercial success would convince skeptics that something new has been discovered and became useful. But I would not use the word "only." A reproducible-on-demand device, demonstrating transmutation (of any kind), would also convince honest skeptics."

3) Responding to this Storms wrote: "The problem with reproduction on demand is that the person doing the reproduction has to invest in a special calorimeter, learn how to use it, and spend time and money while not believing the effect is real. In addition, the level of power must exceed any imagined error. This is a lot to ask of a skeptic.

If transmutation is sought, investment must be made in the equipment required to make such studies. This cost is not trivial. People have already shown demonstrations without much effect. I believe that the ONLY proof would be a device that could be bought ready made and would be easily to setup anywhere. This device would have to make energy without stopping. Of course we can dream that science is fair and objective, but I think we have evidence that this is not true."

4) Referring to my suggestion to X1, not to mix theoretical considerations with experimental results, ARL wrote: " It's possible to present theoretical speculation in an original research report, but it must be done in such a way as to not overshadow the experimental results. For example, made up for an original paper from Pons and Fleischmann:

--- Conclusions ---

We have found anomalous energy in palladium deuteride at a density of X joules per cm^3, far in excess of what is known to be possible for chemistry, and we have exercised great care to rule out artifact. Given that heavy water results are far stronger than otherwise identifical experiments with light water, this leads to an obvious speculation of a nuclear source, but no nuclear products have been found that are commensurate with the anomalous energy, and a nuclear reaction under these conditions radically defies expectations. More research is needed to understand the source of this mysterious energy, and to confirm that there is no unidentified artifact.

5) Yes indeed, CMNR history would be different if the original CF paper ended with a similar conclusion. Thinking about this I wrote: "I would email this to Martin Fleischmann, if he were still alive, and ask him what prevented them from ending the 1989 publication in this way. Perhaps someone in contact with Stanley Pons will ask address this question to him. The reply would be an important contribution to history of science. And, if I recall correctly, Abd has nothing against being quoted outside of this list, provided it is attributed ho him."

6) ARL responded: " Sure. No problem. Perhaps I could answer. It just did not occur to them that they would be so viciously attacked. They made some mistakes, that's all.

The biggest mistake was probably a failure to fully disclose their experimental situation. The initial presentation of their results led to an assumption that one could just pour some heavy water in a jar, stick a palladium cathode in it, and load the cathode with deuterium, a well-known process, and, presto! a nuclear reacion, with neutrons, no less.

That led to a frenzy of attempted replications, it's said that something on the order of a million dollars a month was being spent -- or equivalent resources diverted from other pursuits -- in almost entirely failed replication attempts. And, then, finding no confirmation, physicists, especially, were *angry*.

I don't know that it could have been anticipated. Maybe. But maybe not. I bring this up now, not to criticize Pons and Fleischmann, whose major discovery far outshines whatever errors they may have made, but so that we can avoid repeating the errors."

7) To which X2 responded: "The original F,P & H paper was thoroughly bad all round, from a man (MF) who never wrote bad papers. This I attribute to haste, an excess of lawyers and administrators, and severe distraction on the part of Martin and Stan. The conclusions could have been better, errors could have been eliminated and more technical details could (should) clearly have been provided. This last might have bounded some of the more inept attempts at "replication".

But I submit that no amount of semantic or sociological tweaking would have changed the basic outcome. If F&P were correct in their basic claim of nuclear level heat in the electrochemical D/Pd system (as now seems clear), then a large number of the sultans of science (the high energy and particle physicists) would be seen to be wrong in their basic thinking and approach to energy. Their access to a steady stream of unquestioned funding would be threatened. This could not be allowed. A nasty, threatening, passive aggressive over-response was the natural way for this group to respond (in much the same way some individuals do)."

8) Then Storms added: " I agree with Mike. In fact, the experience at LANL was typical. Initial interest was high and money was devoted to replication. We even talked with Martin and Stan directly. Nevertheless, even though the claims were demonstrated at LANL, the right people did not have success. The claims could not be replicated to the satisfaction of the physics community. Based on many discussions with physicists, such people simply cannot understand the need for a special chemical environment to initiate the process. They, like many people, reject the idea that chemistry has any effect on a nuclear reaction. Consequently, if the nuclear effect cannot occur only by applying energy, it is impossible. When this attitude is added to the need to protect self-interest, the future rejection of CF was inevitable regardless of what F-P said.

9) There might be other messages; but I will stop reporting by showing what was just posted by Paul Biberian, who has been in contact with Stanley Pons. Paul wrote: " Dear all, I believe the answer to the lack of acceptance of CF is in the first paragraph that Stan Pons wrote for my book:

'**My name is Stanley Pons.** In March of 1989, I was one of the co-authors of a public announcement made on behalf of the University of Utah (USA) of the results of a set of scientific experiments conducted there regarding a phenomenon that was soon to be labeled “Cold Fusion”. I will not dwell here on the overwhelming maelstrom of life-changing, personally catastrophic events immediately following that announcement, other than to say that within a short period of time I found the subject had already been declared American dead, American embalmed, and American buried, and myself unofficially exiled forever by the then “president’s men”.

The last two words indicate the source of the problem. Regards."

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415) Another CF Conference is Approaching

July 1, 2013

Ludwik Kowalski

http://csam.montclair.edu/~kowalski/life/intro.html

* * *

1) The next International Cold Fusion conference, ICCF18 will begin in four weeks.

http://iccf18.research.missouri.edu/program.php

This probably explains why so many messages, both theoretical and experimental, have been posted on our private list for CMNS researchers. Responding to a post from X1, I wrote:

2) I am sorry to repeat trivia--not every heat-generating process, especially when thermal energy is generated at the rate of 1 W or less, is nuclear.

The NS part, in the name of our field, refers to unexplained nuclear processes, such as transmutation (for example, generation of helium or copper), emission of nucleons (such as energetic neutrons, protons and alpha particles), emission of gamma rays, changes in isotopic compositions, etc.

Demonstrations of nuclear signatures are extremely valuable, even when there is no evidence of excess heat. The same is true for demonstrations of unexplained excess heat, not correlated with a nuclear process. Such demonstrations are also important, both scientifically and technologically. But they do not belong to our CMNS field. Do you agree?

3) Responding to this, X2 asked: "Would you consider Deep Dirac Levels to be atomic or nuclear?" My reply was also short: "Deep Dirac Levels cannot be characterized as either only atomic or only nuclear. I was thinking about experimental signatures of CMNS."

4) Responding to X2, X3 wrote: "I get very confused by such discussions. First of all, a clear separation exists between chemical and nuclear behavior based on the huge difference in energy and the centuries of experience that show a clear difference between chemical and nuclear behavior. Outside of CF, these conflicts in understanding do not exist. In fact, the clear understand in conventional science is that CF simply cannot occur in a chemical environment, hence is a false claim.

The Deep Dirac Level is pure imagination that X2 is trying to relate to a behavior. Only the behavior of CF allows this DDL structure to be suggested because X2 can claim the normal rules of chemistry and/or nuclear physics do not apply to CF. Once the accepted rules are applied, the DDL must be rejected.

I suggest the Deep Dirac Level is neither only chemical nor only nuclear. It falls in the twilight zone occupied by the hydrino. It might apply to CF but if it does, it would also apply to many

chemical processes where its action would be easy to detect. Unfortunately, X2 [in earlier messages] has not shown this connection so the relationship to chemistry is unknown and apparently unknowable.

The experimental signatures of CMNS are clearly nuclear, as Ludwik says. Therefore, we must find a way to explain how this happens in a chemical structure without violating the basic rules accepted by conventional science. Any other approach will fail, as has been the case so far.

5) Responding to my message, X4 wrote: "Ludwik, what an odd and unexpected statement.

If one-watt heat signals without a corresponding nuclear signal are outside the scope of CMNS, then so is the seminal work of Fleischmann and Pons. Their nuclear
claim rested on the observation that the energy produced in their cells exceeded known chemistry, so the "hitherto unknown process" must be nuclear. So, I strongly disagree with your statement.

6) Instead of asking about "odd an unexpected," I wrote: Let me say the same thing in a slightly different way. Presence of unexplained excess heat may or may not be due to a nuclear process; it can be due, for example, to a totally unknown process, as just stated by X4, or to a combination of two or three known processes (perhaps optical and magnetic). What is wrong with such observations?

7) Quoting my first post, X5 wrote: "I strongly disagree with Ludwik's statement. He has missed the point entirely. The energy output is undefined. To attribute it to cold fusion is self-limiting. Limiting LENR to fusion is the basic error of the field.....[The last sentence, about being "trapped in a metaphor," is omitted; I do not understand it.]

8) Quoting X5, X3 wrote: "I think you have missed the point instead of Ludwik. We have proof that a nuclear process occurs in what appears to be ordinary material that produces energy and detected nuclear products. When this happens, it is called CMNS. Of course, other sources of novel energy might exist in Nature without a nuclear reaction being produced. However, these processes are not called cold fusion. We need to be clear about this. The energy source is defined. We only are uncertain about the process.

If you want to discuss a different source, you are free to do this, but do not confuse it with LENR."

9) Responding to X3, X2 wrote (in part): "It is legitimate for you to say you disagree with my view [an explanation based on Dirak levels] and then decline to say why or to decline to defend your position. But, your repeated statements and innuendos are simply an indication of your frustration at your models not being listened to in the way you insist....."

10) X6 wrote: Pons and Fleischmann turned out to be correct, the process was indeed nuclear. However, the energy density was circumstantial evidence of "nuclear," not direct evidence, and some other unknown but non-nuclear reaction could have been causing the apparent high-energy-density anomaly.

A fundamental error was made in asserting a paradigm shift without direct evidence.

I do understand why it happened, but we must understand this point, because we keep focusing on weak evidence, circumstantial evidence, before we have clearly conveyed the *direct evidence. *

Weak evidence of "nuclear":

1. Excess Heat. Suppose the source of the heat is hydrinos, or some other unknown non-nuclear

phenomenon. If the reaction is "unknown," what is the basis for asserting that it is *nuclear.* It is a *reasonable surmise,* but against this reasonable surmise would be a huge weight of assumption about what is possible.

2. Transmutations other than to helium, including tritium, radiation, etc. These are all at levels *far* below those needed for association with the observed excess heat. These *are* nuclear evidence, but could indicate some reaction other than what is producing the XP. Perhaps nobody ever looked so carefully before. This evidence was plagued by irreproducibility and vulnerability to various artifacts, and this is exactly what was the case with the original PF claim of neutrons. Some experiments rise to the level of strong evidence that "something nuclear is taking place,* but *what*? *It may have nothing to do with the heat-producing effect.*

Direct evidence became available with heat/helium, and that remains practically the *only* direct nuclear evidence as to the main show in the FPHE.

11) Commenting on the last sentence above I wrote: " Instruments needed to replicate the heat/helium experiments certainly exist in several US national laboratories, and elsewhere. They are probably used, by highly qualified scientists, to conduct different kinds of investigations. The DOE knows this. The cost of organizing several independent heat/helium experiments, similar to those already performed, would be a small fraction (probably much less than one percent) of the approved DOE or NSF budgets. What can be done to force the DOE or NSF to organize several independent replications?

12) X6 responded, in part:

... While I do have personal ideas about what should be done, my real proposal is to develop community process, so that what we do is a community effort, with broad consensus behind it. .....If the DoE or other interested agency is *not* convinced of the reality of cold fusion, and regarding helium as the major ash of the FP Heat Effect, after a full review of the literature on heat/helium, then it would be urgent to confirm or falsify the claim of a heat/helium correlation. That is a *central issue,* for anyone not convinced. I advise rigorous refusal to speculate on the specific mechanism, I would use the word "mystery," over and over. "Mystery" does not attack existing physics, but, of course, it allows room for something new to be found. ...

13) Two other researchers also commented. I hope the topic will be discussed at the upcoming ICCF18 conference, and that either DOE or NSF will subsequently organize attempts to replicate helium/heat experiments.

14) On Jul 2, 2013, X3, replying to another post, wrote:

Past experience shows that most of the He is NOT trapped in the cathode. The He is produced so near the surface that at least 1/2 leaves the material. The remaining He can be released by melting the metal, as you say. In the case of the sample covered with gold, even less helium might remain in the metal. However, in this case, only the gold would have to be melted if the NAE is in the gold.

Replying to this, I wrote:

If this is true then the following method can be used to determine the number of He-4 atoms per square centimeters accurately.

Suppose a Pd foil (with a layer of He-4 atoms below its surface) is exposed to a beam of alpha particles, or protons, of known energy, for example, 10 MeV, from an accelerator. Differential cross sections of Rutherford scattering, at various angles (for example 150 degrees), are easy to calculate from the well known (and experimentally confirmed) textbook formula.

Knowing the cross section one can easily calculate the "target" mean thickness (the number of He-4 atoms/cm^2) from the measured beam intensity (counts per second per steroradian), at a known angle. The method is very reliable; I have used it many times in working with various thin targets. But this was about 30-50 years ago. Our thin solid targets were always in the center of a vacuum chamber. The accuracies, typically 1 to 3 percents, were usually sufficient for our purposes.

A gas target below a metallic surface is likely to present some specific technical problems. But distinguishing ions scattered on He from ions scattered on Pd or Au (at a given angle) would not be difficult (on the basis of their kinetic energies). The kind of scattered ions, their energy, and scattering angles (better several than one), should be chosen carefully. Instruments (and experts) to perform such measurements are likely to be available in many nuclear labs, for example, in Berkeley, Brookhaven, Los Alamos, etc.

P.S. I forgot to emphasize the obvious--what is measured is the amount of He remaining, not the amont of helium created during electrolysis. Some He-4 atoms might escape from the cathode during electrolysis, others might escape during subsequent manipulations, for example, into air or into the vacuum of a scattering chamber.

Suppose that 90% of He-4 escapes. Suppose that this fact is not known, when the amount of thermal energy per atom (created during electrolysis) is calculated. In that case the calculated MeV/atom would be ten times higher than in reality.

ADDED ON AUGUST 5, 2013 (AFTER THE ICCF18 CONFERENCE)

14) X7 wrote:

Having read all the proposed theories in an attempt to find one that can guide useful research, I'm at a loss to answer a basic question. The current belief is that no theory successfully explains cold fusion. While I agree, I ask how can this situation be changed?

For a theory to be accepted, it must pass agreed upon tests. To do this, the tests must be clearly identified and given enough importance to encourage their undertaking. No time and money will be used to test a theory that contains obvious flaws, is ignored by scientific peers, or does not identify the required unambiguous tests. Reading the various papers describing theory and listening to the discussion on CMNS and Vortex, I see no effort to satisfy these requirements. I see no effort being made to actually find and test an explanation for cold fusion. Instead, everyone has a theory based on his own limited knowledge of the phenomenon using his own limited understanding of Nature. None of these proposed explanations contain testable behavior or even plausible insight. A few explanations have been given wide circulation in the literature, but no general acknowledgement of their being correct is given by other theoreticians. I see no results from successful tests being published, even by the originator of the theory. In other words, I see no serious effort being made to actually agree on an explanation - the myth of no explanation being sufficient.

The idea behind a predictive test is also not clear because this requirement is generally applied to mathematical theories. Such theories spring from the imagination, after which a prediction needs to be compared to how Nature behaves. But, theories can also be based on how Nature actually behaves using a large collection of behavior that is combined in the theory, such as the Laws of thermodynamics. Such laws can be said to be tested even before they are fully formulated. They are accepted because they are consistent with a wide range of behavior, most of which was obtained before the theory was even created.

I suggest the latter kind of theory must be created to explain cold fusion. Thousands of behaviors have been observed related to the phenomenon. We are no longer ignorant about how cold fusion behaves. In addition, modern science understands what behavior is possible and what is not within a chemical structure. Only lacking is knowledge of these facts by people attempting to provide an explanation. Is it asking too much for this knowledge to be focused on trying to determine which explanation to test and apply? Is it asking too much to stop playing mental games and get down to work trying to actually find a useful explanation? Can we agree on the reality of basic behavior when CF occurs and

on basic conflict with known laws and expected behavior when a theory is examined?

15) Responding to the above, I wrote:

The following three steps toward the first accepted CMNS theory are widely known:

a) Someone describes a protocol for a reproducible-on-demand demonstration of a nuclear effect (such as transmutation, emission of gamma rays, etc) due to a chemical process (such as electrolysis).

b) The demonstration is independently confirmed by others.

c) The reliable protocol and experimental results are then formally published in a refereed journal, for example, in our own JCMNS.

Many theoretical physicists are probably waiting for such publications. The first tentative theory will start guiding experimental research. It will become less tentative when theoretical predictions of unknown facts are confirmed.

This website contains other cold fusion items.
Click to see the list of links

416) Reproducibility

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CMNR researchers are well aware of the frustrating problem of irreproducibility of experimental results. This has been recognized in many posts at the CMNS forum to which I belong. Let me share the most recent posts on this subject; the authors are veteran CMNR researchers.

X1 wrote:

The problem has a temporal component, there are setups that were reproducible in the past, but we need systems that are reproducible NOW. Patterson is a very tragic example.

X2 responded:

I keep hearing about reproducibility, but the concept seems ill defined. Do you mean reproducible with the same material in the same device by the same experimenter, materials from different sources, attempts by different experimenters with different systems and different materials??? How long does the experimenter try till they give up?

Do you mean just blindly going through similar motions without regards to such things as (I have seen all these problems-) disregard as to handling cathode surfaces, leaving D2O open in humid environments, connecting to constant voltage instead of constant current, loading at high currents to "speed things up", disregard as to the temperatures when loading, not anodize cleaning, having electrical joints inside of cells, venting to the humid atmosphere directly instead of through bubblers.

It is nearly impossible to tell people everything they shouldn't do. It is like going into McDonald's and saying I don't want a pizza, I don't want spaghetti, I don't. .... How many things would you have to say not to do? You tell them what you do want and hope that they are "skilled in the art" and have done 20 years of their homework.

I will admit that I cannot just take a random raw piece of Pd or Ni and get identical results. But if you let me screen them, I can get better success ratios. Such selection criteria has been around since '94 - ICCF-4, and at least one protocol on cold working and cathode prep. since '04 - ASTI-5.

Perhaps the most forgiving approach seems to be co-deposition onto Au or Au plating. But I know I was saddled with trying to do a project where they wanted me to follow a specific co-dep protocol that called for co-dep on Cu. Disaster. I think they wanted failure for political purposes. But if you want a high level of success I would say start with co-dep of Pd (or even better Pd with additives) onto Au in D2O and with a Pd anode.

The next point is that some comments above seem to tout "significant levels of power". I don't really know what that means to others. I guess people have been looking at the claims of kW and MW by some. To me significant levels is just 5 or more sigma above your error bars.

So to me the arguments concerning "reproducibility" are just red herrings. Even if I get 1 out of 5 systems to give heat after carefully screening and handling materials and get 4 or 5 sigma signals, I think that is very significant. Many great technologies started with much less. (say early rockets, early transistors- I remember going to the old TI quonset

huts, where DFW airport is now, and going through barrels of out of spec transistors to make my shortwave radio).

The problem is not reproducibility; it is that people choose to not believe. There is enough data out there for those who look.

X3 responded:

Nice post X1. The apparent irreproducibility simply means we do not recognize, acknowledge, understand or control all important input parameters to the effect (or effects) we are studying. There is no magic here, our systems are causal, what comes out is conditioned and determined exactly and completely by what goes in. This speaks to the need for more science, more research, to specify completely all relevant input variables. In some ways the apparent irreproducibility is comforting since systematic errors tend to exactly reproduce. But as Peter (G) suggests, it is tiresome and an impediment to the engineering scale up desired both for practical reasons and to persuade those who choose not to look at data already generated.

If we cannot control everything that is needed (in part because we do not know what they all are), one thing is to ensure that a multiplicity of desired input configurations are present, while ensuring that none of the known undesirable conditions occur, and that a multiplicity of potentially relevant stimuli are provided. Such an experiment is your " co-dep of Pd (or even better Pd with additives) onto Au in D2O and with a Pd anode" to which I would add "stimulated by as wide a range of frequencies and modes as you can launch and accurately measure". Co-dep is guaranteed to be irreproducible - what you are relying on is that the necessary input configuration is present in sufficient quantity (somehow and somewhere) to observe an effect. I "know" (i.e. strongly suspect) that the effect we are seeking is multi-resonant so i want to provide a trigger with as many frequencies and amplitudes present as possible.

X1 might say that this is cheating. The proposed experiment is intended to achieve an artificial statistical reproducibility by acknowledging a basic lack of knowledge of what we need to construct and how we need to stimulate it. But given such statistical reproducibility we could begin to refine the deposition process to see how particular morphologies perform, and hone our inputs to see which are effective, preferably by interrogating the system and using feedback. It is not so hard. What you acutely state is correct: knowing what not to do (and then not doing it) is the key.

X4 responded:

Reproducibility means that a new sample can be made the same way and produce the same behavior. It means that this can be done at will. The electrolytic method has become increasingly reproducible, as you describe, but only in the hands of an expert. However, even using these methods, success is not assured. As you say, many ways to screw-up are available. Nevertheless, a reproducible method is one that can be described easily and works in the hands of an idiot while producing a result at a magnitude that cannot be ignored by an idiot.

The gas loading method is not reproducible except perhaps in Rossi's hands. Z1, Z2, Z3 and Z4 have provided public demonstrations, but at such low power levels that the result can be easily rejected as error or chemical effect. On the other hand, you have seen the difficulty Rossi has had in demonstrating even a kW of power. Yes, people do not want to believe the effect is real. Even certain people in the field do not want to believe basic science is involved. If we want conventional scientists to believe, we need to act like conventional scientists and use the knowledge conventional science accepts as being true and correct, or give a very good reason why expectations are violated. This is not being done.

Significant level of power means a level that can be easily measured, perhaps in excess of 1 watt. It also means a level that does not require an excessive amount of time to collect detectable He or tritium. The exact numbers are unimportant since the levels generally are not close to what is required for effective study anyway.

Let's assume you are an industrialist who whats to fund a study. He comes to your laboratory and sees one or two people working in what can be called a hobby level. You show him a device you say is hotter than it should be. So he

asks questions:

1. How do you know it is hot? How do I know you have measured correctly?
2. How hot should it be? How do you know that?
3. What is making it hot? How do you know this?
4. Can it be scaled to useful levels? Have you tried?
5. Can you provide 50 pounds of the active material for my people to study at my company?

Can you answer all of these question in a way that can be understood by a person who only has a degree in business administration?

X1 added (responding to X2)

m
I have has some success using white noise stimulation.

I do have a problem with the term reproducible. I think that people tend to use it as having heat a large ratio of times for attempts and not reaching a point where you have reproducible results= same ratio of heat vs. attempts. That is to say: is it reproducible if you do 10 sets of 10 runs each and you get 3 out of 10 excess heat events on all 10 sets of 10? That is reproducible results- always getting 33% of them giving you heat. OH well. Again, no one seems to ever define what the mean by reproducible and I have asked.

m- I often "cheat" these days with multiple frequencies (white noise or shot noise) and having samples with thousands of different sites. (I am still using the Pd or Ni in powdered C with mesopores- I still think you need a lattice). I figure that having an array of sizes and sites and using a range of frequencies mean I will have better odds of getting something each attempt. The levels may change but there is something each time. - also I tried sparking through a fluidized powder system. It increases my success/trial but not the average excess level/event. (which is reproducible??) That seems to work "most" of the times but my control and input measurements are the pits.

Right now I am using solid state (packed loaded C and gas) with fast rise time current. (fast rise time short duty cycles seem to help) - I will just say : interesting. Right now I am playing with Ni and it is looking like that adding a little D to the H is useful. Like what we found with CETI- some D2O in there with the H2O helps.

Step 1 - load cold and slow.
Step 2 - treat your chemistry with respect
Step 3 - add heat and some B field
Step 4 - establish some non-equilibrium conditions for the D or H.

Or as I said in Boston - fill and slosh.

I have basically given up on the "prove it to me" route. I have also given up on the make electricity to charge something (car, light,. ..) I am now in make a heater and heat the lab mode. It may just be COP 1:1.25, but I will try. My wife says: you are having to heat the lab with electrical power already, just go ahead and heat it with CF- have fun.

D2
Ah, but a man's reach should exceed his grasp... Or what's a heaven for? - Browning

X5 responded:

X1 wrote: "Perhaps the most forgiving approach seems to be co-deposition onto Au or Au plating. But I know I was saddled with trying to do a project where they wanted me to follow a specific co-dep protocol that called for co-dep on Cu. Disaster. I think they wanted failure for political purposes. ... " **I worked on the same co-dep project as X1 describes above. The directive I received from one of the project managers was to "test the known protocol but produce XP if you can by any reasonable means". This is a paraphrase.**

Like X1, I didn't see excess power from bare copper cathodes but found when I plated gold over the copper cathode and then plated palladium over the gold I observed excess power on every test.

I also had to modify the original Szpak-Boss protocol slightly to use a lower concentration of PdCl2 at a higher initial current.

X1 added:

I so wish they had told me "any reasonable means" instead of stating a specific protocol in my "contract".

I never could get the original recipe on Cu to work. I could see some chemical loading events but never any sustained activity.

I would recommend that any one trying to do the co-dep to try to plate onto Au or Au plated surfaces. I also could get co-dep to work on Ag plates.

I also had higher excess in co-dep devices when I used additives with the Pd and when I used Pd anodes.
For me, I got the most "reliable" excess (for co-dep systems) when I used DC pulses at around 400 Hz and 10% duty cycles(but keeping about a 1V bias to prevent de-loading, higher temp runs (but loading at 10-15C).

X6 responded:

From this POV, X2 is so correct.

What X1 sees as a 'problem', is not. It reflects that presumptuous demands might be part of the problem.

Those of us who are actually working in the field of CF/LANR, and who have given, or are giving, open demonstrations cannot just drop what we're doing (probably working on improvements) and answer the same kinds of questions and demands, repeatedly.

The fact is that an open demonstration takes more than cahones, requiring also money, time, energy, persistence, and in a larger amounts (and requiring longer times) than "Monday morning quarterbacks" may realize.

Also, if X2 had not moved on from his earlier demo, he would be stalled and not doing further R&D, and he may not have developed his system(s) and learned and discovered, even more, which he did.

So, to summarize, it is not really a 'problem', but a probably the result of the experimentalist considering the risk/benefit ratio of continuing the 'same old thing' vs. trying something(s) excitingly, and interestingly, new with 'shift' and 'drift' of engineering and design and diagnostics, etc. etc. .

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United States Patent [19J

Reifenschweiler

[11] 3,968,373

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(54] **DEVICE, IN PARTICULAR A NEUTRON GENERATOR, HAYING A DETACHABLE HIGH-VOLTAGE CONNECTION**

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[30] **Foreign Application Priority Data**

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[51] **Int. Cl.**[2] **H0SG 1/02;** G21G 4/02

[58] **Field of Search .....** 250/501, 502,419; 339/lll,112L

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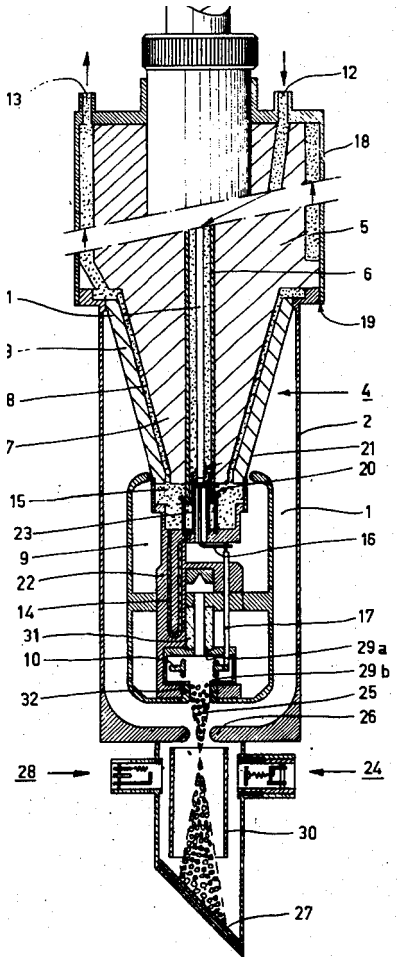
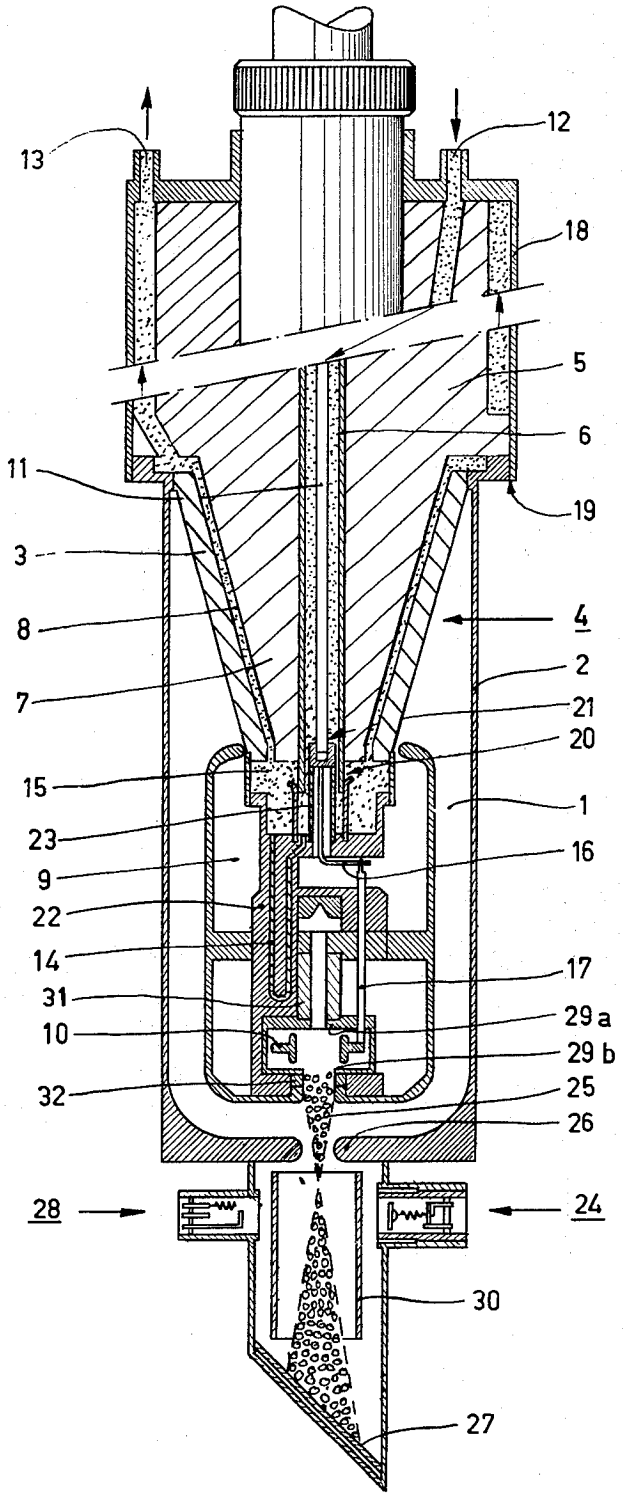
(57] **ABSTRACT**

A device, in particular a neutron generator, having a high voltage connection. A gap between a first and a second insulator of the said high voltage connection is filled with an insulating liquid, preferably oil. Said oil flows and preferably also serves for cooling parts at high voltage of the device, for.example an ion source, As a result of this the dielectric strength of the device is considerably improved.

5 Claims, 1 Drawing. Figure

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DEVICE, IN PARTICULAR A NEUTRON GENERATOR, HAVING A DETACHABLE HIGH-VOLTAGE CONNECTION

The invention relates to a device having a high voltage connection comprising a first insulator for supporting parts at high voltage of the device and a second insulator which comprises a second high voltage supply line, a gap between the said insulators being filled with an insulating liquid.

From the German Auslegeschrift I, I 65,706 a high voltage connection is known in whic insulating liquid is pressurized pneumatically in a gap between two insulators. In this high voltage connection the insulating liquid, for example oil, does not flow.

It is an object of the invention to the invention to improve the dielectric strength of such a high voltage connection. A further object of the invention is to provide a device having such a high voltage connection in which the insulating liquid is also used as a coolant.

According to the invention a device of the type mentioned in the first paragraph is characterized in that the device comprises an inlet and a.n outlet for the said insulating liquid and that the insulating liquid flows through the said gap. The insulating liquid preferably also flows through a cooling circuit in the said parts at high voltage.

By causing the insulating liquid to flow it is achieved that no so-called bridges are formed from contaminations in the insulating liquid. The flowing liquid may then alsa be. used for dissipating thermal energy to, for example, a heat exchanger. The dielectric strength of the connection is considerably improved by the invention. In devices of the type mentioned in the first paragraph producing ionizing radiation, for example neutron generators and X-ray tubes, the insulating properties of said oil under the influence of the ionizing radiation would moreover reduce rapidly when the oil is stationary in. the said gap. Since a large quantity of oil is used in a device according to the. invention of which a small part is present in the device for a short time only and is then subjected to the ionizing radiation, the insulating properties of the oil are maintained for a long . eriod of time so that the oil need be refreshed less p ft o en.

In a device according to the invention the said second insulator preferably hasa conical part which comprises an axial duct for supplying or draining the insulating liquid.

A device according to the invention may be in particular a neutron generator having an ion source at high. voltage with respect to metal parts of an envelope comprising a low pressure gas atmosphere, in which the said ion source is supported in the said envelope by a first fixed insulator which forms part ofthe said envelope and of a detachable high voltage connection, said high voltage connection furthermore. comprising a second detachable insulator which has a high voltage supply line and comprises a conical part and an axial duct and in which an insulating cooling liquid flows in a circuit which is formed by the said axial duct, a cooling circuit in the said ion source and a gap between the said fixed and detachable insulators.

The drawing shows a cross-section of the preferred .neutron generator.

The. invention will be described in greater detail.with reference to the accompanying drawing of a neutron generator according to the iμvention.

The 11eutron generator shown in the drawing comprises an envelope **1** which has a low pressure gas atmosphere and is provided with a metal part **2** and a fixed insulator 3. The fixed insulator **3** forms part of a detachable high-voltage connection **4** which furthermore comprises a detachable insulator **5** which comprises a high-voltage supply line **6.** The insulator **3** is conical and the detachable insulator **5** also has a conical part **7.** A narrow gap **8** is present between the insulator **3** and the conical part7 of the insulator **5.** An ion source**9** is supported in the envelope **1** by the fixed insulator **3.** Viathe high voltage supplyline**6** a positive voltage of approximately 250 kV with respect to the grounded metal parts **2** of the envelope **1** is supplied to the ion source 9. The ion source 9 comprises an anode **10** which cor1veys a positive anode voltage of approximately 5 kV w.ith respect to the cathodes *29a* and *29b* and the remaining metal. parts. of the ion source. Said anode voltage is supplied via a conductor I I whi.ch is present coaxially in the high voltage supply line 6,and the conductors **16** and **17.** In order to preverit breakdown, the gap **8** is filled with oil. The oil flows through the gap and is supplied via aninlet **12** and drained via an .outlet **13.** Via the space betwen the coaxial conductors **6** and **11** the oil reaches the ion source 9. In the ion source **9** the oil flows through a member of oil ducts of which one is denoted by **14** and which are provided in the metal part **22** of the ion source, and then reaches the space **15.** From the space **15** the oil flows via the conical gap **8** to the outlet **13.** The'average 'tlbw rate of the oil in the gap **8** is approximately IO m/mih. With the described construction a large dielectric strength of the detachable high voltage connection **4** is achieved and also a good cooling of the ion source **9.** A further advantage is that he qu lity f the.oil used, for which any of the known msulatmg 01ls may be chosen, reduces only slowly as a result of the neutr?n.radiation generated by the. neutron g nerato!. _This 1s ca se b the fact that a lar e quantity of 01I 1s used which 1s circu- l ted by pumpm so that onlya small part ?f !he quant1ty of 01I u edm ex o e_d to neutron, radiat!on. Asresul_t of this the rehab1hty of t_he total cqmpmen 1s considerably pr moted. Upon d1sassem_bly of the high voltage connection **4** the detachable _msulator **5** together with the conductors **6** and·**11** and with the metal parts **18**_ of the envelope. are removed. The separation between the· parts **18** and the part **2** of the envelope takes place at' 19. The separation between the high voltage supply line **6** and the ion source **9** takes place at **20.** The separation between the conductor **11** and the conductor16 takes place at 21. lt is to be noted that the tube **23** is manufactured from insulation material. The anode voltage of 5 kV is set up across the tube **23.** The space inside the tube **23** communicates with the evacuated space inside the ion source 9. From this it appears that upon disassembly of the high voltage connection **4,** the vacuum, or rather the low pressure gas atmosphere, is maintained.

Although not strictly necessary for a good understanding of the above-described invention, the operation of the neutron generator will hereinafter be roughly described.

The neutron generator shown is of the sealed off type, that to say that during use it is not connected to a vacuum pump and is evacuated only once during the

manufacture. The envelope **1** comprises a gas mixture consisting of *501:k* deuterium and 50% tritium **with** a pressure of approximately I o :[1]mm Hg. The gas mixture is supplied and the pressure thereof is maintained at the correct value by a pressure control 24. The pressure control comprises a large quantity of the gas mixture absorbed in a finely" divided titanium powder, and can supply this by heating.

The mixture of deuterium and tritium is ionized in the ion source **9** by means of electrons which move in the electric field between the ariode **10** and the cathodes ***29a*** and ***29b*** and in the axial magnetic field of the permanent magnets **31** and **32.** So the ion source is of the known "Penning'" type. A beam **25** consisting" of positive deuterium ions and tritium ions having a current intensity of approximately 15mA is extracted from the ion source by the acceleration electrode **26.** The ion source is at a positive voltage of 250 kVoit_relative to the acceleration elecfrode **26** which forms part of the grounded part **2** of the envelope. The ion beam formed passes the screen electrode **30** and impinges upon the target **27.** The target27 consists of a base plate of a material having a small coefficient of absorption and diffusion for deuterium and tritium (for example copper) and a reaction layer.of a material having a large coefficient of absorption for deuterium and tritium.The reaction layer consists of a 5 *μ.m* thick layer of titanium. The target **27** is provicied in the neutron generator without being saturated with deuterium and tritium and in the first hours of life of the generator absorbs deuterium and tritium from the ion beam impingi,ng upon. the targeL **27.** Th'e absorbed deuterium . and. triti m cannot diffuse away to any considerable extent to places in the target which are situated deeper than the t_hickness of the reacti.on layer because. the bas_e plate ha_s a small coefficient of absorption and diffusion for deuterium and tritium. The result is that the reaction layer is satu ated with deuterium and tritium to an ever increasing extent so that the .neutron yeild begins. The neutron generation results in particular from the reaction between deuterium and.tritium. The collision with an energy of 250keV betwe n a deuterium nucleus and .a tritium nucleus provides a neutron having an energy of 14 MeV and an a-particle having an energy of 3.6 MeV. It is to be noted that neutrons are also formed to a small extent from the reaction between two deuterium nuclei. Said neutrons have a much smaller energy. The. neutrons having an energy of 14. MeV form the effective yield of approximately I 0^{12} neutrons per second of the generator. Said neutron yield is achieved after the generator has been in operation for approximately **1** hour when the saturation of the reactio.n layer with deuterium ,and tritium has reached its equilibrium. condition.

The screening electrode **30** has a, negative potential of a few·kV relative to the target **727** so as to prevent secondary electrons which are formed on the target **27** from being accelerated towards the ion source 9. The neutron generator furthermore comprises an ionisation manometer for checking the gas pressure.

The neutron generator may be used inter alia for scientific purposes as activation analysis and due to its large neutron yield and compact construction it is also particularly suitable for medical applications such as cancer therapy, "total body" activation analysis and measurements of. the blood .circulation.

I claim:

1. Apparatus for making high voltage connection to and cooling an ion source, .comprising an ion source having a cooling duct, a first insulator for supporting said ion source, and a detachable high voltage connection.comprising a second insulator adapted to fit in spaced relation to said first insulator with a gap therebetween in communication with one end of said cooling duct of said ion source,said second i_nsulator having a duct connecting with the other end of said cooling duct to form a conduit for continuously carrying cooling and insulatiµg fluid through said duct of said second insulator, said cooling duct of said ion source and said gap between said first and second insulators, and a high voltage supply conductor carried by said second insulator for connection across said conduit to said ion source.

2. Apparatus as defined in claim **1** wherein said high voltage supply conductor is coaxial with said duct of said seqmd insulator at least in the vicirii_ty of connection thereof with said ion source.

3. Apparatus as defined in claim **2** wherein said second insulator is conical in the vicinity of said ion source arid said duct of said second insulator is coaxial therewith at least in the vicinity of said ion source,

4. Apparatus as defined in claim 3 w_herein said apparatus forms part of a neutron generator.

5. Apparatus comprising an ion source at high voltage· with respect to metal parts ofan envelope, characterized in tnat said ion source is supported in said envelope by a first fixed insulawr which forms part of said envelope and of a·detachable high voltage connection, said high voltage connection furthermore comprising a second detachable insulator which has a high voltage supply line and comprises a conical part and an axial duct;'and in which an insulating cooling liquid flows in a circuit which is formed by the said axial duct, a cooling circuit in said ion source and a gap hetween said fixed and detachable insulators.

* * * * *